ISAACI NEWTONI

OPERA

QUÆ EXSTANT OMNIA.

COMMENTARIIS ILLUSTRABAT

SAMUEL HORSLEY, LL.D. R.S.S.

REVERENDO ADMODUM IN CHRISTO PATRI

ROBERTO EPISCOPO LONDINENSI A SACRIS.

TOM. IV.

LONDINI:

EXCUDEBAT JOANNES NICHOLS.

M DCC LXXXII.

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5

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OPTICS:

OR, A

TREATISE

OF THE

REFLECTIONS, REFRACTIONS,

INFLECTIONS AND COLOURS

OF

LIGHT.

T R I G H T'

ON THE

REFLECTIONS AND COLOURS

OF THE

REFRACTIONS REFRACTIONS

AND

OPTICS.

ADVERTISEMENT I.

PART of the enſuing Diſcourſe about Light was written at the deſire of ſome Gentlemen of the Royal Society, *in the year* 1675, *and then ſent to their Secretary, and read at their Meetings, and the reſt was added about twelve Years after to complete the Theory; except the Third Book, and the laſt Propoſition of the Second, which were ſince put together out of ſcattered Papers. To avoid being engaged in Diſputes about theſe matters, I have hitherto delayed the printing, and ſhould ſtill have delayed it, had not the importunity of Friends prevailed upon me. If any other papers writ on this ſubject are got out of my hands, they are imperfect; and were perhaps written before I had tried all the Experiments here ſet down, and fully ſatisfied myſelf about the Laws of Refractions and Compoſition of Colours. I have here publiſhed what I think proper to come abroad, wiſhing that it may not be tranſlated into another language without my conſent.*

The Crowns of Colours, which ſometimes appear about the Sun and Moon, I have endeavoured to give an account of; but for want of ſufficient Obſervations leave that matter to be farther examined. The ſubject of the Third Book I have alſo left imperfect, not having tried all the Experiments which I intended when I was about theſe matters, nor repeated ſome of thoſe which I did try, until I had ſatisfied myſelf about all their circumſtances. To communicate what I have tried, and leave the reſt to others for farther enquiry, is all my deſign in publiſhing theſe papers.

In a Letter written to Mr. Leibnitz *in the year* 1679, *and publiſhed by Dr.* Wallis, *I mentioned a method by which I had found ſome general Theorems about ſquaring Curvilinear Figures, or comparing them with the Conic Sections, or other the ſimpleſt Figures with which they may be compared. And ſome years ago I lent out a Manuſcript containing ſuch Theorems; and having ſince met with ſome things copied out of it, I have on this occaſion made it public, prefixing to it an* Introduction, *and ſubjoining a* Scholium *concerning that method. And I have joined with it another ſmall Tract concerning the Curvilinear Figures of the Second Kind, which was alſo written many years ago, and made known to ſome friends, who have ſolicited the making it publick.* *I. N.*

April 1, 1704.

ADVERTISEMENT II.

IN this Second Edition of these Opticks I have omitted the Mathematical Tracts published at the end of the former Edition, as not belonging to the subject. And at the end of the Third Book I have added some Questions. And to shew that I do not take Gravity for an Essential Property of Bodies, I have added one Question concerning its Cause, chusing to propose it by way of a Question, because I am not yet satisfied about it for want of Experiments.

I. N.

July 16, 1717.

THE

THE

FIRST BOOK

OF

OPTICS.

PART I.

MY deſign in this Book is not to explain the Properties of Light by Hypotheſes, but to propoſe and prove them by reaſon and experiments: in order to which I ſhall premiſe the following Definitions and Axioms.

DEFINITIONS.

DEFIN. I.

By the Rays of Light I underſtand its leaſt parts, and thoſe as well ſucceſſive in the ſame lines, as contemporary in ſeveral lines. For it is manifeſt that Light conſiſts of parts both Succeſſive and Contemporary; becauſe in the ſame place you may ſtop that which comes one moment, and let paſs that which comes preſently after; and in the ſame time you may ſtop it in any one place,

Definitions.

place, and let it paſs in any other. For that part of Light which is ſtopt cannot be the ſame with that which is let paſs. The leaſt Light, or part of Light, which may be ſtopt alone without the reſt of the Light, or propagated alone, or do or ſuffer any thing alone, which the reſt of the Light doth not or ſuffers not, I call a Ray of Light.

DEFIN. II.

Refrangibility of the Rays of Light, is their diſpoſition to be refracted or turned out of their way, in paſſing out of one tranſparent Body, or Medium, into another. And a greater or leſs Refrangibility of Rays, is their diſpoſition to be turned more or leſs out of their way in like incidences on the ſame Medium. Mathematicians uſually conſider the Rays of Light to be lines reaching from the luminous body to the body illuminated; and the refraction of thoſe Rays to be the bending or breaking of thoſe lines in their paſſing out of one Medium into another. And thus may Rays and Refractions be conſidered, if Light be propagated in an inſtant. But, by an argument taken from the Æquations of the times of the Eclipſes of *Jupiter's Satellites*, it ſeems that Light is propagated in time, ſpending in its paſſage from the Sun to us about ſeven minutes of time: and therefore I have choſen to define Rays and Refractions in ſuch general terms as may agree to Light in both caſes.

DEFIN. III.

Reflexibility of Rays, is their diſpoſition to be reflected or turned back into the ſame Medium from any other Medium, upon whoſe ſurface they fall. And Rays are more or leſs reflexible, which are turned back more or leſs eaſily. As if Light paſs out of Glaſs into Air, and, by being inclined more and more to the common ſurface of the Glaſs and Air, begins at length to be totally reflected by that ſurface; thoſe ſorts of Rays which, at like incidences, are reflected moſt copiouſly, or by inclining the Rays begin ſooneſt to be totally reflected, are moſt reflexible.

DEFIN. IV.

DEFINITIONS.

The Angle of Incidence is that Angle, which the line deſcribed by the incident Ray contains with the Perpendicular to the reflecting or refracting ſurface at the point of Incidence.

DEFIN. V.

The Angle of Reflexion or Refraction, is the Angle which the line deſcribed by the reflected or refracted Ray containeth with the Perpendicular to the reflecting or refracting ſurface at the point of Incidence.

DEFIN. VI.

The Sines of Incidence, Reflexion, and Refraction, are the Sines of the Angles of Incidence, Reflexion, and Refraction.

DEFIN. VII.

The Light whoſe Rays are all alike Refrangible, I call Simple, Homogeneal and Similar; and that whoſe Rays are ſome more Refrangible than others, I call Compound, Heterogeneal and Diſſimilar. The former Light I call Homogeneal, not becauſe I would affirm it ſo in all reſpects; but becauſe the Rays, which agree in Refrangibility, agree at leaſt in all thoſe their other properties, which I conſider in the following Diſcourſe.

DEFIN. VIII.

The Colours of Homogeneal Lights, I call Primary, Homogeneal and Simple; and thoſe of Heterogeneal Lights, Heterogeneal and Compound. For theſe are always compounded of the colours of Homogeneal Lights; as wtll appear in the following Diſcourſe.

AXIOMS

Axioms.

AXIOMS.

AX. I.

The Angles of Reflexion, and Refraction, lie in one and the ſame Plane with the Angle of Incidence.

AX. II.

The Angle of Reflexion is equal to the Angle of Incidence.

AX. III.

If the refracted Ray be turned directly back to the Point of Incidence, it ſhall be refracted into the Line before deſcribed by the incident Ray.

AX. IV.

Refraction out of the rarer Medium into the denſer, is made towards the Perpendicular; that is, ſo that the Angle of Refraction be leſs than the Angle of Incidence.

AX. V.

The Sine of Incidence is either accurately or very nearly in a given ratio to the Sine of Refraction.

Whence if that proportion be known in any one inclination of the incident Ray, it is known in all the inclinations, and thereby the Refraction, in all caſes of incidence on the ſame refracting body, may be determined. Thus if the Refraction be made out of Air into Water, the ſine of Incidence of the Red Light is to the ſine of its Refraction as 4 to 3. If out of Air into Glaſs, the Sines are as 17 to 11. In Light of other Colours the ſines have other proportions: but the difference is ſo little that it need ſeldom be conſidered.

Suppoſe, therefore, that RS [in *fig.* 1.] repreſents the Surface of ſtagnating Water, and that C is the point of Incidence, in which any Ray, coming in the Air from A in the Line AC, is reflected or refracted; and I would know whither this Ray ſhall go after Reflexion or Refraction: I erect upon the ſurface of the Water, from the point of Incidence, the Perpendicular CP, and produce it downwards to Q, and conclude by the firſt Axi-

om, that the Ray after Reflexion and Refraction, ſhall be found ſomewhere in the Plane of the Angle of Incidence ACP produced. I let fall therefore upon the Perpendicular, CP, the ſine of Incidence AD; and if the reflected Ray be deſired, I produce AD to B, ſo that DB be equal to AD, and draw CB. For this Line CB ſhall be the reflected Ray; the angle of Reflexion BCP and its ſine BD being equal to the angle and ſine of Incidence, as they ought to be by the ſecond Axiom. But if the refracted Ray be deſired, I produce AD to H, ſo that DH may be to AD as the ſine of Refraction to the ſine of Incidence, that is (if the Light be red) as 3 to 4; and about the Center C, and in the Plane ACP, with the Radius CA deſcribing a Circle ABE, I draw parallel to the Perpendicular CPQ, the Line HE cutting the Circumference in E, and joining CE, this line CE ſhall be the line of the Refracted Ray. For if EF be let fall perpendicularly on the line PQ, this line EF ſhall be the ſine of Refraction of the Ray CE, the angle of Refraction being ECQ; and this ſine EF is equal to DH, and conſequently in proportion to the ſine of Incidence AD as 3 to 4. Axioms.

In like manner, if there be a Priſm of Glaſs (that is, a Glaſs bounded with two equal and parallel triangular ends, and three plain and well poliſhed ſides, which meet in three parallel lines running from the three angles of one end to the three angles of the other end) and if the Refraction of the Light in paſſing croſs this Priſm be deſired: let ACB [in *fig.* 2.] repreſent a plane cutting this Priſm tranſverſly to its three parallel lines, or edges, there where the Light paſſeth through it; and let DE be the Ray incident upon the firſt ſide of the priſm AC, where the Light goes into the glaſs; and by putting the proportion of the ſine of Incidence to the ſine of Refraction as 17 to 11, find EF the firſt refracted Ray. Then taking this Ray for the incident Ray upon the ſecond ſide of the Glaſs, BC, where the Light goes out, find the next refracted Ray FG, by putting the proportion of the ſine of Incidence to the ſine of Refraction as 11 to 17. For if the ſine of Incidence out of Air into Glaſs be to the ſine of Refraction as 17 to 11, the ſine of Incidence out of Glaſs into Air muſt on the contrary be to the ſine of Refraction as 11 to 17, by the third Axiom.

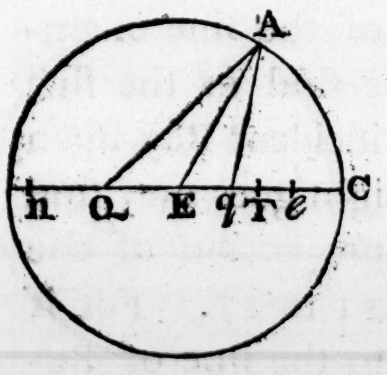
Axioms.

Much after the ſame manner, if ACBD [in *fig.* 3.] repreſent a Glaſs ſpherically convex on both ſides (uſually called a *Lens*, ſuch as is a Burning-glaſs, or Spectacle-glaſs, or an Object-glaſs of a Teleſcope) and it be required to know how Light falling upon it from any lucid point Q ſhall be refracted; let QM repreſent a Ray falling upon any point M of its firſt ſpherical ſurface ACB, and by erecting a perpendicular to the Glaſs at the point M, find the firſt refracted Ray, MN, by the proportion of the ſines 17 to 11. Let that Ray in going out of the Glaſs be incident upon N, and then find the ſecond refracted Ray, N*q*, by the proportion of the ſines 11 to 17. And after the ſame manner may the Refraction be found, when the Lens is convex on one ſide and plane or concave on the other, or concave on both ſides.

AX. VI.

Homogeneal Rays which flow from ſeveral points of any Object, and fall perpendicularly, or almoſt perpendicularly, on any reflecting or refracting plane or ſpherical Surface, ſhall afterwards diverge from ſo many other points, or be parallel to ſo many other lines, or converge to ſo many other points, either accurately or without any ſenſible error. And the ſame thing will happen, if the Rays be reflected or refracted ſucceſſively by two or three or more plane or ſpherical Surfaces.

The point from which Rays diverge, or to which they converge, may be called their *Focus*. And the Focus of the incident Rays being given, that of the reflected or refracted ones



(a) Imagine that rays, emitted from the point Q, fall upon the ſpherical ſurface at A, and are reflected to *q*, and join EA. Then the angles EAQ, EA*q* will be equal; and of conſequence QE will be to E*q* as QA to A*q*. And this analogy will ſubſiſt, wherever in the reflecting ſurface the point A be placed. Let A therefore approach to C; and in the evaneſcence of the arc CA, QA becomes ultimately equal to QC, and *q*A to *q*C. Therefore QC is ultimately to *q*C as QE as *q*E. In CQ produced take Q*n* equal to QE, and from CE cut off C*e* equal to E*q*. Then ſince QC is to *q*C as QE to *q*E; the ſum of QC and QE, that is, C*n*, will be to the ſum of *q*C and *q*E, that is, to EC, as QC to *q*C. (Elem. v. 12.) Alſo the difference of QC and QE, that is EC, will be to the difference of *q*C and *q*E, that is to *eq*, as QC to *q*C. Therefore C*n* is to EC as EC to *eq* (Elem. v. 11.) and of conſequence $\frac{1}{2}$ C*n* to $\frac{1}{2}$ EC or ET, as $\frac{1}{2}$ EC or ET to $\frac{1}{2}$ *eq*. But C*n* is equal to twice ET together with twice EQ. Therefore TQ is $\frac{1}{2}$ C*n*, and T*q* is $\frac{1}{2}$ *eq*. Therefore TQ, TE, T*q*, ultimately when A coincides with C, are continually proportional. Hence the truth of the Newtonian conſtruction is manifeſt.

may

may be found by finding the Refraction of any two Rays, as Axiom. above; or more readily thus.

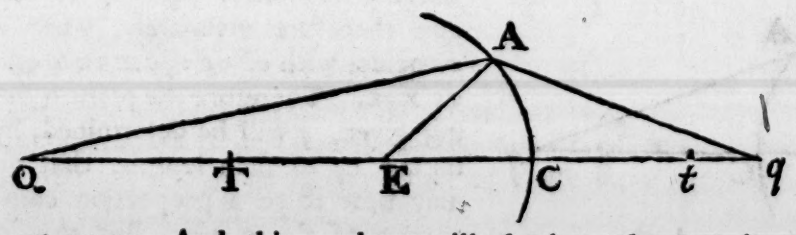
Caſ. 1. Let ACB [in *fig.* 4.] be a reflecting or refracting plane, and Q the Focus of the incident Rays, and Q*q*C a perpendicular to the plane. And if this perpendicular be produced to *q*, ſo that *q*C be equal to QC, the point *q*, ſhall be the Focus of the reflected Rays. Or if *q*C be taken on the ſame ſide of the Plane with QC, and in proportion to QC as the ſine of Incidence to the ſine of Refraction, the point *q* ſhall be the Focus of the refracted Rays.

Caſ. 2. Let ACB [in *fig.* 5.] be the reflecting ſurface of any Sphere whoſe center is E. Biſect any radius thereof (ſuppoſe EC) in T, and if in that radius, on the ſame ſide the point T, you take the points Q and *q*, ſo that TQ, TE, and T*q*, be continual proportionals, and the point Q be the Focus of the incident Rays, the point *q* ſhall be the Focus of the reflected ones (a).

Caſ. 3. Let ACB [in *fig.* 6.] be the refracting ſurface of any Sphere whoſe center is E. In any radius thereof, EC, produced both ways take ET and c*t* equal to one another, and ſeverally in ſuch proportion to that radius, as the leſſer of the ſines of Incidence and Refraction hath to the difference of thoſe ſines. And then if in the ſame line you find any two points Q and *q*, ſo that TQ be to ET as E*t* to *tq*, taking *tq* the contrary way from *t* which TQ lieth from T, and if the point Q be the Focus of any incident Rays, the point *q* ſhall be the Focus of the refracted ones (b).

And

(b) IMAGINE rays, emitted from Q, to fall upon the ſpherical ſurface at A, and to be refracted to *q*, and join EA. Then is QAE the angle of Incidence, and *q*AE the angle of Refraction. Let I denote the ſine of the former, R that of the latter. Now in the triangle QAE, QA is to QE as the ſine of E to I. By permutation QA is to the ſine of E as QE to I, or as the rectangle QE × *q*E to the rectangle I × *q*E. Again in the triangle *q*AE, the ſine of E is to R as *q*A to *q*E. By permutation the ſine of E is to *q*A as R to *q*E, or as the rectangle R × QE to the rectangle *q*E × QE. But ſince QA is to the ſine of E as QE × *q*E to I × *q*E; and the ſine of E to *q*A as R × QE to QE × *q*E. Therefore (ex æquo perturbatè) QA is to *q*A as R × QE to I × *q*E. But becauſe TE is to EC as I to R − I, therefore, inverting and compounding, TC is to TE as R to I. Therefore R × QE : I × *q*E = TC × QE : TE × *q*E; and QA : *q*A = TC × QE : TE × *q*E. And this analogy will obtain, wherever in the refracting ſurface the point A be placed. Let A therefore approach to C; and ſince in the evaneſcence of the arc CA, QA and *q*A become

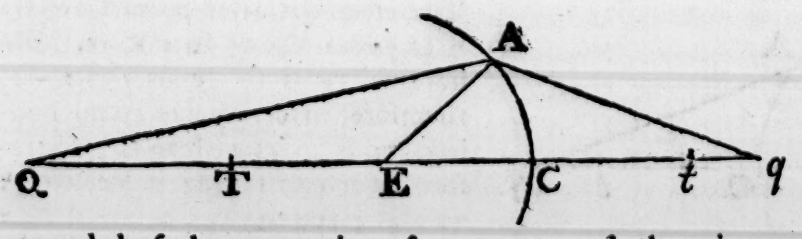


Axioms. And by the ſame means the Focus of the Rays after two or more Reflexions or Refractions may be found.

Caſ. 4. Let ACBD [in *fig.* 7.] be any refracting Lens, ſpherically convex, or concave, or plane on either ſide; and let CD be its axis (that is the line which cuts both its ſurfaces perpendicularly, and paſſes through the centers of the ſpheres) and in this Axis produced let F and *f* be the Foci of the refracted Rays found as above, when the incident Rays on both ſides the Lens are parallel to the ſame Axis; and upon the diameter F*f*, biſected in E, deſcribe a circle. Suppoſe now that any point Q be the Focus of any incident Rays. Draw QE cutting the ſaid circle in T and *t*, and therein take *tq* in ſuch proportion to *t*E, as *t*E or TE hath to TQ. Let *tq* lie the contrary way from *t* which TQ doth from T, and *q* ſhall be the Focus of the refracted Rays, without any ſenſible error; provided the point Q be not ſo remote from the Axis, nor the Lens ſo broad, as to make any of the Rays fall too obliquely on the refracting ſurfaces.

And by the like operations may the reflecting or refracting ſurfaces be found, when the two Foci are given; and thereby a Lens be formed, which ſhall make the Rays flow towards or from what place you pleaſe (c).

So then the meaning of this Axiom is, that if Rays fall upon any plane or ſpherical ſurface or lens, and before their incidence flow from or towards any point Q, they ſhall after Reflexion or Refraction flow from or towards the point *q* found by the foregoing rules. And if the incident rays flow from or towards ſeveral points Q, the reflected or refracted rays ſhall flow from or towards ſo many other points *q* found by the ſame rules. Whether the reflected or refracted rays flow from or towards the point *q* is eaſily known by the ſituation of that point. For if that point



become ultimately equal to Qc and *q*c; therefore ultimately, when A coincides with c, Qc : *q*c = TC × QE : TE × *q*E. Therefore the point Q being given, *q* will be determined, by taking c*q* of ſuch length, that Qc may bear to *q*c a proportion compounded of the proportion of QE to *q*E, and the given proportion of TC to TE. But this is effected by the Newtonian Conſtruction. For TQ being to ET, or its equal c*t*, as E*t*, or its equal

point be on the ſame ſide of the reflecting or refracting ſurface or lens with the point Q, and the incident rays flow from the point Q, the reflected flow towards the point *q*, and the refracted from it; and if the incident rays flow towards Q, the reflected flow from *q*, and the refracted towards it. And the contrary happens when *q* is on the other ſide of that ſurface. AXIOMS.

AX. VII.

Wherever the rays, which come from all the points of any Object, meet again in ſo many points, after they have been made to converge by reflexion or refraction, there they will make a Picture of the object upon any white body on which they fall.

So if PR [in *fig.* 3.] repreſent any Object without doors, and AB be a lens placed at a hole in the window-ſhut of a dark chamber, whereby the rays that come from any point Q of that object are made to converge and meet again in the point *q*; and if a ſheet of white paper be held at *q* for the light there to fall upon it: the Picture of that Object, PR, will appear upon the paper in its proper ſhape and colours. For as the light which comes from the point Q goes to the point *q*, ſo the light which comes from other points, P and R, of the object, will go to ſo many other correſpondent points *p* and *r* (as is manifeſt by the ſixth Axiom); ſo that every point of the Object ſhall illuminate a correſpondent point of the Picture; and thereby make a Picture like the Object in ſhape and colour, this only excepted, that the picture ſhall be inverted. And this is the reaſon of that vulgar experiment of caſting the ſpecies of objects from abroad upon a wall or ſheet of white paper in a dark room.

In a like manner, when a man views any object PQR [in *fig.* 8.]

equal CT to *tq*; by conſequence TQ and CT together, that is QC, will bear to C*t* and *tq* together, that is to *q*C, the proportion of TC to *tq*. (Elem. v. 12.) But TC bears to *tq* the proportion compounded of the proportions of TC to TE, and TE to *tq*. And the proportion of TE to *tq* is the ſame which QE bears to *q*E. For ſince QT is to TE as E*t* to *tq*, by compoſition QE : TE = E*q* : *qt*. And by permutation QE : *q*E = TE : *tq*. Therefore the proportion of TC to *tq*, being compounded of the proportions of TC to TE and TE to *tq*, is compounded of the proportions of TC to TE, and QE to *q*E. Therefore the proportion of QC to *q*C, being the ſame with that of TC to *tq*, as hath been ſhewn, is compounded of theſe ſame proportions. Whence the truth of Newton's Conſtruction is manifeſt.

(*) Vide Lect. Opt. Part I. Sect. IV. Prop. XXIX, XXX. XXXII, XXXIII.

the

Cause of Vision.

the light, which comes from the ſeveral points of the object, is ſo refracted by the tranſparent ſkins and humours of the Eye, (that is by the outward coat EFG called the *Tunica Cornea*, and by the cryſtalline humour AB which is beyond the pupil *mk*) as to converge and meet again at ſo many points in the bottom of the Eye, and there to paint the Picture of the Object upon that ſkin (called the *Tunica Retina*) with which the bottom of the eye is covered. For Anatomiſts, when they have taken off from the bottom of the eye that outward and moſt thick coat called the *Dura Mater*, can then ſee, through the thinner coats, the pictures of objects lively painted thereon. And theſe pictures propagated by motion along the fibres of the Optick Nerves into the brain, are the cauſe of Viſion. For accordingly as theſe pictures are perfect or imperfect, the object is ſeen perfecty or imperfectly. If the eye be tinged with any colour (as in the diſeaſe of the *Jaundiſe*) ſo as to tinge the pictures in the bottom of the eye with that colour, then all objects appear tinged with the ſame colour. If the humours of the eye by old age decay, ſo as by ſhrinking to make the *cornea* and coat of the *cryſtalline humour* grow flatter than before, the light will not be refracted enough; and, for want of a ſufficient refraction, will not converge to the bottom of the eye, but to ſome place beyond it; and by conſequence paint in the bottom of the eye a confuſed picture, and according to the indiſtinctneſs of this Picture the Object will appear confuſed. This is the reaſon of the decay of ſight in old men, and ſhews why their ſight is mended by ſpectacles. For thoſe convex glaſſes ſupply the defect of plumpneſs in the eye, and, by encreaſing the refraction, make the rays converge ſooner, ſo as to convene diſtinctly at the bottom of the eye, if the Glaſs have a due degree of convexity. And the contrary happens in ſhort-ſighted men, whoſe eyes are too plump. For the refraction being now too great, the rays converge and convene in the eyes before they come at the bottom; and therefore the picture made in the bottom, and the viſion cauſed thereby, will not be diſtinct, unleſs the object be brought ſo near the eye, as that the place where the converging rays convene may be removed to the bottom; or that the plumpneſs of the eye be taken off, and the

refractions

refractions diminiſhed by a concave glaſs of a due degree of concavity; or, laſtly, that by age the eye grow flatter till it come to a due figure: for ſhort-ſighted men ſee remote objects beſt in old age, and therefore they are accounted to have the moſt laſting eyes. AXIOMS.

AXIOM VIII.

An Object ſeen by Reflexion or Refraction, appears in that place, from whence the rays, after their laſt reflexion or refraction, diverge in falling on the Spectator's Eye.

If the Object A [in *fig.* 9.] be ſeen by Reflexion of a Looking-glaſs *mn*, it ſhall appear, not in its proper place A, but behind the glaſs at *a*; from whence any rays AB, AC, AD, which flow from one and the ſame point of the object, do, after their reflexion made in the points B, C, D, diverge in going from the glaſs to E, F, G, where they are incident on the ſpectator's eyes. For theſe rays do make the ſame picture in the bottom of the eyes, as if they had come from the object really placed at *a*, without the interpoſition of the looking-glaſs; and all Viſion is made according to the place and ſhape of that picture.

In like manner the Object D [in *fig.* 2.] ſeen through a Priſm, appears not in its proper place D, but is thence tranſlated to ſome other place *d*, ſituated in the laſt refracted ray, FG, drawn backward from F to *d*.

And ſo the Object Q [in *fig.* 10.] ſeen through the Lens AB, appears at the place *q*, from whence the rays diverge in paſſing from the lens to the eye. Now it is to be noted, that the Image of the object, at *q*, is ſo much bigger or leſſer than the Object itſelf at Q, as the diſtance of the image at *q*, from the lens AB, is bigger or leſs than the diſtance of the object at Q from the ſame lens. And if the Object be ſeen through two or more ſuch convex or concave-glaſſes, every Glaſs ſhall make a new Image, and the Object ſhall appear in the place and of the bigneſs of the laſt Image. Which conſideration unfolds the Theory of Microſcopes and Teleſcopes. For that Theory conſiſts in almoſt nothing elſe, than the deſcribing ſuch glaſſes, as ſhall make the laſt Image of any

Axioms.

any object as diſtinct and large and luminous, as it can conveniently be made.

I have now given in Axioms and their Explications the ſum of what hath hitherto been treated of in Opticks. For what hath been generally agreed on I content myſelf to aſſume under the notion of Principles, in order to what I have farther to write. And this may ſuffice for an introduction to readers of quick wit and good underſtanding not yet verſed in Opticks: although thoſe who are already acquainted with this ſcience, and have handled Glaſſes, will more readily apprehend what followeth.

PROPO-

PROPOSITIONS.

PROP. I. THEOR. I.

Lights which differ in Colour, differ alſo in degrees of Refrangibility.

The Proof by Experiments.

Lights differing in Colour differ in Refrangibility.

Exper. 1. I took a black oblong ſtiff paper terminated by parallel ſides, and, with a perpendicular right line drawn croſs from one ſide to the other, diſtinguiſhed it into two equal parts. One of theſe parts I painted with a Red colour, and the other with a Blue. The paper was very black, and the colours intenſe and thickly laid on, that the phænomenon might be more conſpicuous. This paper I viewed through a Priſm of ſolid Glaſs, whoſe two ſides, through which the light paſſed to the eye, were plane and well poliſhed, and contained an angle of about ſixty degrees: which angle I call the Refracting Angle of the Priſm. And whilſt I viewed it, I held it and the priſm before a window, in ſuch manner that the ſides of the paper were parallel to the priſm, and both thoſe ſides and the priſm were parallel to the horizon, and the croſs line was alſo parallel to it: and that the light, which fell from the Window upon the paper, made an angle with the paper, equal to that angle which was made with the ſame paper by the light reflected from it to the Eye. Beyond the priſm was the wall of the chamber, under the window, covered over with black cloth; and the cloth was involved in darkneſs, that no light might be reflected from thence, which in paſſing by the edges of the paper to the eye, might mingle itſelf with the light of the paper, and obſcure the phænomenon thereof. Theſe things being thus ordered, I found that if the Refracting Angle of the Priſm be turned upwards, ſo that the

Lights differing in Colour

paper may ſeem to be lifted upwards by the refraction, its Blue half will be lifted higher by the refraction than its Red half. But if the Refracting Angle of the Priſm be turned downward, ſo that the paper may ſeem to be carried lower by the refraction, its Blue half will be carried ſomething lower thereby than its Red half. Wherefore, in both caſes, the Light, which comes from the Blue half of the paper through the priſm to the eye, does, in like circumſtances, ſuffer a greater refraction than the Light which comes from the Red half, and by conſequence is more Refrangible.

Illuſtration. In the eleventh figure, MN repreſents the window, and DE the paper terminated with parallel ſides, DJ and HE, and by the tranſverſe line FG diſtinguiſhed into two halfs: the one, DG, of an intenſely Blue colour, the other, FE, of an intenſely Red. And BAC*cab* repreſents the priſm, whoſe Refracing Planes, AB*ba* and AC*ca*, meet in the edge of the Refracting Angle A*a*. This edge A*a* being upward, is parallel both to the horizon and to the parallel edges of the paper, DJ and HE; and the tranſverſe line FG is perpendicular to the plane of the window: and *de* repreſents the Image of the paper ſeen by refraction upwards, in ſuch manner that the Blue half, DG, is carried higher to *dg* than the Red half FE is to *fe*, and therefore ſuffers a greater refraction. If the edge of the Refracting Angle be turned downward, the Image of the paper will be refracted downward, ſuppoſe to $\delta\varepsilon$; and the Blue half will be refracted lower to $\delta\gamma$ than the Red half is to $\phi\varepsilon$.

Exper. 2. About the aforeſaid paper, whoſe two halfs were painted over with Red and Blue, and which was ſtiff like thin paſte-board, I lapped ſeveral times a ſlender thread of very black ſilk; in ſuch manner, that the ſeveral parts of the thread might appear upon the colours, like ſo many black lines drawn over them, or like long and ſlender dark ſhadows caſt upon them. I might have drawn black lines with a pen, but the threads were ſmaller and better defined. This paper, thus coloured and lined, I ſet againſt a wall perpendicularly to the horizon, ſo that one of the colours might ſtand to the right hand, and the other to the left.

left. Close before the paper, at the confine of the colours below, I placed a candle, to illuminate the paper strongly: for the experiment was tried in the night. The flame of the candle reached up to the lower edge of the paper, or a very little higher. Then, at the distance of six feet and two or three inches from the paper, upon the floor I erected a glass lens four inches and a quarter broad, which might collect the rays coming from the several points of the paper, and make them converge towards so many other points, at the same distance of six feet and one or two inches on the other side of the lens; and so form the Image of the coloured paper upon a white paper placed there; after the same manner that a lens at a hole in a window casts the Images of Objects abroad upon a sheet of white paper in a dark room. The aforesaid white paper, erected perpendicular to the horizon and to the rays which fell upon it from the lens, I moved sometimes towards the lens, sometimes from it, to find the places where the images of the Blue and Red parts of the coloured paper appeared most distinct. Those places I easily knew by the images of the black lines, which I had made by winding the silk about the paper. For the images of those fine and slender lines (which by reason of their blackness were like shadows on the colours) were confused and scarce visible, unless when the colours on either side of each line were terminated most distinctly. Noting therefore, as diligently as I could, the places where the images of the Red and Blue halfs of the coloured paper appeared most distinct, I found, that where the Red half of the paper appeared distinct, the Blue half appeared confused; so that the black lines drawn upon it could scarce be seen; and on the contrary, where the Blue half appeared most distinct, the Red half appeared confused, so that the black lines upon it were scarce visible. And between the two places, where these images appeared distinct, there was the distance of an inch and a half: the distance of the white paper from the lens, when the image of the Red half of the coloured paper appeared most distinct, being greater by an inch and an half than the distance of the same white paper from the lens, when the image of the Blue half appeared

differ in Refrangibility.

Lights differing in Colour differ in Refrangibility.

moſt diſtinct. In like Incidences therefore of the Blue and Red upon the lens, the Blue was refracted more by the lens than the Red, ſo as to converge ſooner by an inch and a half, and therefore is more Refrangible.

Illuſtration. In the twelfth figure, DE ſignifies the coloured paper, DG the Blue half, FE the Red half, MN the lens, HJ the white paper in that place where the Red half with its black lines appeared diſtinct, and *hi* the ſame paper in that place where the Blue half appeared diſtinct. The place *hi* was nearer to the lens MN, than the place HJ, by an inch and an half.

Scholium. The ſame things ſucceed, notwithſtanding that ſome of the circumſtances be varied: as in the firſt Experiment, when the priſm and paper are any ways inclined to the horizon, and in both when coloured lights are drawn upon very black paper. But in the deſcription of theſe experiments, I have ſet down ſuch circumſtances, by which either the phænomenon might be rendered more conſpicuous, or a novice might more eaſily try them, or by which I did try them only. The ſame thing I have often done in the following Experiments: concerning all which this one admonition may ſuffice. Now from theſe Experiments it follows not, that all the light of the Blue is more refrangible than all the light of the Red: for both lights are mixed of rays differently refrangible, ſo that in the Red there are ſome rays not leſs refrangible than thoſe of the Blue, and in the Blue there are ſome rays not more refrangible than thoſe of the Red: but theſe rays, in proportion to the whole light, are but few, and ſerve to diminiſh the event of the Experiment, but are not able to deſtroy it. For if the Red and Blue colours were more dilute and weak, the diſtance of the Images would be leſs than an inch and a half; and if they were more intenſe and full, that diſtance would be greater, as will appear hereafter. Theſe Experiments may ſuffice for the Colours of Natural Bodies. For in the Colours made by the Refraction of Priſms this Propoſition will appear, by the Experiments which are now to follow in the next

Propoſition.

PROP.

PROP. II. THEOR. II.

The Light of the Sun conſiſts of rays differently Refrangible.

The Proof by Experiments.

THE SUN'S LIGHT A COMPOUND.

Exper. 3. In a very dark chamber, at a round hole, about one-third part of an inch broad, made in the ſhut of a window, I placed a glaſs Priſm, whereby the beam of the ſun's light, which came in at the hole, might be refracted upwards toward the oppoſite wall of the chamber, and there form a coloured Image of the ſun. The axis of the priſm (that is, the line paſſing through the middle of the priſm from one end of it to the other end parallel to the edge of the refracting angle) was in this and the following Experiments perpendicular to the Incident Rays. About this axis I turned the priſm ſlowly, and ſaw the refracted light on the wall, or coloured image of the ſun, firſt to deſcend, and then to aſcend. Between the deſcent and aſcent, when the image ſeemed ſtationary, I ſtopped the priſm, and fixed it in that poſture, that it ſhould be moved no more. For in that poſture the refractions of the light at the two ſides of the refracting angle, that is at the entrance of the rays into the priſm, and at their going out of it, were equal to one another ([d]). So alſo in other Experiments, as often as I would have the refractions on both ſides the priſm to be equal to one another, I noted the place, where the image of the ſun, formed by the refracted light, ſtood ſtill between its two contrary motions, in the common period of its progreſs and regreſs; and when the image fell upon that place, I made faſt the priſm. And in this poſture, as the moſt convenient, it is to be underſtood that all the priſms are placed in the following Experiments, unleſs where ſome other poſture is deſcribed. The priſm therefore being placed in this poſture, I let the refracted light fall perpendicularly upon a ſheet of white paper at the oppoſite wall of the chamber, and obſerved the figure and dimenſions of the Solar Image formed on

([d]) Vid. Lect. Opt. Part I. § 10. and Prop. XXV and XXVI.

the

The Sun's Light

the paper by that light. This image was oblong and not oval, but terminated with two rectilinear and parallel ſides, and two ſemicircular ends. On its ſides it was bounded pretty diſtinctly, but on its ends very confuſedly and indiſtinctly, the light there decaying and vaniſhing by degrees. The Breadth of this image anſwered to the ſun's diameter, and was about two inches and the eighth part of an inch, including the penumbra. For the image was eighteen feet and an half diſtant from the priſm, and at this diſtance that breadth, if diminiſhed by the diameter of the hole in the window-ſhut, that is by a quarter of an inch, ſubtended an angle at the priſm of about half a degree, which is the ſun's apparent diameter. But the Length of the image was about ten inches and a quarter, and the length of the rectilinear ſides about eight inches; and the refracting angle of the priſm, whereby ſo great a length was made, was 64 degrees. With a leſs angle the Length of the image was leſs, the Breadth remaining the ſame. If the priſm was turned about its axis, that way which made the rays emerge more obliquely out of the ſecond refracting ſurface of the priſm, the image ſoon became an inch or two longer, or more; and if the priſm was turned about the contrary way, ſo as to make the rays fall more obliquely on the firſt refracting ſurface, the image ſoon became an inch or two ſhorter. And therefore in trying this experiment, I was as curious as I could be in placing the priſm, by the above-mentioned rule, exactly in ſuch a poſture, that the refractions of the rays, at their emergence out of the priſm, might be equal to that at their incidence on it. The priſm had ſome veins running along, within the glaſs, from one end to the other, which ſcattered ſome of the ſun's light irregularly, but had no ſenſible effect in increaſing the length of the coloured ſpectrum. For I tried the ſame experiment with two other priſms with the ſame ſucceſs. And particularly with a priſm which ſeemed free from ſuch veins, and whoſe refracting angle was $62\frac{1}{2}$ degrees, I found the length of the image $9\frac{3}{4}$ or 10 inches at the diſtance of $18\frac{1}{2}$ feet from the priſm, the breadth of the hole in the window-ſhut being $\frac{1}{4}$ of an inch, as before. And becauſe it is eaſy to commit

commit a miſtake in placing the priſm in its due poſture, I repeated the experiment four or five times, and always found the length of the image that which is ſet down above. With another priſm, of clearer glaſs and better poliſh, which ſeemed free from veins, and whoſe refracting angle was $63\frac{1}{2}$ degrees, the length of this image, at the ſame diſtance of $18\frac{1}{2}$ feet, was alſo about 10 inches, or $10\frac{1}{8}$. Beyond theſe meaſures, for about a $\frac{1}{4}$ or $\frac{1}{3}$ of an inch, at either end of the ſpectrum, the light of the Clouds ſeemed to be a little tinged with Red and Violet; but ſo very faintly, that I ſuſpected that tincture might either wholly, or in great meaſure, ariſe from ſome rays of the ſpectrum ſcattered irregularly by ſome inequalities in the ſubſtance and poliſh of the glaſs, and therefore I did not include it in theſe meaſures. Now the different magnitude of the hole in the window-ſhut, and different thickneſs of the priſm, where the rays paſſed through it, and different inclinations of the priſm to the horizon, made no ſenſible changes in the Length of the image. Neither did the different matter of the priſms make any: for in a veſſel made of poliſhed plates of glaſs, cemented together in the ſhape of a priſm and filled with water, there is the like ſucceſs of the experiment according to the quantity of the refraction. It is farther to be obſerved, that the rays went on in right lines from the priſm to the image; and therefore at their very going out of the priſm had all that inclination to one another from which the length of the image proceeded, that is the inclination of more than two degrees and an half. And yet according to the Laws of Optics vulgarly received, they could not poſſibly be ſo much inclined to one another. For let EG [in *fig.* 13.] repreſent the window-ſhut, F the hole made therein, through which a beam of the ſun's light was tranſmitted into the darkned chamber, and ABC a triangular imaginary plane, whereby the priſm is feigned to be cut tranſverſly through the middle of the light. Or if you pleaſe, let ABC repreſent the priſm itſelf, looking directly towards the ſpectator's eye with its nearer end: and let XY be the ſun, MN the paper upon which the ſolar image or ſpectrum is caſt, and PT the image itſelf; whoſe ſides, towards *v* and *w*, are rectilinear and parallel, and ends, towards P and T ſemicircular.

A COMPOUND.

The Sun's Light

circular. YKHP and XLJT are two rays; the firſt of which comes from the lower part of the ſun to the higher part of the image, and is refracted in the priſm at K and H; and the latter comes from the higher part of the ſun to the lower part of the image, and is refracted at L and J. Since the refractions on both ſides the priſm are equal to one another, that is the refraction at K equal to the refraction at J, and the refraction at L equal to the refraction at H, ſo that the refractions of the incident rays at K and L taken together are equal to the refractions of the emergent rays at H and J taken together: it follows, by adding equal things to equal things, that the refractions at K and H taken together, are equal to the refractions at J and L taken together; and therefore the two rays, being equally refracted, have the ſame inclination to one another after refraction, which they had before; that is the inclination of half a degree anſwering to the ſun's diameter. For ſo great was the inclination of the rays to one another before refraction. So then, the Length of the image PT would, by the rules of vulgar Optics, ſubtend an angle of half a degree at the priſm, and by conſequence be equal to the breadth *vw*; and therefore the image would be round (e). Thus it would be, were the two rays XLJT and YKHP, and all the reſt which form the image P*w*T*v*, alike refrangible. And therefore ſeeing by experience it is found that the image is not round, but about five times longer than broad, the rays, which going to the upper end P of the image ſuffer the greateſt refraction, muſt be more refrangible than thoſe which go to the lower end T, unleſs the inequality of refraction be caſual.

This image or ſpectrum PT was coloured, being Red at its leaſt refracted end T, and Violet at its moſt refracted end P, and Yellow, Green and Blue in the intermediate ſpaces. Which agrees with the firſt Propoſition, that lights which differ in Colour do alſo differ in Refrangibility. The length of the image in the foregoing Experiments I meaſured from the fainteſt and outmoſt Red at one end, to the fainteſt and outmoſt Blue at the other end, excepting only a little penumbra, whoſe breadth ſcarce exceeded a quarter of an inch, as was ſaid above.

(e) Vide Lect. Opt. Part I. § 4—9.

Exper. 4. In the ſun's beam which was propagated into the room through the hole in the window-ſhut, at the diſtance of ſome feet from the hole, I held the priſm in ſuch a poſture, that its axis might be perpendicular to that beam. Then I looked through the priſm upon the Hole, and turning the priſm to and fro about its axis, to make the image of the hole aſcend and deſcend; when between its two contrary motions it ſeemed ſtationary, I ſtopped the priſm, that the refractions of both ſides of the Refracting Angle might be equal to each other, as in the former Experiment. In this ſituation of the priſm, viewing through it the ſaid hole, I obſerved the Length of its refracted image to be many times greater than its Breadth; and that the moſt refracted part thereof appeared Violet, the leaſt refracted Red, the middle parts Blue, Green, and Yellow in order. The ſame thing happened when I removed the priſm out of the Sun's light, and looked through it upon the Hole ſhining by the light of the Clouds beyond it. And yet if the refraction were done regularly, according to one certain proportion of the ſines of incidence and refraction, as is vulgarly ſuppoſed, the refracted image ought to have appeared round. A COMPOUND.

So then, by theſe two Experiments it appears, that in equal Incidences there is a conſiderable inequality of Refractions. But whence this inequality ariſes, whether it be that ſome of the incident rays are refracted more and others leſs, conſtantly, or by chance; or that one and the ſame ray is by refraction diſturbed, ſhattered, dilated, and as it were ſplit and ſpread into many diverging rays, as *Grimaldo* ſuppoſes, does not yet appear by theſe Experiments, but will appear by thoſe that follow.

Exper. 5. Conſidering therefore, that if in the third Experiment the image of the ſun ſhould be drawn out into an oblong form, either by a dilatation of every ray, or by any other caſual inequality of the refractions, the ſame oblong image would, by a ſecond refraction made ſideways, be drawn out as much in Breadth by the like dilatation of the rays, or other caſual inequality of the refractions ſideways; I tried what would be the effects of ſuch a ſecond refraction. For this end I ordered all things as in the third Experiment; and then placed a ſecond priſm

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immediately after the firſt, in a croſs poſition to it, that it might again refract the beam of the ſun's light, which came to it through the firſt priſm. In the firſt priſm this beam was refracted upwards, and in the ſecond ſideways. And I found, that by the refraction of the ſecond priſm the Breadth of the image was not increaſed; but its ſuperior part, which in the firſt priſm ſuffered the greater refraction and appeared Violet and Blue, did again in the ſecond priſm ſuffer a greater refraction than its inferior part, which appeared Red and Yellow; and this without any dilatation of the image in Breadth.

Illuſtration. Let S [in *fig.* 14] repreſent the ſun; F the hole in the window; ABC the firſt priſm; DH the ſecond priſm; Y the round image of the ſun, made by a direct beam of light when the priſms are taken away; PT the oblong image of the ſun, made by that beam paſſing through the firſt priſm alone, when the ſecond priſm is taken away; and *pt* the image made by the croſs refractions of both priſms together. Now if the rays, which tend towards the ſeveral points of the round image, Y, were dilated and ſpread by the refraction of the firſt priſm, ſo that they ſhould not any longer go in ſingle lines to ſingle points, but that every ray being ſplit, ſhattered, and changed from a linear ray to a ſuperficies of rays diverging from the point of refraction, and lying in the plane of the angles of incidence and refraction, they ſhould go in thoſe planes to ſo many lines reaching almoſt from one end of the image PT to the other; and if that image ſhould thence become oblong: thoſe rays, and their ſeveral parts tending towards the ſeveral points of the image PT, ought to be again dilated and ſpread ſideways by the tranſverſe refraction of the ſecond priſm, ſo as to compoſe a four-ſquare image, ſuch as is repreſented at π. For the better underſtanding of which, let the image PT be diſtinguiſhed into five equal parts PQK, KQRL, LRSM, MSVN, NVT. And by the ſame irregularity that the orbicular light Y is, by the refraction of the firſt priſm, dilated and drawn out into a long image PT; the light PQK, which takes up a ſpace of the ſame length and breadth with the light Y, ought to be, by the refraction of the ſecond priſm, dilated and drawn out into the long image πqkp; and the

light

light KQRL into the long image *kqrl*; and the lights RLSM, MSVN, NVT, into ſo many other long images *lrsm*, *msvn*, *nvt*]; and all theſe long images would compoſe the four-ſquare image *π*]. Thus it ought to be, were every ray dilated by refraction, and ſpread into a triangular ſuperficies of rays diverging from the point of refraction. For the ſecond refraction would ſpread the rays one way, as much as the firſt doth another, and ſo dilate the image in Breadth as much as the firſt doth in Length. And the ſame thing ought to happen, were ſome rays caſually refracted more than others. But the event is otherwiſe. The image PT was not made broader by the refraction of the ſecond priſm, but only became oblique, as it is repreſented at *pt*; its upper end P being by the refraction tranſlated to a greater diſtance than its lower end T. So then the light which went towards the upper end, P, of the image, was (at equal incidences) more refracted in the ſecond priſm, than the light which tended towards the lower end T; that is the Blue and Violet, than the Red and Yellow: and therefore was more refrangible. The ſame light was, by the refraction of the firſt priſm, tranſlated farther from the place Y to which it tended before refraction; and therefore ſuffered, as well in the firſt priſm as in the ſecond, a greater refraction than the reſt of the light, and by conſequence was more refrangible than the reſt, even before its incidence on the firſt priſm. A COMPOUND.

Sometimes I placed a third priſm after the ſecond, and ſometimes alſo a fourth after the third, by all which the image might be often refracted ſideways: but the rays, which were more refracted than the reſt in the firſt priſm, were alſo more refracted in all the reſt, and that without any dilatation of the image ſideways: and therefore thoſe rays, for their conſtancy of a greater refraction, are deſervedly reputed more refrangible.

But that the meaning of this experiment may more clearly appear, it is to be conſidered that the rays, which are equally refrangible, do fall upon a circle anſwering to the ſun's diſque. For this was proved in the third Experiment. By a circle I underſtand not here a perfect geometrical circle; but any orbicular figure, whoſe length is equal to its breadth, and which, as to ſenſe, may ſeem circular. Let therefore AG [in *fig.* 15] repre-

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ſent the circle, which all the moſt refrangible rays, propagated from the whole diſque of the ſun, would illuminate and paint upon the oppoſite wall, if they were alone; EL the circle, which all the leaſt refrangible rays would, in like manner, illuminate and paint, if they were alone; BH, CJ, DK, the circles, which ſo many intermediate ſorts of rays would ſucceſſively paint upon the wall, if they were ſingly propagated from the ſun in ſucceſſive order, the reſt being always intercepted; and conceive that there are other intermediate circles without number, which innumerable other intermediate ſorts of rays would ſucceſſively paint upon the wall, if the ſun ſhould ſucceſſively emit every ſort apart. And ſeeing the ſun emits all theſe ſorts at once, they muſt all together illuminate and paint innumerable equal circles; of all which, being according to their degrees of refrangibility placed in order in a continual ſeries, that oblong ſpectrum, PT, is compoſed, which I deſcribed in the third Experiment. Now if the ſun's circular image Y, [in *fig.* 14, 15] which is made by an unrefracted beam of light, was, by any dilatation of the ſingle rays, or by any other irregularity in the refraction of the firſt priſm, converted into the oblong ſpectrum, PT: then ought every circle, AG, BH, CJ, &c. in that ſpectrum, by the croſs refraction of the ſecond priſm again dilating or otherwiſe ſcattering the rays as before, to be in like manner drawn out and transformed into an oblong figure; and thereby the Breadth of the image PT would be now as much augmented, as the Length of the image Y was before by the refraction of the firſt priſm; and thus, by the refraction of both priſms together, would be formed a four-ſquare figure *pπt*], as I deſcribed above. Wherefore, ſince the breadth of the ſpectrum PT is not increaſed by the refraction ſideways, it is certain that the rays are not ſplit or dilated, or otherways irregularly ſcattered by that refraction, but that every circle is, by a regular and uniform refraction, tranſlated entire into another place, as the circle AG, by the greateſt refraction, into the place *ag*; the circle BH, by a leſs refraction, into the place *bh*; the circle CJ, by a refraction ſtill leſs, into the place *ci*; and ſo of the reſt: by which means a new ſpectrum *pt*, inclined to the former PT, is in like manner compoſed of circles lying in

in a right line; and theſe circles muſt be of the ſame bigneſs with the former, becauſe the breadths of all the ſpectrums, Y, PT and *pt*, at equal diſtances from the priſms are equal. A COMPOUND.

I concluded farther, that by the breadth of the hole F, through which the light enters into the dark chamber, there is a penumbra made in the circuit of the ſpectrum Y, and that penumbra remains in the rectilinear ſides of the ſpectrums PT and *pt*. I placed therefore at that hole a lens or object-glaſs of a teleſcope, which might caſt the image of the ſun diſtinctly on Y, without any penumbra at all; and found that the penumbra of the rectilinear ſides of the oblong ſpectrums, PT and *pt*, was alſo thereby taken away, ſo that thoſe ſides appeared as diſtinctly defined, as did the circumference of the firſt image Y. Thus it happens, if the glaſs of the priſms be free from veins, and their ſides be accurately plane, and well poliſhed without thoſe numberleſs waves or curles which uſually ariſe from ſand-holes a little ſmoothed with poliſhing with putty. If the glaſs be only well poliſhed and free from veins, and the ſides not accurately plane, but a little convex or concave, as it frequently happens; yet may the three ſpectrums, Y, PT and *pt*, want penumbras, but not in equal diſtances from the priſms. Now from this want of penumbras, I knew more certainly that every one of the circles was refracted according to ſome moſt regular, uniform, and conſtant law. For if there were any irregularity in the refraction, the right lines AE and GL, which all the circles in the ſpectrum PT do touch, could not by that refraction be tranſlated into the lines *ae* and *gl*, as diſtinct and ſtraight as they were before; but there would ariſe, in thoſe tranſlated lines, ſome penumbra or crookedneſs or undulation, or other ſenſible perturbation contrary to what is found by experience. Whatſoever penumbra or perturbation ſhould be made in the circles by the croſs refraction of the ſecond priſm, all that penumbra or perturbation would be conſpicuous in the right lines *ae* and *gl*, which touch thoſe circles. And therefore ſince there is no ſuch penumbra or perturbation in thoſe right lines, there muſt be none in the circles. Since the diſtance between thoſe tangents, or breadth of the ſpectrum, is not increaſed by the refractions, the diameters of the circles are

not

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not increaſed thereby. Since thoſe tangents continue to be right lines, every circle, which in the firſt priſm is more or leſs refracted, is exactly in the ſame proportion more or leſs refracted in the ſecond. And ſeeing all theſe things continue to ſucceed after the ſame manner, when the rays are again in a third priſm, and again in a fourth, refracted ſideways; it is evident, that the rays of one and the ſame circle, as to their degree of refrangibility, continue always uniform and homogeneal to one another, and that thoſe of ſeveral circles do differ in degree of refrangibility, and that in ſome certain and conſtant proportion. Which is the thing I was to prove.

There is yet another circumſtance or two of this Experiment, by which it becomes ſtill more plain and convincing. Let the ſecond priſm DH [in *fig.* 16] be placed not immediately after the firſt, but at ſome diſtance from it; ſuppoſe in the mid-way between it and the wall, on which the oblong ſpectrum PT is caſt; ſo that the light from the firſt priſm may fall upon it in the form of an oblong ſpectrum $\pi\tau$ parallel to this ſecond priſm, and be refracted ſideways to form the oblong ſpectrum *pt* upon the wall. And you will find as before, that this ſpectrum *pt* is inclined to that ſpectrum PT, which the firſt priſm forms alone without the ſecond; the Blue ends P and *p* being farther diſtant from one another, than the Red ones T and *t*; and by conſequence that the rays which go to the blue end, π, of the image $\pi\tau$, and which therefore ſuffer the greateſt refraction in the firſt priſm, are again in the ſecond priſm more refracted than the reſt.

The ſame thing I tried alſo by letting the ſun's light into a dark room through two little round holes, F and ϕ, [in *fig.* 17] made in the window, and with two parallel priſms, ABC and $\alpha\beta\gamma$, placed at thoſe holes (one at each) refracting thoſe two beams of light to the oppoſite wall of the chamber; in ſuch manner that the two coloured images, PT and MN, which they there painted, were joined end to end, and lay in one ſtraight line; the Red end T of the one touching the Blue end M of the other. For if theſe two refracted beams were again, by a third priſm DH placed croſs

(f) This Experiment may be tried with two priſms only. For the buſineſs of the two ABC, $\alpha\beta\gamma$

croſs to the two firſt, refracted ſideways, and the ſpectrums thereby tranſlated to ſome other part of the wall of the chamber, ſuppoſe the ſpectrum PT to *pt* and the ſpectrum MN to *mn*; theſe tranſlated ſpectrums, *pt* and *mn*, would not lie in one ſtraight line with their ends contiguous as before, but be broken off from one another and become parallel; the Blue end *m* of the image *mn* being, by a greater refraction, tranſlated farther from its former place MT, than the Red end *t* of the other image *pt* from the ſame place MT; which puts the Propoſition paſt diſpute. And this happens whether the third priſm, DH, be placed immediately after the two firſt, or at a great diſtance from them, ſo that the light refracted in the two firſt priſms be either white and circular, or coloured and oblong, when it falls on the third (f). A COMPOUND.

Exper. 6. In the middle of two thin boards I made round holes, a third part of an inch in diameter, and in the window-ſhut a much broader hole being made, to let into my darkened chamber a large beam of the ſun's light; I placed a priſm behind the ſhut in that beam, to refract it towards the oppoſite wall; and cloſe behind the priſm I fixed one of the boards, in ſuch manner that the middle of the refracted light might paſs through the hole made in it, and the reſt be intercepted by the board. Then, at the diſtance of about twelve feet from the firſt board, I fixed the other board; in ſuch manner, that the middle of the refracted light, which came through the hole in the firſt board and fell upon the oppoſite wall, might paſs through the hole in this other board; and the reſt, being intercepted by the board, might paint upon it the coloured ſpectrum of the ſun. And cloſe behind this board I fixed another priſm, to refract the light, which came through the hole. Then I returned ſpeedily to the firſt priſm; and by turning it ſlowly to and fro about its axis, I cauſed the image, which fell upon the ſecond board, to move up and down upon that board, that all its parts might ſucceſſively paſs through the hole in that board, and fall upon the priſm behind it. And in the mean time, I noted the places on the oppoſite wall, to which that light, after its refraction in the ſecond

(f) $\alpha\beta\gamma$ may be performed by a ſingle one having a black paper, with two round holes in it, paſted on the ſide which is turned to the light. Vide LECT. OPT. Part II. § 19.

priſm,

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priſm, did paſs; and by the difference of the places I found that the light, which being moſt refracted in the firſt priſm did go to the Blue end of the image, was again more refracted in the ſecond priſm, than the light which went to the Red end of that image; which proves as well the firſt Propoſition as the ſecond. And this happened whether the axis of the two priſms were parallel, or inclined to one another and to the horizon in any given angles.

Illuſtration. Let F [in *fig.* 18] be the wide hole in the window ſhut, through which the ſun ſhines upon the firſt priſm ABC, and let the refracted light fall upon the middle of the board DE, and the middle part of that light upon the hole G, made in the middle of that board. Let this trajected part of the light fall again upon the middle of the ſecond board, *de*, and there paint ſuch an oblong coloured image of the ſun, as was deſcribed in the third Experiment. By turning the priſm ABC ſlowly to and fro about its axis, this image will be made to move up and down the board *de*, and by this means all its parts, from one end to the other, may be made to paſs ſucceſſively through the hole *g*, which is made in the middle of that board. In the mean while another priſm, *abc*, is to be fixed next after that hole *g*, to refract the trajected light a ſecond time. And theſe things being thus ordered, I marked the places, M and N, of the oppoſite wall upon which the refracted light fell; and found, that whilſt the two boards and ſecond priſm remained unmoved, thoſe places, by turning the firſt priſm about its axis, were changed perpetually. For when the lower part of the light, which fell upon the ſecond board, *de*, was caſt through the hole, *g*, it went to a lower place, M, on the wall; and when the higher part of that light was caſt through the ſame hole, *g*, it went to a higher place, N, on the wall; and when any intermediate part of the light was caſt through that hole, it went to ſome place on the wall between M and N. The unchanged poſition of the holes in the boards made the incidence of the rays upon the ſecond priſm to be the ſame in all caſes. And yet, in that common incidence, ſome of the rays were more refracted and others leſs. And thoſe were more refracted in this priſm, which, by a greater refraction

in the firſt priſm, were more turned out of the way; and there- A COMPOUND.
fore, for their conſtancy of being more refracted, are deſervedly called more refrangible.

Exper. 7. At two holes made near one another in my window-ſhut I placed two priſms, one at each; which might caſt upon the oppoſite wall (after the manner of the third Experiment) two oblong coloured images of the ſun. And at a little diſtance from the wall, I placed a long ſlender paper with ſtraight and parallel edges, and ordered the priſms and paper ſo, that the Red colour of one image might fall directly upon one half of the paper, and the Violet colour of the other image upon the other half of the ſame paper: ſo that the paper appeared of two colours, Red and Violet, much after the manner of the painted paper in the firſt and ſecond Experiments. Then with a black cloth I covered the wall behind the paper, that no light might be reflected from it to diſturb the Experiment; and viewing the paper through a third priſm held parallel to it, I ſaw that half of it, which was illuminated by the Violet light, to be divided from the other half by a greater refraction, eſpecially when I went a good way off from the paper. For when I viewed it too near at hand, the two halfs of the paper did not appear fully divided from one another, but ſeemed contiguous at one of their angles, like the painted paper in the firſt Experiment. Which alſo happened when the paper was too broad.

Sometimes inſtead of the paper I uſed a white thread; and this appeared through the priſm divided into two parallel threads, as is repreſented in the nineteenth figure; where DG denotes the thread illuminated with Violet light from D to E, and with Red light from F to G, and *edfg* are the parts of the thread ſeen by refraction. If one half of the thread be conſtantly illuminated with Red, and the other half be illuminated with all the colours ſucceſſively (which may be done by cauſing one of the priſms to be turned about its axis whilſt the other remains unmoved) this other half, in viewing the thread through the priſm, will appear in a continued right line with the firſt half when illuminated with Red; and begin to be a little divided from it, when illuminated with Orange; and remove farther from it, when il-

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luminated with Yellow; and ſtill farther when with Green; and farther when with Blue; and go yet farther off, when illuminated with Indigo; and fartheſt when with deep Violet. Which plainly ſhews, that the lights of ſeveral colours are more and more refrangible from one another, in this order of their colours; Red, Orange, Yellow, Green, Blue, Indigo, deep Violet; and ſo proves as well the firſt Propoſition as the ſecond.

I cauſed alſo the coloured ſpectrums PT [in *fig.* 17] and MN, made in a dark chamber by the refractions of two priſms, to lie in a right line end to end, as was deſcribed above in the fifth Experiment; and viewing them through a third priſm, held parallel to their length, they appeared no longer in a right line, but became broken from one another, as they are repreſented at *pt* and *mn*; the Violet end, *m*, of the ſpectrum, *mn*, being by a greater refraction tranſlated farther from its former place, MT, than the Red end, *t*, of the other ſpectrum *pt*.

I farther cauſed thoſe two ſpectrums PT [in *fig.* 20] and MN to become co-incident, in an inverted order of their colours, the Red end of each falling on the Violet end of the other, as they are repreſented in the oblong figure PTMN; and then viewing them through a priſm DH held parallel to their length, they appeared not co-incident as when viewed with the naked eye, but in the form of two diſtinct ſpectrums *pt* and *mn* croſſing one another in the middle after the manner of the letter x. Which ſhews, that the Red of the one ſpectrum and Violet of the other, which were co-incident at PN and MT, being parted from one another, by a greater refraction of the Violet to *p* and *m*, than of the Red to *n* and *t*, do differ in degrees of refrangibility.

I illuminated alſo a little circular piece of white paper all over, with the lights of both priſms intermixed; and when it was illuminated with the Red of one ſpectrum and deep Violet of the other, ſo as by the mixture of thoſe colours to appear all over Purple, I viewed the paper, firſt at a leſs diſtance, and then at a greater, through a third priſm; and as I went from the paper, the refracted image thereof became more and more divided by the unequal refraction of the two mixed colours, and at length parted into two diſtinct images, a Red one and a Violet one;

whereof

whereof the Violet was fartheſt from the paper, and therefore ſuffered the greateſt refraction. And when the priſm at the window, which caſt the violet on the paper, was taken away, the Violet image diſappeared; but when the other priſm was taken away, the Red vaniſhed: which ſhews, that theſe two images were nothing elſe than the lights of the two priſms, which had been intermixed on the purple paper, but were parted again, by their unequal refractions, made in the third priſm, through which the paper was viewed. This alſo was obſervable, that if one of the priſms at the window, ſuppoſe that which caſt the Violet on the paper, was turned about its axis to make all the colours in this order, Violet, Indigo, Blue, Green, Yellow, Orange, Red, fall ſucceſſively on the paper from that priſm, the Violet image changed colour accordingly, turning ſucceſſively to Indigo, Blue, Green, Yellow and Red; and in changing colour came nearer and nearer to the Red image made by the other priſm, until, when it was alſo Red, both images became fully co-incident. A COMPOUND.

I placed alſo two paper-circles very near one another, the one in the Red light of one priſm, and the other in the Violet light of the other. The circles were each of them an inch in diameter, and behind them the wall was dark, that the Experiment might not be diſturbed by any light coming from thence. Theſe circles thus illuminated, I viewed through a priſm, ſo held that the refraction might be made towards the Red circle, and as I went from them they came nearer and nearer together, and at length became co-incident; and afterwards when I went ſtill farther off, they parted again in a contrary order, the Violet by a greater refraction being carried beyond the Red.

Exper. 8. In Summer, when the ſun's light uſes to be ſtrongeſt, I placed a priſm at the hole of the window-ſhut, as in the third Experiment, yet ſo that its axis might be parallel to the axis of the World, and at the oppoſite wall, in the ſun's refracted light, I placed an open book. Then going ſix feet and two inches from the book, I placed there the above-mentioned lens, by which the light, reflected from the book, might be made to converge and meet again at the diſtance of ſix feet and two inches

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behind the lens, and there paint the ſpecies of the book upon a ſheet of white paper, much after the manner of the ſecond Experiment. The book and lens being made faſt, I noted the place where the paper was, when the letters of the book, illuminated by the fulleſt Red light of the ſolar image falling upon it, did caſt their ſpecies on that paper moſt diſtinctly: and then I ſtayed, till by the motion of the ſun, and conſequent motion of his image on the book, all the colours from that Red to the middle of the Blue paſſed over thoſe letters; and when thoſe letters were illuminated by that Blue, I noted again the place of the paper, when they caſt their ſpecies moſt diſtinctly upon it: and I found that this laſt place of the paper was nearer to the lens, than its former place, by about two inches and an half, or two and three quarters. So much ſooner therofore did the light in the Violet end of the image, by a greater refraction, converge and meet, than the light in the Red end. But in trying this the chamber was as dark as I could make it. For if theſe colours be diluted and weakened by the mixture of any adventitious light, the diſtance between the places of the paper will not be ſo great. This diſtance, in the ſecond Experiment, where the colours of Natural Bodies were made uſe of, was but an inch and an half, by reaſon of the imperfection of thoſe colours. Here, in the colours of the Priſm, which are manifeſtly more full, intenſe, and lively than thoſe of Natural Bodies, the diſtance is two inches and three quarters. And were the colours ſtill more full, I queſtion not but that the diſtance would be conſiderably greater. For the coloured light of the priſm, by the interfering of the circles deſcribed in the ſecond figure of the fifth Experiment, and alſo by the light of the very bright clouds next the ſun's body intermixing with theſe colours, and by the light ſcattered by the inequalities in the poliſh of the priſm, was ſo very much compounded, that the ſpecies, which thoſe faint and dark colours, the Indigo and Violet, caſt upon the paper, were not diſtinct enough to be well obſerved.

Exper. 9. A priſm, whoſe two angles at its baſe were equal to one another and half right ones, and the third a right one, I placed in a beam of the ſun's light let into a dark chamber,

through a hole in the window-ſhut, as in the third Experiment. And turning the priſm ſlowly about its axis, until all the light which went through one of its angles, and was refracted by it, began to be reflected by its baſe, at which till then it went out of the glaſs; I obſerved that thoſe rays, which had ſuffered the greateſt refraction, were ſooner reflected than the reſt. I conceived therefore that thoſe rays of the Reflected light, which were moſt Refrangible, did firſt of all, by a total reflexion, become more copious in that light than the reſt; and that afterwards the reſt alſo, by a total reflexion, became as copious as theſe. To try this, I made the Reflected light paſs through another priſm, and being refracted by it to fall afterwards upon a ſheet of white paper placed at ſome diſtance behind it; and there, by that Refraction, to paint the uſual colours of the priſm. And then cauſing the firſt priſm to be turned about its axis as above, I obſerved that when thoſe rays, which in this priſm had ſuffered the greateſt refraction, and appeared of a Blue and Violet colour, began to be totally reflected, the Blue and Violet light on the paper, which was moſt refracted in the ſecond priſm, received a ſenſible increaſe above that of the Red and Yellow, which was leaſt refracted; and afterwards when the reſt of the light, which was Green, Yellow and Red, began to be totally reflected in the firſt priſm, the light of thoſe colours on the paper received as great an increaſe, as the Violet and Blue had done before. Whence it is manifeſt, that the beam of light reflected by the baſe of the priſm, being augmented firſt by the more refrangible rays, and afterwards by the leſs refrangible ones, is compounded of rays differently refrangible. And that all ſuch Reflected light is of the ſame nature with the ſun's light, before its incidence on the baſe of the priſm, no man ever doubted: it being generally allowed, that light, by ſuch reflexions, ſuffers no alteration in its modifications and properties. I do not here take notice of any refractions made in the ſides of the firſt priſm, becauſe the light enters it perpendicularly at the firſt ſide, and goes out perpendicularly at the ſecond ſide, and therefore ſuffers none. So then, the ſun's incident light being of the ſame temper and conſtitution with his emergent light, and the laſt being compounded of rays

A COMPOUND.

differently

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differently refrangible, the firſt muſt be in like manner compounded.

Illuſtration. In the twenty-firſt figure, ABC is the firſt priſm; BC its baſe; B and C its equal angles at the baſe, each of 45 degrees; A its rectangular vertex; FM a beam of the ſun's light, let into a dark room through a hole, F, one third part of an inch broad; M its incidence on the baſe of the priſm; MG a leſs refracted ray; MH a more refracted ray; MN the beam of light reflected from the baſe; VXY the ſecond priſm, by which this beam, in paſſing through it, is refracted; N*t* the leſs refracted light of this beam; and N*p* the more refracted part thereof. When the firſt priſm, ABC, is turned about its axis, according to the order of the letters ABC, the rays MH emerge more and more obliquely out of that priſm, and at length after their moſt oblique emergence are reflected towards N, and going on to *p* do increaſe the number of the rays N*p*. Afterwards by continuing the motion of the firſt priſm, the rays MG are alſo reflected to N, and increaſe the number of the rays N*t*. And therefore the light MN admits into its compoſition, firſt the more refrangible rays, and then the leſs refrangible rays; and yet, after this compoſition, is of the ſame nature with the ſun's immediate light FM, the reflexion of the ſpecular baſe BC cauſing no alteration therein (g).

Exper. 10. Two priſms, which were alike in ſhape, I tied ſo together, that their axes and oppoſite ſides being parallel, they compoſed a parallelepiped. And, the ſun ſhining into my dark chamber through a little hole in the window-ſhut, I placed that parallelepiped in his beam at ſome diſtance from the hole, in ſuch a poſture, that the axes of the priſms might be perpendicular to the incident rays; and that thoſe rays, being incident upon the firſt ſide of one priſm, might go on through the two contiguous ſides of both priſms, and emerge out of the laſt ſide of the ſecond priſm. This ſide, being parallel to the firſt ſide of the firſt priſm, cauſed the emerging light to be parallel to the incident. Then, beyond theſe two priſms I placed a third, which might refract that emergent light, and by that refraction caſt

(g) Vid. Lect. Opt. Part II. § 71—75.

the

the uſual colours of the priſm upon the oppoſite wall, or upon a ſheet of white paper, held at a convenient diſtance behind the priſm, for that refracted light to fall upon it. After this I turned the parallelepiped about its axis; and found, that when the contiguous ſides of the two priſms became ſo oblique to the incident rays, that thoſe rays began all of them to be reflected, thoſe rays, which in the third priſm had ſuffered the greateſt Refraction and painted the paper with Violet and Blue, were firſt of all by a total Reflexion taken out of the tranſmitted light; the reſt remaining, and on the paper painting their colours of Green, Yellow, Orange, and Red, as before; and afterwards by continuing the motion of the two priſms, the reſt of the rays alſo, by a total Reflexion, vaniſhed in order, according to their degrees of Refrangibility. The light therefore, which emerged out of the two priſms, is compounded of rays differently refrangible, ſeeing the more refrangible rays may be taken out of it, while the leſs refrangible remain. But this light, being trajected only through the parallel ſuperficies of the two priſms, if it ſuffered any change by the refraction of one ſuperficies, it loſt that impreſſion by the contrary refraction of the other ſuperficies; and ſo, being reſtored to its priſtine conſtitution, became of the ſame nature and condition as at firſt, before its incidence on thoſe priſms; and therefore, before its incidence, was as much compounded of rays differently refrangible, as afterwards. A COMPOUND.

Illuſtration. In the twenty-ſecond figure, ABC and BCD are the two priſms tied together in the form of a parallelepiped, their ſides BC and CB being contiguous, and their ſides AB and CD parallel. And HJK is the third priſm, by which the ſun's light, propagated through the hole F into the dark chamber, and there paſſing through thoſe ſides of the priſms, AB, BC, CB and CD, is refracted at O to the white paper PT; falling there partly upon P by a greater refraction, partly upon T by a leſs refraction, and partly upon R, and other intermediate places, by intermediate refractions. By turning the parallelepiped ACBD about its axis, according to the order of the letters A, C, D, B, at length when the contiguous planes, BC and CB, become ſufficiently oblique to the rays FM, which are incident upon them at M, there will vaniſh

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nish totally out of the refracted light OPT, first of all the most refracted rays OP, (the rest OR and OT remaining as before) then the rays OR, and other intermediate ones, and lastly, the least refracted rays OT. For when the plane BC becomes sufficiently oblique to the rays incident upon it, those rays will begin to be totally reflected by it towards N; and first the most refrangible rays will be totally reflected (as was explained in the preceding Experiment) and by consequence must first disappear at P; and afterwards the rest, as they are in order totally reflected to N, they must appear in the same order at R and T. So then the rays, which at O suffer the greatest refraction, may be taken out of the light MO, whilst the rest of the rays remain in it; and therefore that light MO is compounded of rays differently refrangible. And because the planes AB and CD are parallel, and therefore by equal and contrary refractions destroy one anothers effects, the incident light FM must be of the same kind and nature with the emergent light MO, and therefore doth also consist of rays differently refrangible. These two lights, FM and MO, before the most refrangible rays are separated out of the emergent light MO, agree in colour, and in all other properties so far as my observation reaches; and therefore are deservedly reputed of the same nature and constitution, and by consequence the one is compounded as well as the other. But after the most refrangible rays begin to be totally reflected, and thereby separated out of the emergent light MO, that light changes its colour from White to a dilute and faint Yellow, a pretty good Orange, a very full Red successively, and then totally vanishes. For after the most refrangible rays, which paint the paper at P with a Purple colour, are, by a total reflexion, taken out of the beam of light MO; the rest of the colours, which appear on the paper at R and T, being mixed in the light MO, compound there a faint Yellow: and after the Blue and part of the Green which appear on the paper between P and R are taken away; the rest, which appear between R and T, that is the Yellow, Orange, Red, and a little Green, being mixed in the beam MO compound there an Orange; and when all the rays are by reflexion taken out of the beam MO, except the least refrangible, which at T appear of a full Red, their

their colour is the ſame in that beam MO, as afterwards at T, the refraction of the priſm HJK ſerving only to ſeparate the differently refrangible rays, without making any alteration in their colours, as ſhall be more fully proved hereafter. All which confirms as well the firſt Propoſition as the ſecond. A COMPOUND.

Scholium. If this Experiment and the former be conjoined and made one, by applying a fourth priſm VXY [in *fig.* 22] to refract the reflected beam MN towards *pt*, the concluſion will be clearer. For then the light N*p*, which in the fourth priſm is more refracted, will become fuller and ſtronger when the light OP, which in the third priſm HJK is more refracted, vaniſhes at P; and afterwards, when the leſs refracted light, OT, vaniſhes at T, the leſs refracted light N*t* will become encreaſed, whilſt the more refracted light at *p* receives no farther encreaſe. And as the trajected beam, MO, in vaniſhing is always of ſuch a colour, as ought to reſult from the mixture of the colours which fall upon the paper PT, ſo is the reflected beam, MN, always of ſuch a colour, as ought to reſult from the mixture of the colours which fall upon the paper *pt*. For when the moſt refrangible rays are, by a total reflexion, taken out of the beam MO, and leave that beam of an Orange colour, the exceſs of thoſe rays in the reflected light, does not only make the Violet, Indigo and Blue at *p* more full, but alſo makes the beam MN change from the yellowiſh colour of the ſun's light, to a pale White inclining to Blue, and afterward recover its yellowiſh colour again, ſo ſoon as all the reſt of the tranſmitted light MOT is reflected.

Now ſeeing that in all this variety of Experiments, whether the trial be made in light Reflected, and that either from Natural Bodies, as in the firſt and ſecond Experiment, or Specular, as in the ninth; or in light Refracted, and that, either before the unequally refracted rays are, by diverging, ſeparated from one another, and loſing their whiteneſs which they have altogether, appear ſeverally of ſeveral colours, as in the fifth Experiment; or after they are ſeparated from one another, and appear coloured as in the ſixth, ſeventh, and eighth Experiments; or in light trajected through parallel ſuperficies, deſtroying each others effects, as in the tenth Experiment; there are always found rays,

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which at equal incidences on the ſame Medium ſuffer unequal refractions; and that without any ſplitting or dilating of ſingle rays, or contingence in the inequality of the refractions, as is proved in the fifth and ſixth Experiments: and ſeeing the rays which differ in refrangibility may be parted and ſorted from one another; and that either by Refraction, as in the third Experiment, or by Reflexion, as in the tenth; and then the ſeveral ſorts apart at equal incidences ſuffer unequal refractions, and thoſe ſorts are more refracted than others after ſeparation, which were more refracted before it, as in the ſixth and following Experiments; and if the Sun's light be trajected through three or more croſs priſms ſucceſſively, thoſe rays, which in the firſt priſm are refracted more than others, are in all the following priſms refracted more than others in the ſame rate and proportion, as appears by the fifth Experiment: it is manifeſt, that the Sun's Light is an Heterogeneous Mixture of rays, ſome of which are conſtantly more Refrangible than others, as was propoſed.

PROP. III. THEOR. III.

The Sun's light conſiſts of rays differing in Reflexibility, and thoſe rays are more reflexible than others, which are more refrangible.

This is manifeſt by the ninth and tenth Experiments: for in the ninth Experiment, by turning the priſm about its axis, until the rays within it, which in going out into the air were refracted by its baſe, became ſo oblique to the baſe, as to begin to be totally reflected thereby; thoſe rays became firſt of all totally reflected, which before, at equal incidences with the reſt, had ſuffered the greateſt refraction. And the ſame thing happens in the reflexion made by the common baſe of the two priſms in the tenth Experiment.

PROP. IV. PROB. I.

To ſeparate from one another the Heterogeneous rays of Compound Light.

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The heterogeneous rays are in ſome meaſure ſeparated from one another by the refraction of the priſm in the third Experiment;

ment; and in the fifth Experiment, by taking away the penumbra from the rectilinear ſides of the coloured image, that ſeparation in thoſe very rectilinear ſides or ſtraight edges of the image becomes perfect. But in all places between thoſe rectilinear edges, thoſe innumerable circles there deſcribed, which are ſeverally illuminated by homogeneal rays, by interfering with one another, and being every where commixed, do render the light ſufficiently compound. But if theſe circles, whilſt their centers keep their diſtances and poſitions, could be made leſs in diameter, their interfering one with another, and by conſequence the mixture of the heterogeneous rays would be proportionally diminiſhed. In the twenty-third figure let AG, BH, CJ, DK, EL, FM be the circles, which ſo many ſorts of rays, flowing from the ſame diſque of the ſun, do in the third Experiment illuminate; of all which and innumerable other intermediate ones, lying in a continual ſeries between the two rectilinear and parallel edges of the ſun's oblong image PT, that image is compoſed; as was explained in the fifth Experiment. And let *ag*, *bh*, *ci*, *dk*, *el*, *fm*, be ſo many leſs circles, lying in a like continual ſeries between two parallel right lines *af* and *gm*, with the ſame diſtances between their centers, and illuminated by the ſame ſorts of rays; that is, the circle *ag* with the ſame ſort by which the correſponding circle AG was illuminated; and the circle *bh* with the ſame ſort by which the correſponding circle BH was illuminated; and the reſt of the circles, *ci*, *dk*, *el*, *fm*, reſpectively, with the ſame ſorts of rays by which the ſeveral correſponding circles, CJ, DK, EL, FM, were illuminated. In the figure, PT, compoſed of the greater circles, three of thoſe circles, AG, BH, CJ, are ſo expanded into one another, that the three ſorts of rays by which thoſe circles are illuminated, together with other innumerable ſorts of intermediate rays, are mixed at QR in the middle of the circle BH. And the like mixture happens throughout almoſt the whole length of the figure PT. But in the figure, *pt*, compoſed of the leſs circles, the three leſs circles, *ag*, *bh*, *ci*, which anſwer to thoſe three greater, do not extend into one another; nor are there any where mingled ſo much as any two of the three ſorts of rays,

into its conſtituent Parts.

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by which thoſe circles are illuminated, and which in the figure PT are all of them intermingled at BH.

Now he that ſhall thus conſider it, will eaſily underſtand, that the mixture is diminiſhed in the ſame proportion with the diameters of the circles. If the diameters of the circles, whilſt their centers remain the ſame, be made three times leſs than before, the mixture will be alſo three times leſs; if ten times leſs, the mixture will be ten times leſs, and ſo of other proportions. That is, the mixture of the rays in the greater figure, PT, will be to their mixture in the leſs, *pt*, as the latitude of the greater figure is to the latitude of the leſs. For the latitudes of theſe figures are equal to the diameters of their circles. And hence it eaſily follows, that the mixture of the rays in the refracted ſpectrum, *pt*, is to the mixture of the rays in the direct and immediate light of the ſun, as the breadth of that ſpectrum is to the difference between the length and breadth of the ſame ſpectrum.

So then, if we would diminiſh the mixture of the rays, we are to diminiſh the diameters of the circles. Now theſe would be diminiſhed if the ſun's diameter to which they anſwer could be made leſs than it is, or (which comes to the ſame purpoſe) if without doors, at a great diſtance from the priſm towards the ſun, ſome opake body were placed, with a round hole in the middle of it, to intercept all the ſun's light, excepting ſo much as, coming from the middle of his body, could paſs through that hole to the priſm. For ſo the circles AG, BH, and the reſt, would not any longer anſwer to the whole diſque of the ſun, but only to that part of it, which could be ſeen from the priſm through that hole, that it is to the apparent magnitude of that hole viewed from the priſm. But that theſe circles may anſwer more diſtinctly to that hole, a lens is to be placed by the priſm to caſt the image of the hole, (that is, every one of the circles AG, BH, &c.) diſtinctly upon the paper at PT; after ſuch a manner as by a lens, placed at a window, the ſpecies of objects abroad are caſt diſtinctly upon a paper within the room, and the rectilinear ſides of the oblong ſolar image in the fifth Experiment became diſtinct without any penumbra. If this be done, it will not be neceſſary to place that hole very far off, no not beyond the window. And

therefore

therefore inſtead of that hole, I uſed the hole in the window-ſhut as follows. into its conſtituent Parts.

Exper. 11. In the ſun's light let into my darkened chamber, through a ſmall round hole in my window-ſhut, at about ten or twelve feet from the window, I placed a lens, by which the image of the hole might be diſtinctly caſt upon a ſheet of white paper, placed at the diſtance of ſix, eight, ten or twelve feet from the lens. For according to the difference of the lenſes I uſed various diſtances, which I think not worth the while to deſcribe. Then immediately after the lens I placed a priſm, by which the trajected light might be refracted either upwards or ſideways, and thereby the round image, which the lens alone did caſt upon the paper, might be drawn out into a long one with parallel ſides, as in the third Experiment. This oblong image I let fall upon another paper, at about the ſame diſtance from the priſm as before, moving the paper either towards the priſm or from it, until I found the juſt diſtance where the rectilinear ſides of the image became moſt diſtinct. For in this caſe, the circular images of the hole which compoſe that image, after the ſame manner that the circles *ag*, *bh*, *ci*, &c. do the figure *pt* [in *fig.* 23] were terminated moſt diſtinctly without any penumbra, and therefore extended into one another the leaſt that they could, and by conſequence the mixture of the heterogeneous rays was now the leaſt of all. By this means I uſed to form an oblong image (ſuch as is *pt*) [in *fig.* 23 and 24] of circular images of the hole (ſuch as are *ag*, *bh*, *ci*, &c.) and by uſing a greater or leſs hole in the window-ſhut, I made the circular images *ag*, *bh*, *ci*, &c. of which it was formed, to become greater or leſs at pleaſure, and thereby the mixture of the rays in the image *pt* to be as much or as little as I deſired.

Illuſtration. In the twenty-fourth figure, F repreſents the circular hole in the window-ſhut; MN the lens whereby the image or ſpecies of that hole is caſt diſtinctly upon a paper at J; ABC the priſm, whereby the rays are, at their emerging out of the lens, refracted from J towards another paper at *pt*, and the round image at J is turned into an oblong image, *pt*, falling on that other paper. This image, *pt*, conſiſts of circles placed one after

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another in a rectilinear order, as was ſufficiently explained in the fifth Experiment; and theſe circles are equal to the circle J, and conſequently anſwer in magnitude to the hole F; and therefore by diminiſhing that hole they may be at pleaſure diminiſhed, whilſt their centers remain in their places. By this means I made the breadth of the image *pt* to be forty times, and ſometimes ſixty or ſeven times leſs than its length. As for inſtance, if the breadth of the hole F be one tenth of an inch, and MF the diſtance of the lens from the hole be 12 feet; and if *p*B or *p*M the diſtance of the image *pt* from the priſm or lens be 10 feet, and the Refracting angle of the priſm be 62 degrees, the breadth of the image *pt* will be one twelfth of an inch, and the length about ſix inches; and therefore the length to the breadth as 72 to 1, and by conſequence the light of this image 71 times leſs compound than the ſun's direct light. And light thus far ſimple and homogeneal, is ſufficient for trying all the Experiments in this Book about Simple Light. For the compoſition of heterogeneal rays is in this light ſo little, that it is ſcarce to be diſcovered and perceived by ſenſe, except perhaps in the Indigo and Violet. For theſe, being dark colours, do eaſily ſuffer a ſenſible allay by that little ſcattering light, which uſes to be refracted irregularly by the inequalities of the priſm.

Yet inſtead of the circular hole F, it is better to ſubſtitute an oblong hole, ſhaped like a long parallelogram with its length parallel to the priſm ABC. For if this hole be an inch or two long, and but a tenth or twentieth part of an inch broad, or narrower: the light of the image *pt* will be as ſimple as before, or ſimpler, and the image will become much broader, and therefore more fit to have experiments tried in its light than before.

Inſtead of this parallelogram-hole may be ſubſtituted a triangular one of equal ſides, whoſe baſe for inſtance is about the tenth part of an inch, and its height an inch or more. For by this means, if the axis of the priſm be parallel to the perpendicular of the triangle, the image *pt* [in *fig.* 25] will now be formed of equicrural triangles *ag*, *bh*, *ci*, *dk*, *el*, *fm*, &c. and innumerable other intermediate ones anſwering to the triangular hole in ſhape and bigneſs, and lying one after another in a continual ſe-

ries

ries between two parallel lines *af* and *gm*. Theſe triangles are a little intermingled at their baſes, but not at their vertices; and therefore the light on the brighter ſide *af* of the image, where the baſes of the triangles are, is a little compounded, but on the darker ſide *gm* is altogether uncompounded, and in all places between the ſides the compoſition is proportional to the diſtances of the places from that obſcurer ſide *gm*. And having a ſpectrum *pt* of ſuch a compoſition, we may try experiments either in its ſtronger and leſs ſimple light near the ſide *af*, or in its weaker and ſimple light near the other ſide *gm*, as it ſhall ſeem moſt convenient. into its conſtituent Parts.

But in making experiments of this kind the chamber ought to be made as dark as can be, leſt any foreign light mingle itſelf with the light of the ſpectrum *pt*, and render it compound; eſpecially if we would try experiments in the more ſimple light next the ſide *gm* of the ſpectrum; which being fainter, will have a leſs proportion to the foreign light, and ſo, by the mixture of that light, be more troubled and made more compound. The lens alſo ought to be good, ſuch as may ſerve for Optical uſes; and the priſm ought to have a large angle, ſuppoſe of 65 or 70 degrees, and to be well wrought, being made of glaſs free from bubbles and veins, with its ſides not a little convex or concave, as uſually happens, but truly plane, and its poliſh elaborate, as in working Optick-glaſſes, and not ſuch as is uſually wrought with putty; whereby the edges of the ſand-holes being worn away, there are left all over the glaſs a numberleſs company of very little convex polite riſings, like waves. The edges alſo of the priſm and lens, ſo far as they may make any irregular refraction, muſt be covered with a black paper glewed on. And all the light of the ſun's beam let into the chamber, which is uſeleſs and unprofitable to the Experiment, ought to be intercepted with black paper, or other black obſtacles. For otherwiſe the uſeleſs light, being reflected every way in the chamber, will mix with the oblong ſpectrum, and help to diſturb it. In trying theſe things ſo much diligence is not altogether neceſſary, but it will promote the ſucceſs of the Experiments, and by a very ſcrupulous examiner of things deſerves to be applied. It is difficult

to

to get glaſs-priſms fit for this purpoſe, and therefore I uſed ſometimes Priſmatick Veſſels, made with pieces of broken looking-glaſſes, and filled with rain-water. And to increaſe the refraction, I ſometimes impregnated the water ſtrongly with *Saccharum Saturni*.

PROP. V. THEOR. IV.

Homogeneal Light is refracted regularly, without any dilatation, ſplitting, or ſhattering of the rays; and the confuſed Viſion of objects, ſeen through refracting bodies by heterogeneal light, ariſes from the different Refrangibility of ſeveral ſorts of rays.

Regular Refraction of Homogeneal Light.

The firſt part of this Propoſition has been already ſufficiently proved in the fifth Experiment, and will farther appear by the Experiments which follow.

Exper. 12. In the middle of a black paper I made a round hole, about a fifth or ſixth part of an inch in diameter. Upon this paper I cauſed the ſpectrum of Homogeneal Light, deſcribed in the former Propoſition, ſo to fall, that ſome part of the light might paſs through the hole of the paper. This tranſmitted part of the light I refracted with a priſm placed behind the paper, and letting this refracted light fall perpendiculayly upon a white paper, two or three feet diſtant from the priſm, I found that the ſpectrum formed on the paper by this light was not oblong, as when it is made (in the third Experiment) by refracting the ſun's compound light, but was (ſo far as I could judge by my eye) perfectly circular, the length being no greater than the breadth. Which ſhews that this light is refracted regularly without any dilatation of the rays.

Exper. 13. In the Homogeneal Light I placed a paper-circle of a quarter of an inch in diameter, and in the ſun's unrefracted Heterogeneal White Light I placed another paper-circle of the ſame bigneſs. And going from the papers to the diſtance of ſome feet, I viewed both circles through a priſm. The circle illuminated by the ſun's Heterogeneal Light appeared very oblong, as in the fourth Experiment, the length being many times greater than the breadth; but the other circle illuminated with Homogeneal

geneal Light appeared circular and diſtinctly defined, as when it is viewed with the naked eye. Which proves the whole Propoſition.

Exper. 14. In the Homogeneal Light I placed flies, and ſuch like minute objects, and viewing them through a priſm, I ſaw their parts as diſtinctly defined, as if I had viewed them with the naked eye. The ſame objects placed in the ſun's unrefracted Heterogeneal Light, which was white, I viewed alſo through a priſm, and ſaw them moſt confuſedly defined, ſo that I could not diſtinguiſh their ſmaller parts from one another. I placed alſo the letters of a ſmall print one while in the Homogeneal Light, and then in the Heterogeneal; and viewing them through a priſm, they appeared, in the latter caſe, ſo confuſed and indiſtinct that I could not read them; but in the former, they appeared ſo diſtinct that I could read readily, and thought I ſaw them as diſtinct, as when I viewed them with my naked eye. In both caſes I viewed the ſame objects, through the ſame priſm, at the ſame diſtance from me, and in the ſame ſituation. There was no difference but in the light, by which the objects were illuminated, and which in one caſe was ſimple, and in the other compound; and therefore the diſtinct viſion in the former caſe, and confuſed in the latter, could ariſe from nothing elſe than from that difference of the lights. Which proves the whole Propoſition.

And in theſe three Experiments it is farther very remarkable, that the colour of Homogeneal Light was never changed by the refraction.

PROP. VI. THEOR. V.

The ſine of Incidence of every ray conſidered apart, is to its ſine of Refraction in a given ratio.

Given proportions between the ſines of Incidence and Refraction.

That every Ray, conſidered apart, is conſtant to itſelf in ſome degree of refrangibility, is ſufficiently manifeſt out of what has been ſaid. Thoſe rays which in the firſt refraction are, at equal incidences, moſt refracted, are alſo in the following refractions, at equal incidences, moſt refracted; and ſo of the leaſt refrangible, and the reſt which have any mean degree of refrangibili-

Given proportions between the

ty, as is manifeſt by the fifth, ſixth, ſeventh, and eighth, and ninth Experiments. And thoſe which the firſt time, at like incidences, are equally refracted, are again at like incidences equally and uniformly refracted; and that, whether they be refracted before they be ſeparated from one another, as in the fifth Experiment; or whether they be refracted apart, as in the twelfth, thirteenth and fourteenth Experiments. The refraction therefore of every ray apart is regular, and what rule that refraction obſerves we are now to ſhew.

The late Writers in Opticks teach, that the ſines of incidence are in a given proportion to the ſines of refraction, as was explained in the fifth Axion: and ſome, by inſtruments fitted for meaſuring of refractions, or otherwiſe experimentally examining this proportion, do acquaint us that they have found it accurate. But whilſt they, not underſtanding the different refrangibility of ſeveral rays, conceived them all to be refracted according to one and the ſame proportion, it is to be preſumed that they adapted their meaſures only to the middle of the refracted light; ſo that from their meaſures we may conclude only, that the rays which have a mean degree of refrangibility, that is thoſe which, when ſeparated from the reſt, appear Green, are refracted according to a given proportion of their ſines. And therefore we are now to ſhew that the like given proportions obtain in all the reſt. That it ſhould be ſo is very reaſonable, Nature being ever conformable to herſelf: but an experimental proof is deſired. And ſuch a proof will be had, if we can ſhew that the ſines of refraction of rays differently refrangible are one to another in a given proportion, when their ſines of incidence are equal. For if the ſines of refraction of all the rays are in given proportions to the ſine of refraction of a ray which has a mean degree of refrangibility, and this ſine is in a given proportion to the equal ſines of incidence, thoſe other ſines of refraction will alſo be in given proportions to the equal ſines of incidence. Now when the ſines of incidence are equal; it will appear by the following Experiment that the ſines of refraction are in a given proportion to one another.

Exper.

Exper. 15. The ſun ſhining into a dark chamber through a little round hole in the window-ſhut, let s [in *fig.* 26.] repreſent his round white image painted on the oppoſite wall by his direct light; PT, his oblong coloured image made by refracting that light with a priſm placed at the window; and *pt*, or 2*p*2*t*, or 3*p*3*t*, his oblong coloured image made by refracting again the ſame light ſideways, with a ſecond priſm placed immediately after the firſt in a croſs poſition to it, as was explained in the fifth Experiment: that is to ſay, *pt* when the refraction of the ſecond priſm is ſmall, 2*p*2*t* when its refraction is greater, and 3*p*3*t* when it is greateſt. For ſuch will be the diverſity of the refractions if the Refracting Angle of the ſecond priſm be of various magnitudes; ſuppoſe of fifteen or twenty degrees, to make the image *pt*; of thirty or forty, to make the image 2*p*2*t*; and of ſixty, to make the image 3*p*3*t*. But for want of ſolid glaſs-priſms with angles of convenient bigneſſes, there may be veſſels made of poliſhed plates of glaſs, cemented together in the form of priſms, and filled with water. Theſe things being thus ordered, I obſerved that all the ſolar images or coloured ſpectrums PT, *pt*, 2*p*2*t*, 3*p*3*t* did very nearly converge to the place, s, on which the direct light of the ſun fell, and painted his white round image, when the priſms were taken away. The axis of the ſpectrum PT, that is the line drawn through the middle of it parallel to its rectilinear ſides, did, when produced, paſs exactly through the middle of that white round image s. And when the refraction of the ſecond priſm was equal to the refraction of the firſt, the Refracting Angle of them both being about 60 degrees, the axis of the ſpectrum 3*p*3*t* made by that refraction, did, when produced, paſs alſo through the middle of the ſame white round image s. But when the refraction of the ſecond priſm was leſs than that of the firſt, the produced axes of the ſpectrums, *tp*, or 2*t*2*p*, made by that refraction did cut the produced axis of the ſpectrum TP in the points *m* and *n*, a little beyond the center of that white round Image s. Whence the proportion of the line 3*t*T to the line 3*p*P was a little greater than the proportion of 2*t*T to 2*p*P, and this proportion a little greater than that of *t*T to *p*P. Now when the light of the ſpectrum PT falls perpendicularly

Lines of Incidence and Refraction.

Given proportions between the

upon the wall, thoſe lines $3tT$, $3pP$, and $2tT$, $2pP$ and tT, pP, are the tangents of the refractions; and therefore by this Experiment the proportions of the tangents of the refractions are obtained, from whence the proportions of the ſines being derived, they come out equal, ſo far as by viewing the ſpectrums, and uſing ſome mathematical reaſoning, I could eſtimate. For I did not make an accurate computation. So then the Propoſition holds true in every ray apart, ſo far as appears by Experiment. And that it is accurately true, may be demonſtrated upon this ſuppoſition. *That bodies refract light by acting upon its rays in lines perpendicular to their ſurfaces.* But in order to this demonſtration, I muſt diſtinguiſh the motion of every ray into two motions, the one perpendicular to the refracting ſurface, the other parallel to it, and concerning the perpendicular motion lay down the following Propoſition.

If any motion or moving thing whatſoever be incident, with any velocity, on any broad and thin ſpace, terminated on both ſides by two parallel planes; and, in its paſſage through that ſpace, be urged perpendicularly towards the farther plane, by any force which at given diſtances from the plane is of given quantities; the perpendicular velocity of that motion or thing, at its emerging out of that ſpace, ſhall be always equal to the ſquare root of the ſum of the ſquare of the perpendicular velocity of that motion or thing, at its incidence on that ſpace, and of the ſquare of the perpendicular velocity which that motion or thing would have at its emergence, if, at its incidence, its perpendicular velocity was infinitely little.

And the ſame Propoſition holds true of any motion or thing perpendicularly retarded in its paſſage through that ſpace, if inſtead of the ſum of the two ſquares you take their difference. The demonſtration Mathematicians will eaſily find out, and therefore I ſhall not trouble the reader with it (g).

Suppoſe now that a ray coming moſt obliquely in the line MC [in

(g) See the figure of the xcvth Propoſition of the firſt book of the Principia.
In that figure AH and Id are equal. Alſo TH and IK. GH and IK are as the whole velocities of the incident and emergent ray. Therefore GA and Kd are as the perpendicular velocities of incidence and emergence. But $Kd = \sqrt{IK^2 - Id^2} = \sqrt{TH^2 - AH^2} = \sqrt{TH^2 - GH^2 + GA^2}$. Hence if GA,

[in *fig.* 1.] be refracted at C by the plane RS into the line CN; and if it be required to find the line CE, into which any other ray AC ſhall be refracted, let MC, AD, be the ſines of incidence of the two rays, and NG, EF, their ſines of refraction; and let the equal motions of the incident rays be repreſented by the equal lines MC and AC; and the motion MC being conſidered as parallel to the refracting plane, let the other motion AC be diſtinguiſhed into two motions AD and DC; one of which AD is parallel, and the other DC perpendicular to the refracting ſurface. In like manner, let the motions of the emerging rays be diſtinguiſhed into two; whereof the perpendicular ones are $\frac{MC}{NG}$ CG and $\frac{AD}{EF}$ CF. And if the force of the refracting plane begins to act upon the rays either in that plane, or at a certain diſtance from it on the one ſide, and ends at a certain diſtance from it on the other ſide, and in all places between thoſe two limits acts upon the rays in lines perpendicular to that refracting plane, and the actions upon the rays at equal diſtances from the refracting plane be equal, and at unequal ones either equal or unequal according to any rate whatever: that motion of the ray, which is parallel to the refracting plane, will ſuffer no alteration by that force; and that motion, which is perpendicular to it, will be altered according to the rule of the foregoing Propoſition. If therefore for the perpendicular velocity of the emerging ray, CN, you write $\frac{MC}{NG}$ CG, as above; then the perpendicular velocity of any other emerging ray, CE, which was $\frac{AD}{EF}$ CF, will be equal to the ſquare root of $CDq + \frac{MCq}{NGq}$ CGq. And by ſquaring theſe equals, and adding to them the equals ADq and MCq − CDq, and dividing the ſums by the equals CFq + EFq, and CGq + NGq, you will have $\frac{ADq}{EFq}$ equal to $\frac{MCq}{NGq}$. Whence AD, the ſine of incidence, is to EF the ſine of refraction, as MC to NG, that is, in a given *ratio*. And this demonſtration being general, without determining what light is, or by what kind of force it is refracted, or aſſuming any thing farther than that the refracting

Sines of Incidence and Refraction.

GA, the perpendicular velocity of incidence, be gradually diminiſhed till it be reduced to nothing; K*d*, the perpendicular velocity of emergence, becomes ultimately equal to $\sqrt{TH^2 - GH^2}$. And in any given magnitude of GA, K*d* is equal to the ſquare root of the ſum of the ſquare of that its ultimate quantity and the ſquare of GA. And this is what Newton here affirms.

body acts upon the rays in lines perpendicular to its ſurface; I take it to be a very convincing argument of the full truth of this Propoſition ([h]).

So then, if the *ratio* of the ſines of incidence and refraction of any ſort of rays be found in any one caſe, it is given in all caſes; and this may be readily found by the method in the following Propoſition.

PROP. VII. THEOR. VI.

The perfection of Teleſcopes is impeded by the different refrangibility of the rays of light.

The imperfection of Teleſcopes is vulgarly attributed to the Spherical Figures of the glaſſes, and therefore Mathematicians have propounded to figure them by the Conical Sections. To ſhew that they are miſtaken, I have inſerted this Propoſition; the truth of which will appear by the meaſures of the refractions of the ſeveral ſorts of rays; and theſe meaſures I thus determine.

To meaſure the different Refractions of the extreme Rays.

In the third Experiment of this firſt Part, where the Refracting Angle of the priſm was $62\frac{1}{2}$ degrees, the half of that angle 31 deg. 15 min. is the angle of incidence of the rays, at their going out of the glaſs into the air ([i]); and the ſine of this angle is 5188, the radius being 10000. When the axis of this priſm was parallel to the horizon, and the refraction of the rays, at their incidence on this priſm, equal to that at their emergence out of it, I obſerved with a quadrant the angle, which the mean refrangible rays (that is, thoſe which went to the middle of the ſun's coloured image) made with the horizon; and by this angle and the ſun's altitude obſerved at the ſame time, I found the angle which the emergent rays contained with the incident to be 44 deg. and 40 min. and the half of this angle added to the angle of incidence 31 deg. 15 min. makes the angle of refraction ([k]), which is therefore 53 deg. 35 min. and its ſine 8047. Theſe are the ſines

([h]) This Propoſition is the xcivth of the firſt book of the Principia, where it is more elegantly proved.

([i]) Vide Lect. Opt. Part I. § 31. Lem. I.

of

of incidence and refraction of the mean refrangible rays, and their proportion in round numbers is 20 to 31. This glaſs was of a colour inclining to green. The laſt of the priſms mentioned in the third Experiment was of clear white glaſs. Its Refracting Angle $63\frac{1}{2}$ degrees. The angle, which the emergent rays contained with the incident, 45 deg. 50 min. The ſine of half the firſt angle 5262. The ſine of half the ſum of the angles 8157. And their proportion in round numbers 20 to 31, as before.

To meaſure the different Refractions of the extreme Rays.

From the length of the image, which was about $9\frac{3}{4}$ or 10 inches, ſubduct its breadth, which was $2\frac{1}{8}$ inches, and the remainder $7\frac{3}{4}$ inches would be the length of the image, were the ſun but a point; and therefore ſubtends the angle which the moſt and leaſt refrangible rays, when incident on the priſm in the ſame lines, do contain with one another after their emergence. Whence this angle is 2 deg. 0′. 7″. For the diſtance between the image and the priſm, where this angle is made, was $18\frac{1}{2}$ feet, and at that diſtance the chord, $7\frac{3}{4}$ inches, ſubtends an angle of 2 deg. 0′. 7″. Now half this angle is the angle which theſe emergent rays contain with the emergent mean refrangible rays, and a quarter thereof, that is 30′. 2″, may be accounted the angle which they would contain with the ſame emergent mean refrangible rays, were they co-incident to them within the glaſs, and ſuffered no other refraction than that at their emergence. For if two equal refractions, the one at the incidence of the rays on the priſm, the other at their emergence, make half the angle 2 deg. 0′. 17″; then one of thoſe refractions will make about a quarter of that angle; and this quarter added to and ſubducted from the angle of refraction of the mean refrangible rays, which was 53 deg. 35′, gives the angles of refraction of the moſt and leaſt refrangible rays 54 deg. 5′. 2″, and 53 deg. 4′. 58″, whoſe ſines are 8099 and 7995, the common angle of incidence being 31 deg. 15′, and its ſine 5188; and theſe ſines, in the leaſt round numbers, are in proportion to one another, as 78 and 77 to 50 ([l]).

([k]) Lect. Opt. Part I. § 31. Lem. II.
([l]) Lect. Opt. Part I. § 32.

Now

Now if you ſubduct the common ſine of incidence, 50, from the ſines of refraction, 77 and 78; the remainders, 27 and 28, ſhew that in ſmall refractions the refraction of the leaſt refrangible rays is to the refraction of the moſt refrangible ones as 27 to 28 very nearly, and that the difference of the refractions of the leaſt refrangible and moſt refrangible rays is about the $27\frac{1}{2}$th part of the whole refraction of the mean refrangible rays.

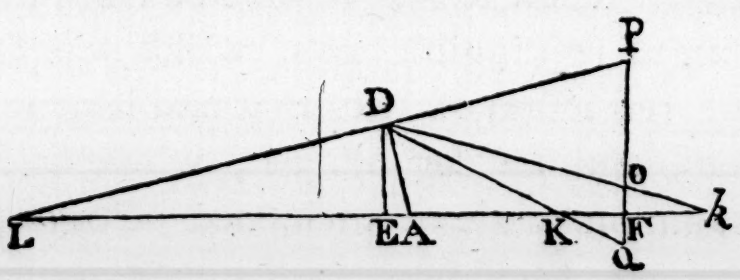
Imperfection of Teleſcopes.

Whence they that are skilled in Opticks will eaſily underſtand, that the breadth of the leaſt circular ſpace, into which object-glaſſes of teleſcopes can collect all ſorts of parallel rays, is about the $27\frac{1}{2}$th part of half the aperture of the glaſs, or 55th part of the whole aperture (m); and that the focus of the moſt refrangible rays is nearer to the object-glaſs than the focus of the leaſt refrangible ones, by about the $27\frac{1}{2}$th part of the diſtance between the object-glaſs and the focus of the mean refrangible ones.

And if rays of all ſorts, flowing from any one lucid point in the axis of any convex lens, be made, by the refraction of the lens, to converge to points not too remote from the lens, the focus of the moſt refrangible rays ſhall be nearer to the lens, than the focus of the leaſt refrangible ones, by a diſtance which is to the

(m) Lect. Opt. Part I. Prop. xxxvii.

(n) Imagine that Heterogeneal Rays, flowing from a lucid point L, in LA the axis of a ſpherical lens AD, fall upon the edge of the lens at D; and that they are refracted by the lens ſo as to meet the axis, the moſt refrangible in K, the leaſt refrangible in *k*, and thoſe of the mean refrangibility in F. Newton affirms that K*k* is to $\frac{1}{27.5}$ FA as LF to LA, very nearly; provided the diſtance FA be not too great. And this we thus demonſtrate. From the point of incidence, D, draw DE perpendicular to the axis of the lens. Through F draw FP parallel with DE; and let FP meet the direct ray, LD, produced in P, and the refracted rays, D*k* and DK, in o and Q. Now if the diſtance FA be not vaſtly great, KF and *k*F will be very nearly equal; alſo OF and FQ; oQ will be the leaſt breadth of the circular ſpace, in which the lens DA collects the heterogeneal rays emitted from L, and FO or FQ half that leaſt breadth. (Lect. Opt. Part I. Prop. xxxvii.) But becauſe the lines FO and DE are parallel, therefore *k*F : *k*E = FO : DE. Alſo KF : KE = FQ or FO : DE. Therefore *k*F + KF or K*k* : *k*E + KE = FO : DE. But KF and *k*F being equal, *k*E + KE is twice FE. And if the lens DA be but a ſmall portion of its ſphere, twice FE will not ſenſibly exceed twice FA. Hence K*k* : 2FA = FO : DE very nearly. And by conſequence K*k* : FA = 2FO or oQ : DE. But oQ bears to DE a proportion



the $27\frac{1}{2}$th part of the diſtance of the focus of the mean refrangible rays from the lens, as the diſtance between that focus and the lucid point from whence the rays flow, is to the diſtance between that lucid point and the lens very nearly (n). of Teleſcopes.

Now to examine whether the difference between the refractions, which the moſt refrangible and the leaſt refrangible rays, flowing from the ſame point, ſuffer in the object-glaſſes of teleſcopes and ſuch like glaſſes, be ſo great as is here deſcribed, I contrived the following Experiment.

Exper. 16. The lens which I uſed in the ſecond and eighth Experiments, being placed ſix feet and an inch diſtant from any object, collected the ſpecies of that object, by the mean refrangible rays, at the diſtance of ſix feet and an inch from the lens on the other ſide. And therefore by the foregoing rule it ought to collect the ſpecies of that object, by the leaſt refrangible rays, at the diſtance of ſix feet and $3\frac{2}{3}$ inches from the lens; and by the moſt refrangible ones, at the diſtance of five feet and $10\frac{1}{3}$ inches from it: ſo that between the two places, where theſe leaſt and moſt refrangible rays collect the ſpecies, there may be the diſtance of about $5\frac{1}{3}$ inches. For by that rule, as ſix feet and an inch (the diſtance of the lens from the lucid object) is to twelve feet and two inches (the diſtance of the lucid object from the focus of the mean refrangible rays) that is, as one is to two, ſo is

portion compounded of the proportions of OQ to FP, and of FP to DE; i. e. of the proportions of OQ to FP, and of LF to LE. But taking the proportions of the ſines of refraction of the ſeveral rays, which are refrangible in the greateſt, leaſt, and mean degree, as they are here ſtated, it follows, from what is ſhewn in the XXXVIIth Prop. of Lect. Opt. Part I, that OQ bears to FP the proportion of 1 to 27,5. Therefore the proportion of OQ to DE is compounded of the proportions of 1 to 27,5, and of LF to LE. And the proportion of Kk to FA being the ſame, or very nearly the ſame with that of OQ to DE, is compounded of theſe ſame proportions. That is, $Kk : FA = 1 : 27,5 + LF : LE$. But again, if the lens DA be a very ſmall portion of a ſphere, LE will not be ſenſibly leſs than LA, and the proportions of LF to LE and LA will not be ſenſibly different. Hence $Kk : FA = 1 : 27,5 + LF : LA$ very nearly. That is, $Kk : FA = LF : 27,5$ LA very nearly. By equality $Kk : \frac{1}{27,5} FA = LF : LA$ very nearly. Q. E. D.

If the diſtance FA be ſo great, that KF and Fk become ſenſibly unequal; the proportion of Kk to FA, here ſtated, will no longer ſubſiſt.

Cor. If the lucid point be removed farther and farther from the lens; the proportion of LF and LA, and of conſequence that of Kk and $\frac{1}{27,5}$ FA, approaches to equality; and ultimately, when the ray LD becomes parallel to the axis, LF, LA become equal, and $Kk = \frac{1}{27,5}$ FA, juſt as Newton affirms.

Imperfection the $27\frac{1}{2}$th part of ſix feet and an inch (the diſtance between the lens and the ſame focus) to the diſtance between the focus of the moſt refrangible rays and the focus of the leaſt refrangible ones; which is therefore $5\frac{17}{55}$ inches, that is very nearly $5\frac{1}{3}$ inches. Now to know whether this meaſure was true, I repeated the ſecond and eighth Experiment with coloured light, which was leſs compounded than that I there made uſe of: for I now ſeparated the heterogeneous rays one from another, by the method I deſcribed in the eleventh Experiment; ſo as to make a coloured ſpectrum about twelve or fifteen times longer than broad. This ſpectrum I caſt on a printed book, and placing the abovementioned lens at the diſtance of ſix feet and an inch from this ſpectrum, to collect the ſpecies of the illuminated letters at the ſame diſtance on the other ſide; I found that the ſpecies of the letters illuminated with Blue were nearer to the lens, than thoſe illuminated with deep Red, by about three inches, or three and a quarter: but the ſpecies of the letters illuminated with Indigo and Violet appeared ſo confuſed and indiſtinct, that I could not read them. Whereupon viewing the priſm, I found it was full of veins running from one end of the glaſs to the other; ſo that the refraction could not be regular. I took another priſm therefore which was free from veins; and inſtead of the letters I uſed two or three parallel black lines, a little broader than the ſtroaks of the letters; and caſting the colours upon theſe lines, in ſuch manner that the lines ran along the colours from one end of the ſpectrum to the other; I found the focus where the Indigo, or confine of this colour and Violet, caſt the ſpecies of the black lines moſt diſtinctly, to be about four inches or $4\frac{1}{4}$ nearer to the lens, than the focus where the deepeſt Red caſt the ſpecies of the ſame black lines moſt diſtinctly. The Violet was ſo faint and dark, that I could not diſcern the ſpecies of the lines diſtinctly by that colour; and therefore conſidering that the priſm was made of a dark coloured glaſs inclining to Green, I took another priſm of clear white glaſs; but the ſpectrum of colours, which this priſm made, had long white ſtreams of faint light ſhooting out from both ends of the colours; which made me conclude that ſomething was amiſs: and viewing the priſm, I found two or three little

little bubbles in the glaſs which refracted the light irregularly. Wherefore I covered that part of the glaſs with black paper; and letting the light paſs through another part of it, which was free from ſuch bubbles, the ſpectrum of colours became free from thoſe irregular ſtreams of light, and was now ſuch as I deſired. But ſtill I found the Violet ſo dark and faint, that I could ſcarce ſee the ſpecies of the lines by the Violet, and not at all by the deepeſt part of it, which was next the end of the ſpectrum. I ſuſpected therefore that this faint and dark colour might be allayed by that ſcattering light, which was refracted and reflected irregularly, partly by ſome very ſmall bubbles in the glaſſes, and partly by the inequalities of their poliſh: which light, though it was but little, yet it being of a White colour, might ſuffice to affect the ſenſe ſo ſtrongly, as to diſturb ths phænomena of that weak and dark colour the Violet; and therefore I tried, as in the 12th, 13th and 14th Experiments, whether the light of this colour did not conſiſt of a ſenſible mixture of heterogeneous rays, but found it did not. Nor did the refractions cauſe any other ſenſible colour than Violet to emerge out of this light; as they would have done out of White light, and by conſequence out of this Violet Light, had it been ſenſibly compounded with White Light. And therefore I concluded, that the reaſon why I could not ſee the ſpecies of the lines diſtinctly by this colour, was only the darkneſs of this colour and thinneſs of its light, and its diſtance from the axis of the lens; I divided therefore thoſe parallel black lines into equal parts, by which I might readily know the diſtances of the colours in the ſpectrum from one another; and noted the diſtances of the lens from the foci of ſuch colours as caſt the ſpecies of the lines diſtinctly: and then conſidered, whether the difference of thoſe diſtances bear ſuch proportion to $5\frac{1}{3}$ inches, the greateſt difference of the diſtances which the foci of the deepeſt Red and Violet ought to have from the lens, as the diſtance of the obſerved colours from one another in the ſpectrum bear to the greateſt diſtance of the deepeſt Red and Violet meaſured in the rectilinear ſides of the ſpectrum; that is, to the length of thoſe ſides, or exceſs of the length of the ſpectrum above its breadth. And my obſervations were as follows. of Teleſcopes.

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When I obſerved and compared the deepeſt ſenſible Red, and the colour in the confine of Green and Blue, which at the rectilinear ſides of the ſpectrum was diſtant from it half the length of thoſe ſides; the focus where the confine of Green and Blue caſt the ſpecies of the lines diſtinctly on the paper, was nearer to the lens, than the focus where the Red caſt thoſe lines diſtinctly on it, by about $2\frac{1}{2}$ or $2\frac{3}{4}$ inches. For ſometimes the meaſures were a little greater, ſometimes a little leſs, but ſeldom varied from one another above $\frac{1}{5}$ of an inch. For it was very difficult to define the places of the foci, without ſome little errors. Now if the colours diſtant half the length of the image (meaſured at its rectilinear ſides) give $2\frac{1}{2}$ or $2\frac{3}{4}$ difference of the diſtances of their foci from the lens, then the colours diſtant the whole length ought to give 5 or $5\frac{1}{2}$ inches difference of thoſe diſtances.

But here it is to be noted, that I could not ſee the Red to the full end of the ſpectrum, but only to the center of the ſemicircle which bounded that end, or a little farther; and therefore I compared this Red not with that colour which was exactly in the middle of the ſpectrum, or confine of Green and Blue, but with that which converged a little more to the Blue than to the Green. And as I reckoned the whole length of the colours not to be the whole length of the ſpectrum, but the length of its rectilinear ſides, ſo completing the ſemicircular ends into circles, when either of the obſerved colours fell within thoſe circles, I meaſured the diſtance of that colour from the ſemicircular end of the ſpectrum; and ſubducting half this diſtance from the meaſured diſtance of the two colours, I took the remainder for their corrected diſtance; and in theſe obſervations ſet down this corrected diſtance for the difference of the diſtances of their foci from the lens. For as the length of the rectilinear ſides of the ſpectrum would be the whole length of all the colours, were the circles, of which (as we ſhewed) that ſpectrum conſiſts, contracted and reduced to phyſical points; ſo in that caſe this corrected diſtance would be the real diſtance of the two obſerved colours.

When therefore I farther obſerved the deepeſt ſenſible Red, and that Blue whoſe corrected diſtance from it was $\frac{7}{12}$ parts of the length of the rectilinear ſides of the ſpectrum, the difference of the

the diſtances of their foci from the lens was about $3\frac{1}{4}$ inches: and as 7 to 12 ſo is $3\frac{1}{4}$ to $5\frac{4}{7}$. of Teleſcopes.

When I obſerved the deepeſt ſenſible Red, and that Indigo whoſe corrected diſtance was $\frac{8}{12}$ or $\frac{2}{3}$ of the length of the rectilinear ſides of the ſpectrum; the difference of the diſtances of their foci from the lens was about $3\frac{2}{3}$ inches: and as 2 to 3 ſo is $3\frac{2}{3}$ to $5\frac{1}{2}$.

When I obſerved the deepeſt ſenſible Red, and that deep Indigo whoſe corrected diſtance from one another was $\frac{9}{12}$ or $\frac{3}{4}$ of the length of the rectilinear ſides of the ſpectrum; the difference of the diſtances of their foci from the lens was about 4 inches: and as 3 to 4 ſo is 4 to $5\frac{1}{3}$.

When I obſerved the deepeſt ſenſible Red, and that part of the Violet next the Indigo, whoſe corrected diſtance from the Red was $\frac{10}{12}$ or $\frac{5}{6}$ of the length of the rectilinear ſides of the ſpectrum; the difference of the diſtances of their foci from the lens was about $4\frac{1}{2}$ inches: and as 5 to 6, ſo is $4\frac{1}{2}$ to $5\frac{2}{5}$. For ſometimes when the lens was advantageouſly placed, ſo that its axis reſpected the Blue, and all things elſe were well ordered, and the ſun ſhone clear, and I held my eye very near to the paper on which the lens caſt the ſpecies of the lines, I could ſee pretty diſtinctly the ſpecies of thoſe lines by that part of the Violet which was next the Indigo; and ſometimes I could ſee them by above half the Violet. For in making theſe Experiments I had obſerved, that the ſpecies of thoſe colours only appear diſtinct, which were in or near the axis of the lens: ſo that if the Blue or Indigo were in the axis, I could ſee their ſpecies diſtinctly; and then the Red appeared much leſs diſtinct than before. Wherefore I contrived to make the ſpectrum of colours ſhorter than before; ſo that both its ends might be nearer to the axis of the lens. And now its length was about $2\frac{1}{2}$ inches, and breadth about $\frac{1}{5}$ or $\frac{1}{6}$ of an inch. Alſo inſtead of the black lines on which the ſpectrum was caſt, I made one black line broader than thoſe, that I might ſee its ſpecies more eaſily; and this line I divided by ſhort croſs lines into equal parts, for meaſuring the diſtances of the obſerved colours. And now I could ſometimes ſee the ſpecies of this line with its diviſions almoſt as far as the center

of

Imperfection of the ſemicircular Violet end of the ſpectrum, and made theſe farther obſervations.

When I obſerved the deepeſt ſenſible Red, and that part of the Violet whoſe corrected diſtance from it was about $\frac{8}{9}$ parts of the rectilinear ſides of the ſpectrum; the difference of the diſtances of the foci of thoſe colours from the lens, was one time $4\frac{2}{3}$, another time $4\frac{3}{4}$, another time $4\frac{7}{8}$ inches: and as 8 to 9, ſo are $4\frac{2}{3}$, $4\frac{3}{4}$, $4\frac{7}{8}$, to $5\frac{1}{4}$, $5\frac{11}{32}$, $5\frac{31}{64}$ reſpectively.

When I obſerved the deepeſt ſenſible Red, and deepeſt ſenſible Violet (the corrected diſtance of which colours when all things were ordered to the beſt advantage, and the ſun ſhone very clear, was about $\frac{11}{12}$ or $\frac{15}{16}$ parts of the length of the rectilinear ſides of the coloured ſpectrum) I found the difference of the diſtances of their foci from the lens ſometimes $4\frac{3}{4}$, ſometimes $5\frac{1}{4}$, and for the moſt part 5 inches, or thereabouts: and as 11 to 12, or 15 to 16, ſo is 5 inches to $5\frac{1}{2}$ or $5\frac{1}{3}$ inches.

And by this progreſſion of Experiments I ſatisfied myſelf, that had the light at the very ends of the ſpectrum been ſtrong enough to make the ſpecies of the black lines appear plainly on the paper, the focus of the deepeſt Violet would have been found nearer to the lens, than the focus of the deepeſt Red, by about $5\frac{1}{3}$ inches at leaſt. And this is a farther evidence, that the ſines of incidence and refraction of the ſeveral ſorts of rays, hold the ſame proportion to one another in the ſmalleſt refractions which they do in the greateſt.

My progreſs in making this nice and troubleſome Experiment I have ſet down more at large, that they, that ſhall try it after me, may be aware of the circumſpection requiſite to make it ſucceed well. And if they cannot make it ſucceed ſo well as I did, they may notwithſtanding collect, by the proportion of the diſtance of the colours of the ſpectrum to the difference of the diſtances of their foci from the lens, what would be the ſucceſs in the more diſtant colours by a better trial. And yet if they uſe a broader lens than I did, and fix it to a long ſtraight ſtaff, by means of which it may be readily and truly directed to the colour whoſe focus is deſired; I queſtion not but the Experiment will ſucceed better with them, than it did with me. For I directed the axis

as

as nearly as I could to the middle of the colours; and then the faint ends of the ſpectrum, being remote from the axis, caſt their ſpecies leſs diſtinctly on the paper, than they would have done, had the axis been ſucceſſively directed to them. of Teleſcopes.

Now by what has been ſaid, it is certain that the rays which differ in refrangibility do not converge to the ſame focus; but if they flow from a lucid point, as far from the lens on one ſide as their foci are on the other, the focus of the moſt refrangible rays ſhall be nearer to the lens than that of the leaſt refrangible, by above the fourteenth part of the whole diſtance: and if they flow from a lucid point, ſo very remote from the lens, that before their incidence they may be accounted parallel, the focus of the moſt refrangible rays ſhall be nearer to the lens than the focus of the leaſt refrangible, by about the 27th or 28th part of their whole diſtance from it. And the diameter of the circle in the middle ſpace between thoſe two foci, which they illuminate when they fall there on any plane perpendicular to the axis (which circle is the leaſt into which they can all be gathered) is about the 55th part of the diameter of the aperture of the glaſs. So that it is a wonder, that Teleſcopes repreſent objects ſo diſtinct as they do. But were all the rays of light equally refrangible, the error ariſing only from the ſphericalneſs of the figures of glaſſes would be many hundred times leſs. For if the object-glaſs of a Teleſcope be plano-convex, and the plane ſide be turned towards the object, and the diameter of the ſphere whereof this glaſs is a ſegment be called D, and the ſemidiameter of the aperture of the glaſs be called S, and the ſine of incidence out of glaſs into air, be to the ſine of refraction as I to R: the rays, which come parallel to the axis of the glaſs, ſhall, in the place where the image of the object is moſt diſtinctly made, be ſcattered all over a little circle, whoſe diameter is $\frac{Rq}{Iq} \times \frac{S\text{ cub.}}{D\text{ quad.}}$ very nearly; as I gather by computing the errors of the rays by the method of Infinite Series, and rejecting the terms whoſe quantities are inconſiderable (°). As for inſtance, if the ſine of incidence, I, be to the ſine of refraction, R, as 20 to 31; and if D,

(°) Vide Lect. Opt. Part I. Sect. IV. Prop. XXXI. Cor. 7.

the

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But you will ſay, if the errors cauſed by the different refrangibility be ſo very great, how comes it to paſs that objects appear through Teleſcopes ſo diſtinct as they do? I anſwer, it is becauſe the erring rays are not ſcattered uniformly over all that circular ſpace, but collected infinitely more denſely in the center than in any other part of the circle; and in the way from the center to the circumference grow continually rarer and rarer, ſo as at the circumference to become infinitely rare; and by reaſon of their rarity are not ſtrong enough to be viſible, unleſs in the center, and very near it. Let ADE (in *fig.* 27.) repreſent one of thoſe circles, deſcribed with the center C and ſemidiameter AC; and let BFG be a ſmaller circle concentrick to the former, cutting with its circumference the diameter AC in B; and biſect AC in N: and, by my reckoning, the denſity of the light in any place B will be to its denſity in N, as AB to BC; and the whole light, within the leſſer circle BFG, will be to the whole light within the greater AED, as the exceſs of the ſquare of AC above the ſquare of AB is to the ſquare of AC. As if BC be the fifth part of AC, the light will be four times denſer in B than in N; and the whole light within the leſs circle, will be to the whole light within the greater, as nine to twenty-five. Whence it is evident that the light within the leſs circle, muſt ſtrike the ſenſe much more ſtrongly, than that faint and dilated light round about, between it and the circumference of the greater.

But

of TELESCOPES.

But it is farther to be noted, that the moſt luminous of the Priſmatick Colours are the Yellow and Orange. Theſe affect the ſenſes more ſtrongly than all the reſt together, and next to theſe in ſtrength are the Red and Green. The Blue, compared with theſe, is a faint and dark colour; and the Indigo and Violet are much darker and fainter: ſo that theſe, compared with the ſtronger colours, are little to be regarded. The images of objects are therefore to be placed, not in the focus of the mean refrangible rays, which are in the confine of Green and Blue, but in the focus of thoſe rays which are in the middle of the Orange and Yellow; there, where the colour is moſt luminous and fulgent: that is, in the brighteſt Yellow, that Yellow which inclines more to Orange than to Green. And by the refraction of theſe rays (whoſe ſines of incidence and refraction in glaſs are as 17 and 11) the refraction of glaſs and cryſtal for optical uſes is to be meaſured. Let us therefore place the image of the object in the focus of theſe rays, and all the Yellow and Orange will fall within a circle, which diameter is about the 250th part of the diameter of the aperture of the glaſs. And if you add the brighter half of the Red (that half which is next the Orange) and the brighter half of the Green (that half which is next the Yellow) about three-fifth-parts of the light of theſe two colours will fall within the ſame circle, and two fifth-parts will fall without it round about; and that which falls without will be ſpread through almoſt as much more ſpace as that which falls within, and ſo in the groſs be almoſt three times rarer. Of the other half of the Red and Green (that is of the deep Dark Red and Willow-Green) about one quarter will fall within this circle, and three quarters without; and that which falls without will be ſpread through about four or five times more ſpace than that which falls within, and ſo in the groſs be rarer; and if compared with the whole light within it, will be about 25 times rarer than all that taken in the groſs; or rather more than 30 or 40 times rarer: becauſe the deep Red, in the end of the ſpectrum of colours made by a priſm, is very thin and rare, and the Willow-Green is ſomething rarer than the Orange and Yellow. The light of theſe colours therefore being ſo very much rarer than that within the circle, will

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ſcarce affect the ſenſe; eſpecially ſince the deep Red and Willow Green of this light, are much darker colours than the reſt. And for the ſame reaſon the Blue and Violet, being much darker colours than theſe, and much more rarified, may be neglected. For the denſe and bright light of the circle, will obſcure the rare and weak light of theſe dark colours round about it, and render them almoſt inſenſible. The ſenſible image of a lucid point is therefore ſcarce broader than a circle, whoſe diameter is the 250th part of the diameter of the aperture of the object-glaſs of a good teleſcope; or not much broader, if you except a faint and dark miſty light round about it, which a ſpectator will ſcarce regard. And therefore in a teleſcope whoſe aperture is four inches, and length an hundred feet, it exceeds not $2''.45'''$, or $3''$. And in a teleſcope whoſe aperture is two inches, and length 20 or 30 feet, it may be $5''$ or $6''$, and ſcarce above. And this anſwers well to experience: for ſome aſtronomers have found the diameters of the fixed ſtars, in teleſcopes of between 20 and 60 feet in length, to be about $5''$ or $6''$, or at moſt $8''$ or $10''$ in diameter. But if the eye-glaſs be tinged faintly with the ſmoke of a lamp or torch, to obſcure the light of the ſtar, the fainter light in the circumference of the ſtar ceaſes to be viſible, and the ſtar (if the glaſs be ſufficiently ſoiled with ſmoke) appears ſomething more like a mathematical point. And for the ſame reaſon, the enormous part of the light in the circumference of every lucid point ought to be leſs diſcernible in ſhorter teleſcopes than in longer, becauſe the ſhorter tranſmit leſs light to the eye.

Now that the fixed ſtars, by reaſon of their immenſe diſtance, appear like points, unleſs ſo far as their light is dilated by refraction, may appear from hence; that when the moon paſſes over them and eclipſes them, their light vaniſhes, not gradually like that of the planets, but all at once; and in the end of the eclipſe it returns into ſight all at once, or certainly in leſs time than the ſecond of a minute; the refraction of the moon's atmoſphere a little protracting the time in which the light of the ſtar firſt vaniſhes, and afterwards returns into ſight.

Now if we ſuppoſe the ſenſible image of a lucid point, to be even 250 times narrower than the aperture of the glaſs: yet this image

image would be ſtill much greater, than if it were only from the ſpherical figure of the glaſs. For were it not for the different refrangibility of the rays, its breadth, in an 100 foot teleſcope whoſe aperture is 4 inches, would be but $\frac{961}{72000000}$ parts of an inch, as is manifeſt by the foregoing computation. And therefore in this caſe the greateſt errors, ariſing from the ſpherical figure of the glaſs, would be to the greateſt ſenſible errors, ariſing from the different refrangibility of the rays, as $\frac{961}{72000000}$ to $\frac{4}{250}$ at moſt, that is only as 1 to 1200. And this ſufficiently ſhews that it is not the ſpherical figures of glaſſes, but the different refrangibility of the rays, which hinders the perfection of teleſcopes. of TELESCOPES.

There is another argument, by which it may appear, that the different refrangibility of rays is the true cauſe of the imperfection of teleſcopes. For the errors of the rays, ariſing from the ſpherical figures of object-glaſſes, are as the cubes of the apertures of the object-glaſſes; and thence to make teleſcopes of various lengths magnify with equal diſtinctneſs, the apertures of the object-glaſſes, and the charges or magnifying powers ought to be as the cubes of the ſquare-roots of their lengths; which doth not anſwer to experience. But the errors of the rays, ariſing from the different refrangibility, are as the apertures of the object-glaſſes; and thence to make teleſcopes of various lengths magnify with equal diſtinctneſs, their apertures and charges ought to be as the ſquare roots of their lengths; and this anſwers to experience, as is well known. For inſtance, a teleſcope of 64 feet in length, with an aperture of $2\frac{2}{3}$ inches, magnifies about 120 times, with as much diſtinctneſs as one of a foot in length, with $\frac{1}{3}$ of an inch aperture, magnifies 15 times.

Now were it not for this different refrangibility of rays, teleſcopes might be brought to a greater perfection than we have yet deſcribed, by compoſing the object-glaſs of two glaſſes with water between them. Let ADFC [in *fig.* 28.] repreſent the object-glaſs compoſed of two glaſſes, ABED and BEFC, alike convex on the outſides AGD and CHF, and alike concave on the inſides BME, BNE. with water in the concavity BMEN. Let the ſine of incidence out of glaſs into air be as I to R, and out of water into air as K to R, and by conſequence out of glaſs into water as I to K:

and let the diameter of the ſphere to which the convex ſides, AGD and CHF, are ground be D; and the diameter of the ſphere to which the concave ſides, BME and BNE, are ground be to D, as the cube-root of KK−KI to the cube-root of RK−RI: and the refractions on the concave ſides of the glaſſes will very much correct the errors of the refractions on the convex ſides, ſo far as they ariſe from the ſphericalneſs of the figure. And by this means might teleſcopes be brought to ſufficient perfection, were it not for the different refrangibility of ſeveral ſorts of rays. But by reaſon of this different refrangibility, I do not yet ſee any other means of improving teleſcopes by refractions alone, than that of increaſing their lengths; for which end the late contrivance of *Hugenius* ſeems well accommodated. For very long tubes are cumberſome, and ſcarce to be readily managed, and by reaſon of their length are very apt to bend, and ſhake by bending, ſo as to cauſe a continual trembling in the objects, whereby it becomes difficult to ſee them diſtinctly: whereas, by his contrivance, the glaſſes are readily manageable, and the object-glaſs, being fixed upon a ſtrong upright pole, becomes more ſteady.

Reflecting

Seeing therefore the improvement of teleſcopes of given lengths by refractions is deſperate; I contrived heretofore a perſpective by reflexion, uſing inſtead of an object-glaſs a concave metal. The diameter of the ſphere, to which the metal was ground concave, was about 25 Engliſh inches, and by conſequence the length of the inſtrument about ſix inches and a quarter. The eye-glaſs was plano-convex, and the diameter of the ſphere to which the convex ſide was ground was about $\frac{1}{5}$ of an inch, or a little leſs, and by conſequence it magnified between 30 and 40 times. By another way of meaſuring I found that it magnified about 35 times. The concave metal bore an aperture of an inch and a third part; but the aperture was limited not by an opake circle, covering the limb of the metal round about, but by an opake circle placed between the eye-glaſs and the eye; and perforated in the middle with a little round hole for the rays to paſs through to the eye. For this circle by being placed here, ſtopped much of the erroneous light, which otherwiſe would have diſturbed the viſion. By comparing it with a pretty good perſpective of four

four feet in length, made with a concave eye-glaſs, I could read at a greater diſtance with my own inſtrument than with the glaſs. Yet objects appeared much darker in it than in the glaſs, and that partly becauſe more light was loſt by reflexion in the metal, than by refraction in the glaſs, and partly becauſe my inſtrument was overcharged. Had it magnified but 30 or 25 times, it would have made the object appear more briſk and pleaſant. Two of theſe I made about 16 years ago, and have one of them ſtill by me, by which I can prove the truth of what I write. Yet it is not ſo good as at the firſt. For the concave has been divers times tarniſhed and cleared again, by rubbing it with very ſoft leather. When I made theſe, an artiſt in *London* undertook to imitate it; but uſing another way of poliſhing them than I did, he fell much ſhort of what I had attained to, as I afterwards underſtood by diſcourſing with the under workman he had employed. The poliſh I uſed was in this manner. I had two round copper-plates each ſix inches in diameter, the one convex the other concave, ground very true to one another. On the convex I ground the object-metal or concave which was to be poliſhed, till it had taken the figure of the convex, and was ready for a poliſh. Then I pitched over the convex very thinly, by dropping melted pitch upon it, and warming it to keep the pitch ſoft, whilſt I ground it with the concave copper wetted, to make it ſpread evenly all over the convex. Thus by working it well, I made it as thin as a groat; and after the convex was cold, I ground it again to give it as true a figure as I could. Then I took putty, which I had made very fine by waſhing it from all its groſſer particles, and laying a little of this upon the pitch, I ground it upon the pitch with the concave copper till it had done making a noiſe; and then upon the pitch I ground the object-metal with a briſk motion, for about two or three minutes of time, leaning hard upon it. Then I put freſh putty upon the pitch, and ground it again till it had done making a noiſe, and afterwards ground the object-metal upon it as before. And this work I repeated till the metal was poliſhed, grinding it the laſt time with all my ſtrength for a good while together, and frequently breathing upon the pitch to keep it moiſt, without laying on any more freſh putty.

TELESCOPE.

The

Reflecting

The object-metal was two inches broad, and about one-third part of an inch thick, to keep it from bending. I had two of theſe metals, and when I had poliſhed them both I tried which was beſt; and ground the other again, to ſee if I could make it better, than that which I kept. And thus by many trials I learned the way of poliſhing, till I made thoſe two reflecting perſpectives I ſpake of above. For this art of poliſhing will be better learned by repeated practice than by my deſcription. Before I ground the object-metal on the pitch, I always ground the putty on it with the concave copper till it had done making a noiſe; becauſe if the particles of the putty were not by this means made to ſtick faſt in the pitch, they would by rolling up and down grate and fret the object-metal and fill it full of little holes.

But becauſe metal is more difficult to poliſh than glaſs, and is afterwards very apt to be ſpoiled by tarniſhing, and reflects not ſo much light as glaſs quick-ſilvered over does: I would propound to uſe, inſtead of the metal, a glaſs ground concave on the foreſide, and as much convex on the backſide, and quick-ſilvered over on the convex ſide. The glaſs muſt be every where of the ſame thickneſs exactly. Otherwiſe it will make objects look coloured and indiſtinct. By ſuch a glaſs I tried, above five or ſix years ago, to make a reflecting teleſcope of four feet in length to magnify about 150 times, and I ſatisfied myſelf, that there wants nothing but a good artiſt to bring the deſign to perfection. For the glaſs being wrought by one of our *London* artiſts after ſuch a manner as they grind glaſſes for teleſcopes, though it ſeemed as well wrought as the object-glaſſes uſe to be, yet when it was quick-ſilvered, the reflexion diſcovered innumerable inequalities all over the glaſs. And by reaſon of theſe inequalities, objects appeared indiſtinct in this inſtrument. For the errors of reflected rays cauſed by any inequality of the glaſs, are about ſix times greater than the errors of refracted rays cauſed by the like inequalities. Yet by this Experiment I ſatisfied myſelf that the reflexion on the concave ſide of the glaſs, which I feared would diſturb the viſion, did no ſenſible prejudice to it; and by conſequence that nothing is wanting to perfect theſe teleſcopes, but good workmen, who can grind and poliſh glaſſes truly ſpherical.

An

An object-glaſs of a fourteen-foot teleſcope, made by an artificer at *London*, I once mended conſiderably, by grinding it on pitch with putty, and leaning very eaſily on it in the grinding, leſt the putty ſhould ſcratch it. Whether this way may not do well enough for poliſhing theſe reflecting glaſſes, I have not yet tried. But he that ſhall try either this, or any other way of poliſhing, which he may think better, may do well to make his glaſſes ready for poliſhing, by grinding them without that violence, wherewith our *London* workmen preſs their glaſſes in grinding. For by ſuch violent preſſure, glaſſes are apt to bend a little in the grinding, and ſuch bending will certainly ſpoil their figure. To recommend therefore the conſideration of theſe reflecting glaſſes, to ſuch artiſts as are curious in figuring glaſſes, I ſhall deſcribe this optical inſtrument in the following Propoſition. TELESCOPE.

PROP. VIII. PROB. II.

To ſhorten Teleſcopes.

Let ABDC [in *fig.* 29.] repreſent a glaſs ſpherically concave on the foreſide AB, and as much convex on the backſide CD, ſo that it be every where of an equal thickneſs. Let it not be thicker on one ſide than on the other, leſt it make objects appear coloured and indiſtinct; and let it be very truly wrought and quick-ſilvered over on the backſide, and ſet in the tube VXYZ which muſt be very black within. Let EFG repreſent a priſm of glaſs, or cryſtal, placed near the other end of the tube, in the middle of it, by means of a handle of braſs or iron FGK, to the end of which made flat it is cemented. Let this priſm be rectangular at E, and let the other two angles at F and G be accurately equal to each other, and by conſequence equal to half right ones; and let the plane ſides FE and GE be ſquare, and by conſequence the third ſide FG a rectangular parallelogram, whoſe length is to its breadth in a ſubduplicate proportion of two to one. Let it be ſo placed in the tube, that the axis of the ſpeculum may paſs through the middle of the ſquare ſide EF perpendicularly, and by conſequence through the middle of the ſide FG at an angle of 45 degrees; and let the ſide EF be turned towards the ſpeculum, and the

Reflecting

the diſtance of this priſm from the ſpeculum be ſuch, that the rays of the light PQ, RS, &c. which are incident upon the ſpeculum in lines parallel to the axis thereof, may enter the priſm at the ſide EF, and be reflected by the ſide FG, and thence go out of it, through the ſide GE, to the point T; which muſt be the common focus of the ſpeculum ABDC, and of a plano-convex eye-glaſs, H, through which thoſe rays muſt paſs to the eye. And let the rays, at their coming out of the glaſs, paſs through a ſmall round hole or aperture made in a little plate of lead, braſs or ſilver, wherewith the glaſs is to be covered, which hole muſt be no bigger than is neceſſary for light enough to paſs through. For ſo it will render the object diſtinct, the plate, in which it is made, intercepting all the erroneous part of the light which comes from the verges of the ſpeculum AB. Such an inſtrument well made, if it be ſix foot long (reckoning the length from the ſpeculum to the priſm, and thence to the focus T) will bear an aperture of ſix inches at the ſpeculum, and magnify between two and three hundred times. But the hole H here limits the aperture with more advantage, than if the aperture was placed at the ſpeculum. If the inſtrument be made longer or ſhorter, the aperture muſt be in proportion as the cube of the ſquare-root of the length, and the magnifying as the aperture. But it is convenient that the ſpeculum be an inch or two broader than the aperture at leaſt, and that the glaſs of the ſpeculum be thick, that it bend not in the working. The priſm EFG muſt be no bigger than is neceſſary, and its backſide FG muſt not be quick-ſilvered over. For without quick-ſilver it will reflect all the light incident on it from the ſpeculum.

In this inſtrument the object will be inverted, but may be erected by making the ſquare ſides EF and EG of the priſm EFG not plane but ſpherically convex, that the rays may croſs as well before they come at it, as afterwards between it and the eye-glaſs. If it be deſired that the inſtrument bear a larger aperture, that may be alſo done by compoſing the ſpeculum of two glaſſes with water between them.

If the theory of making teleſcopes could at length be fully brought into practice, yet there would be certain bounds beyond

Fig. 1.

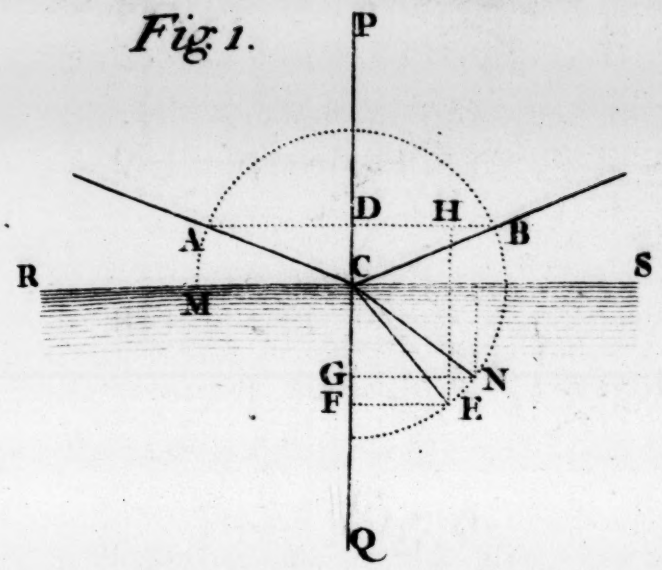
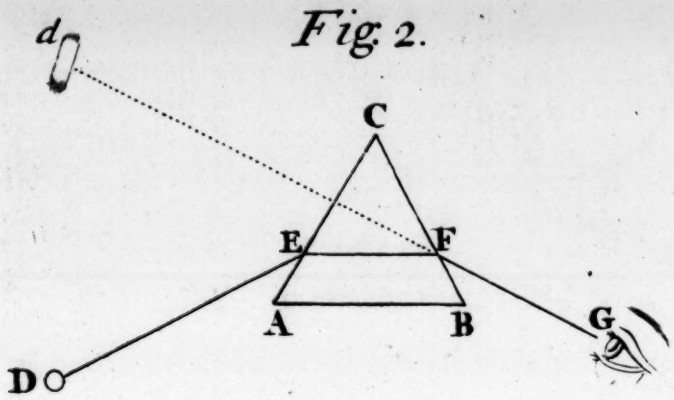


Fig. 2.



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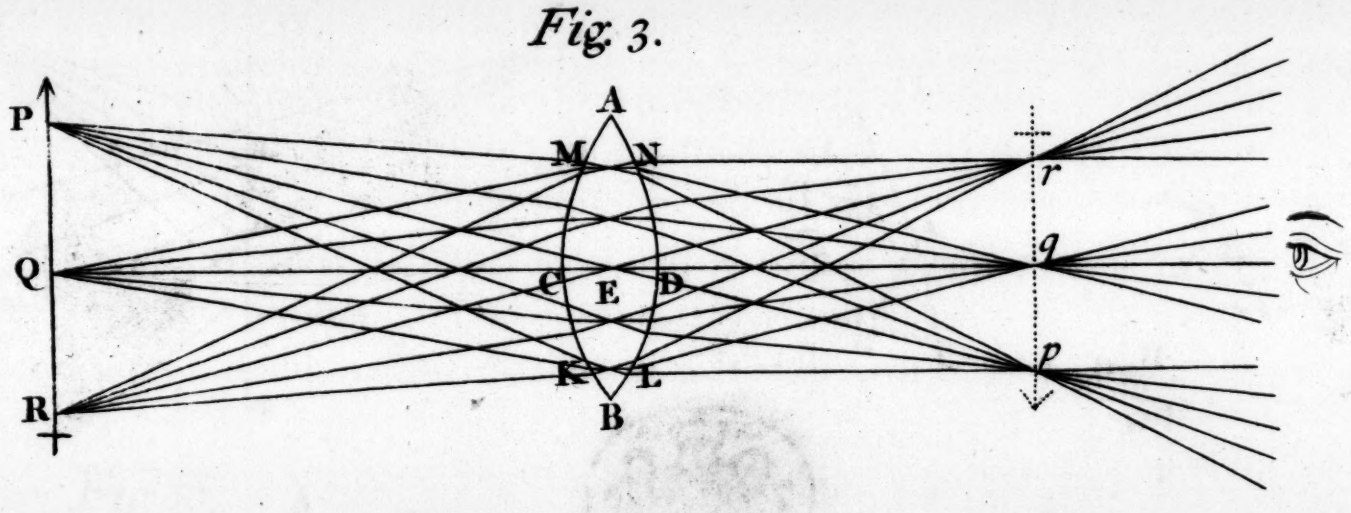

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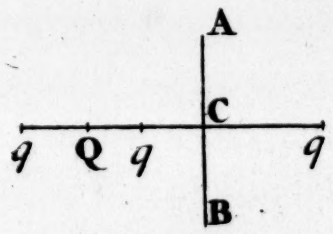

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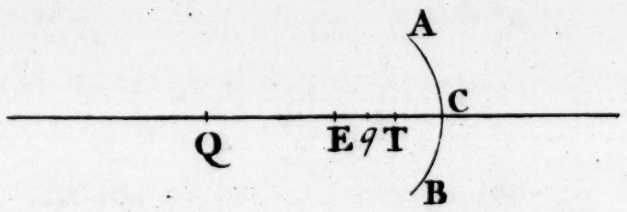


Fig. 6.

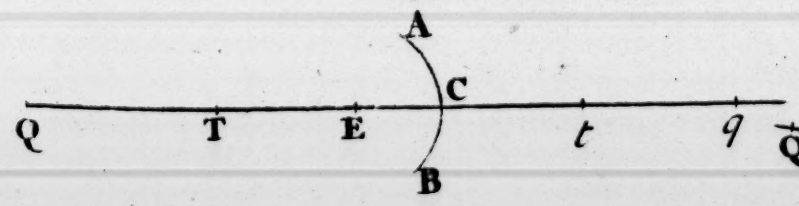
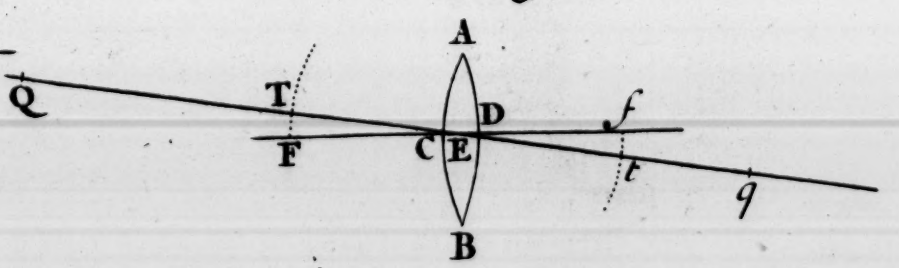
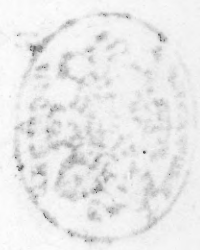


Fig. 7.





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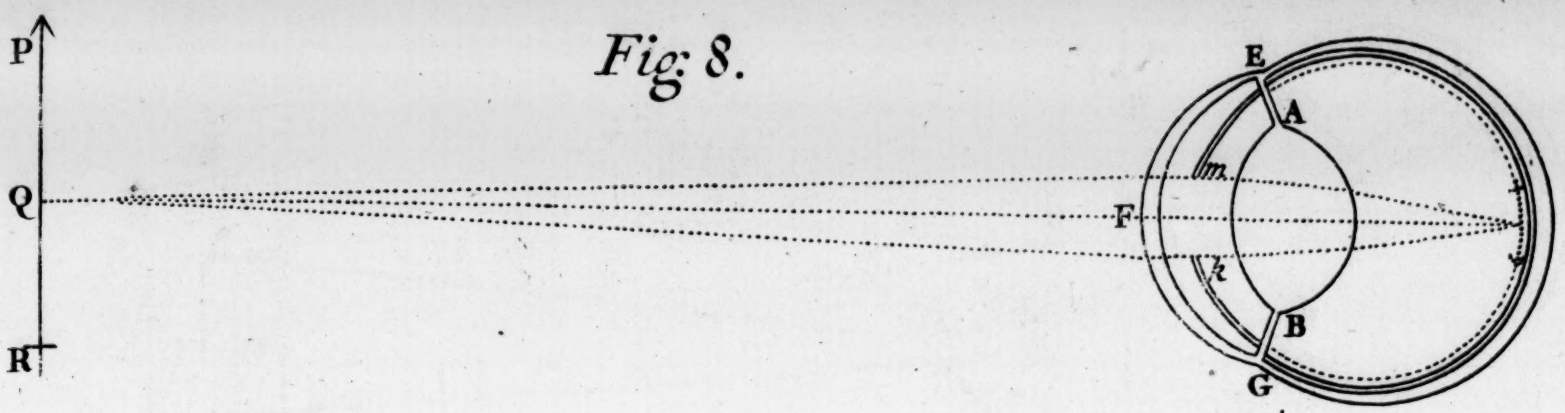

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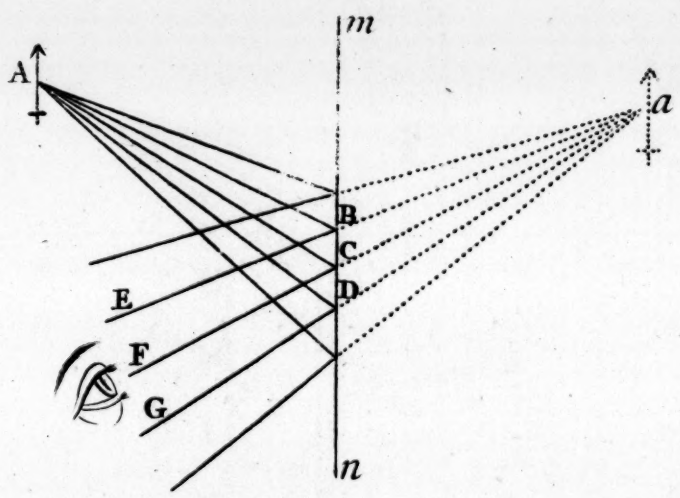

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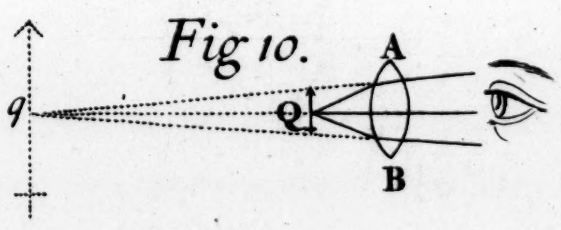

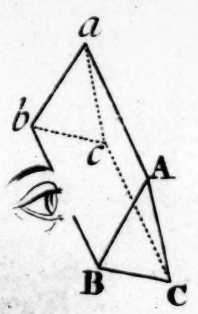

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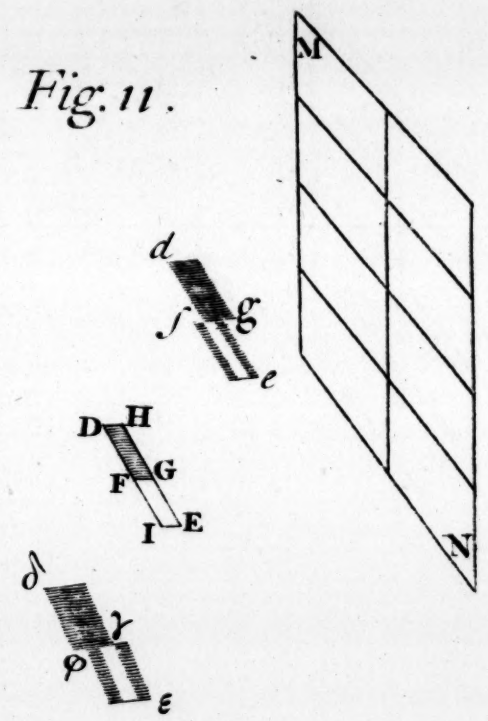

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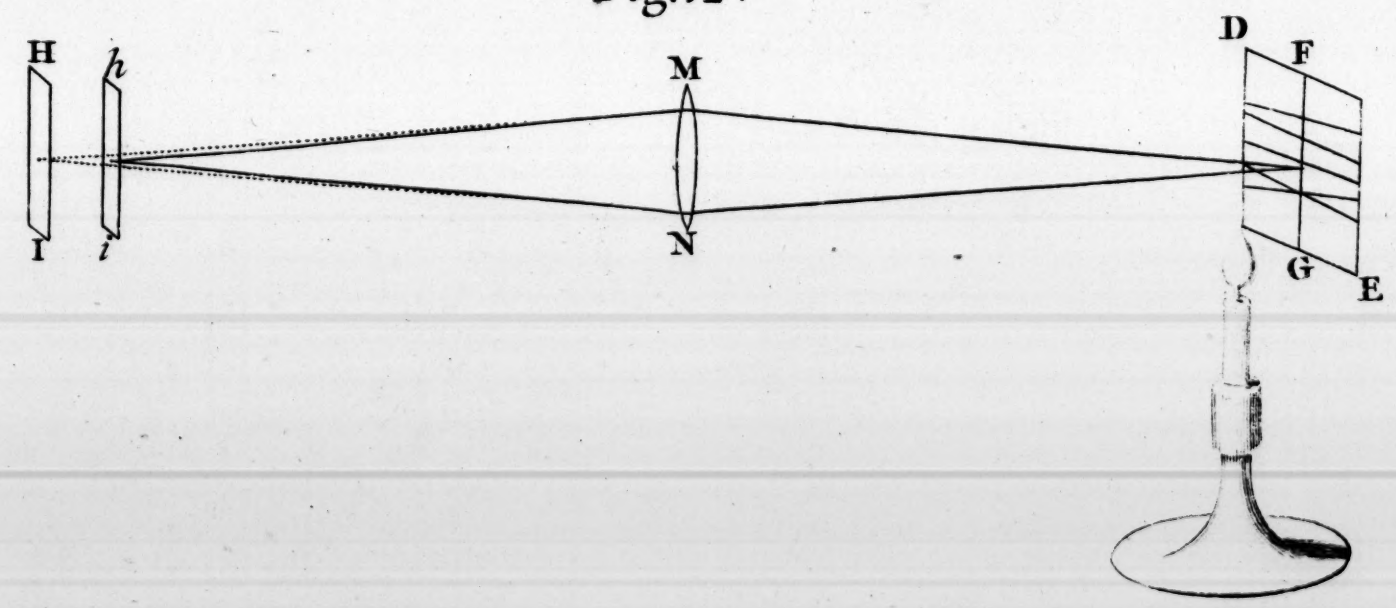

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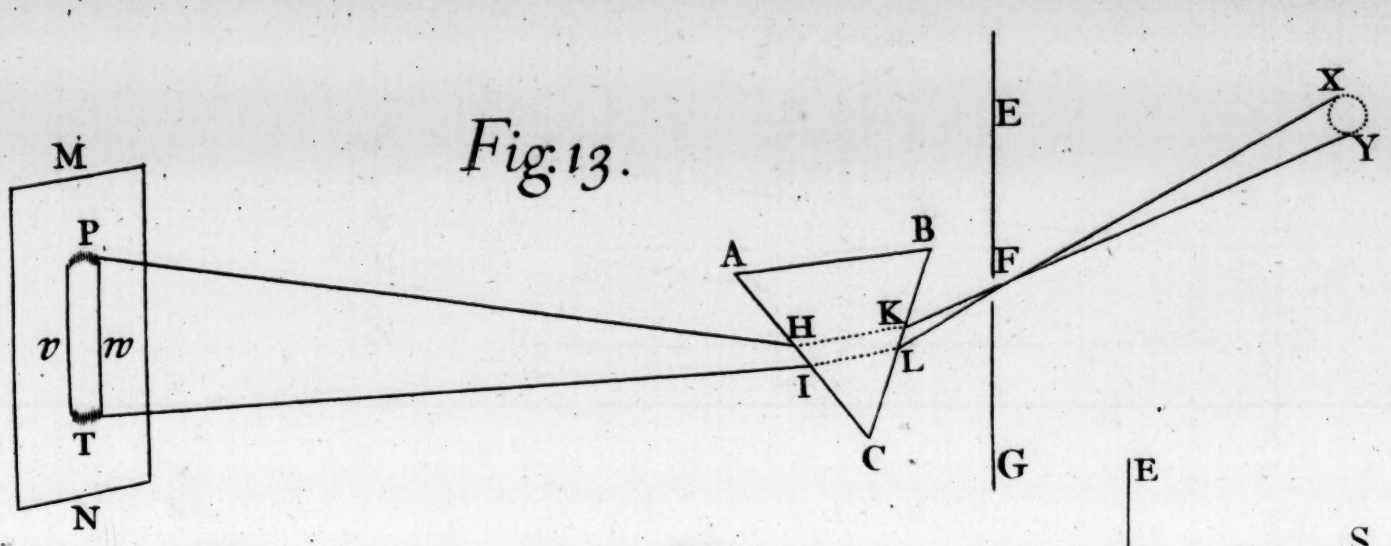

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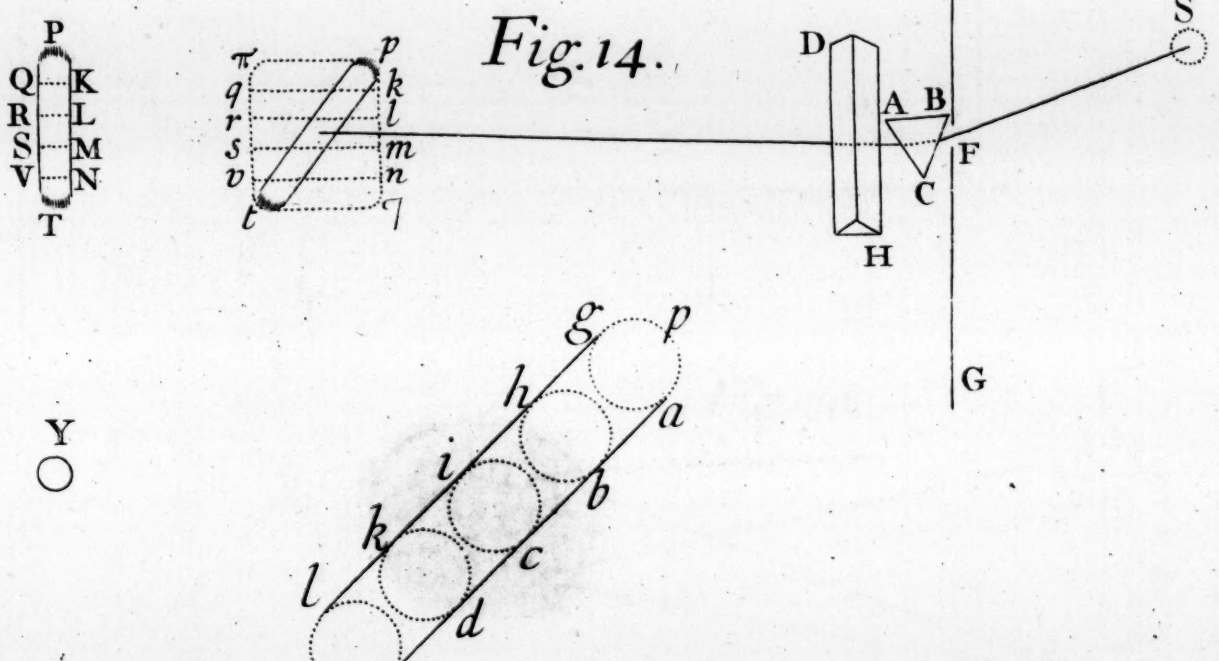

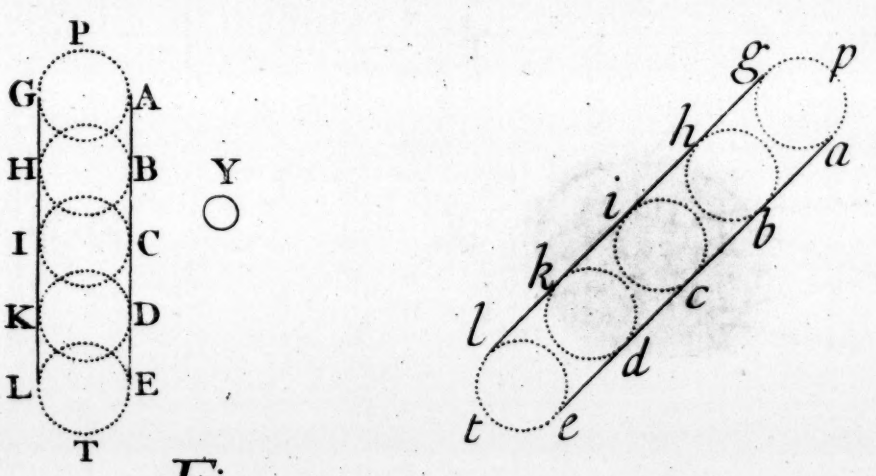

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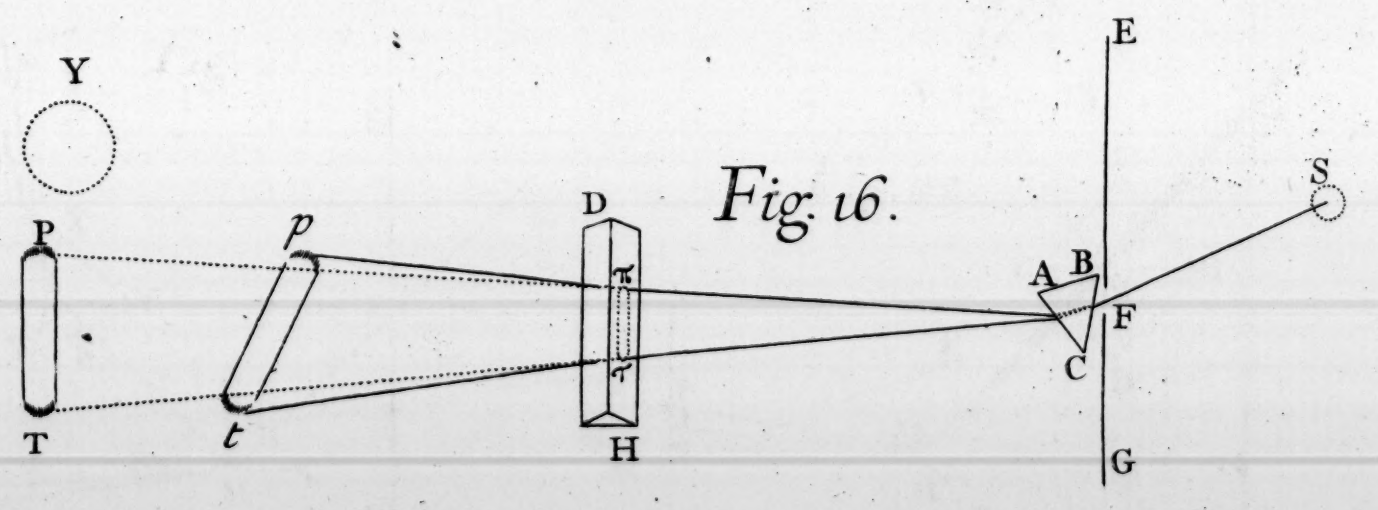

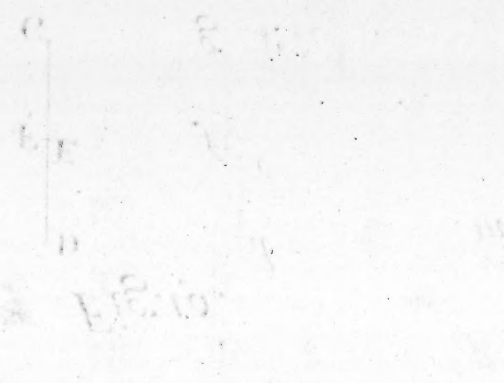

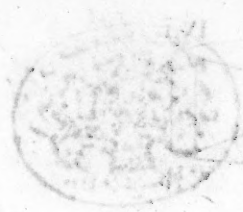

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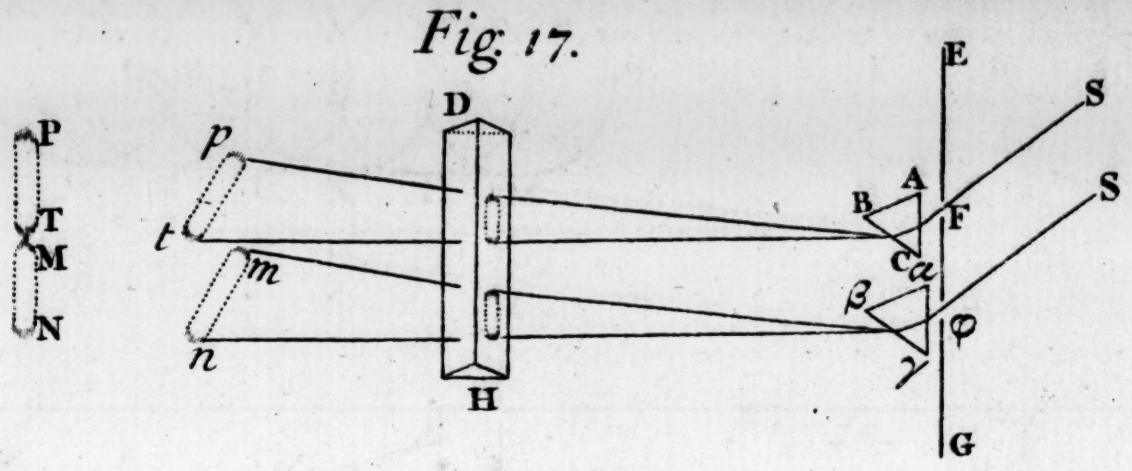

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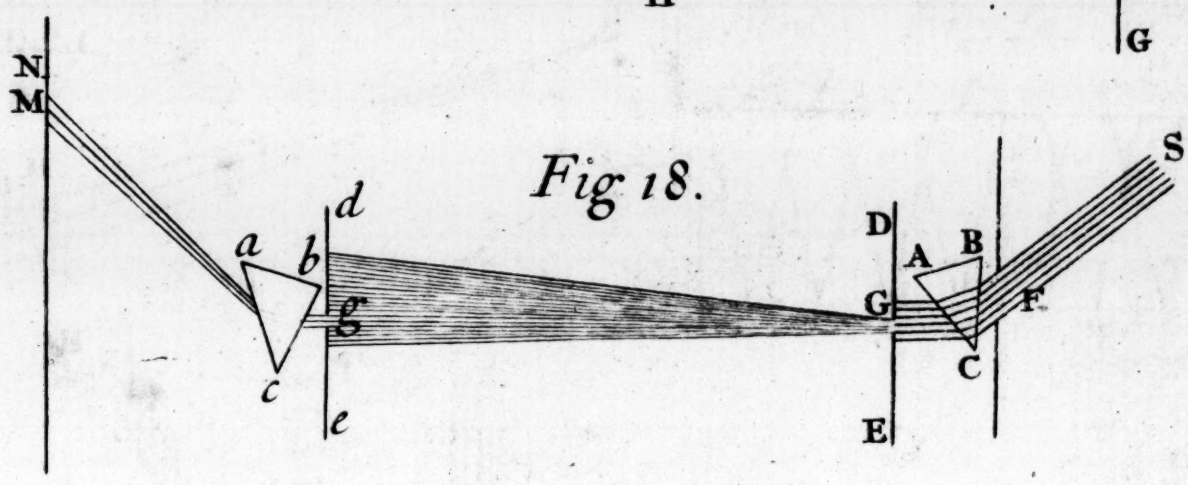

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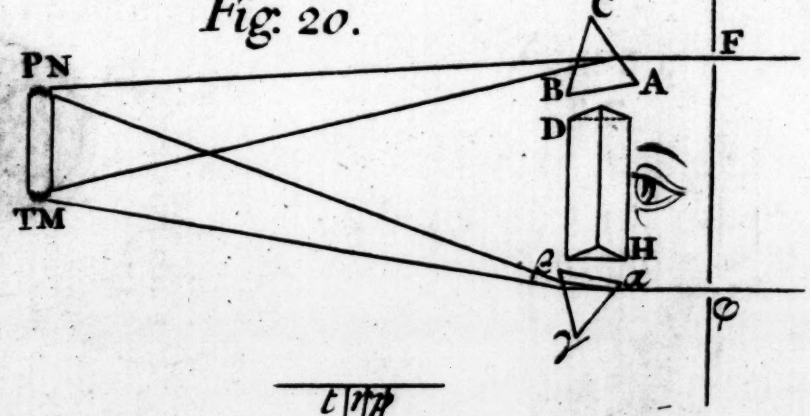

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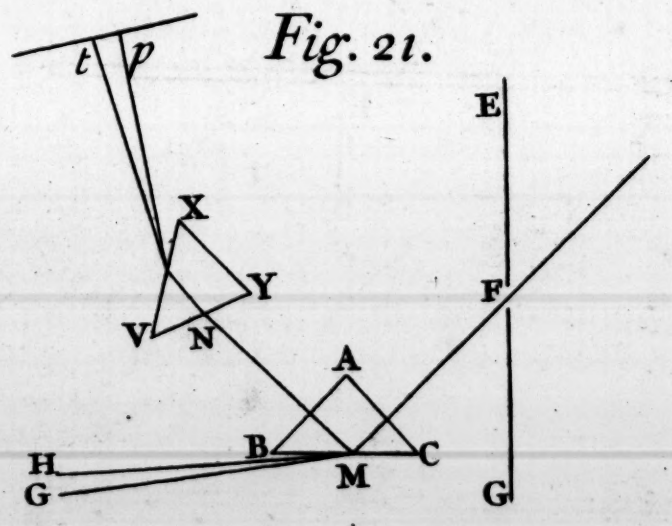

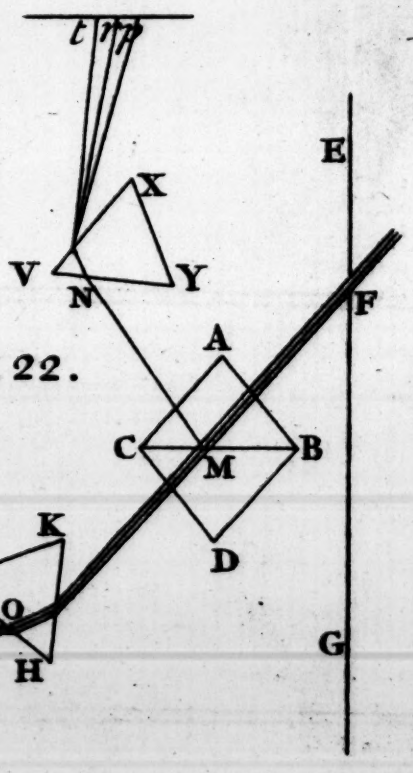

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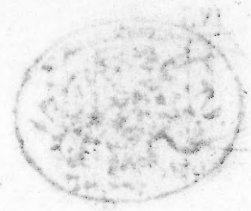

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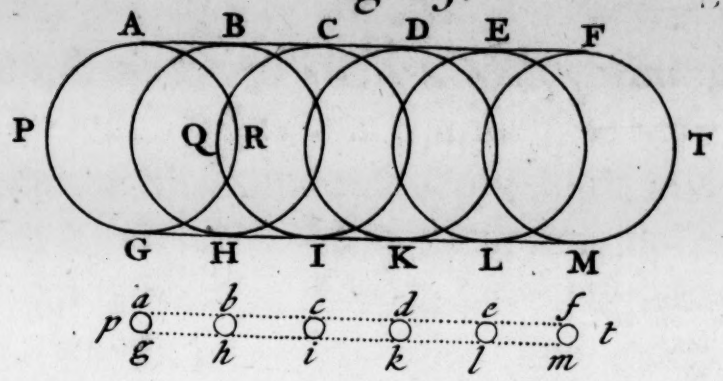

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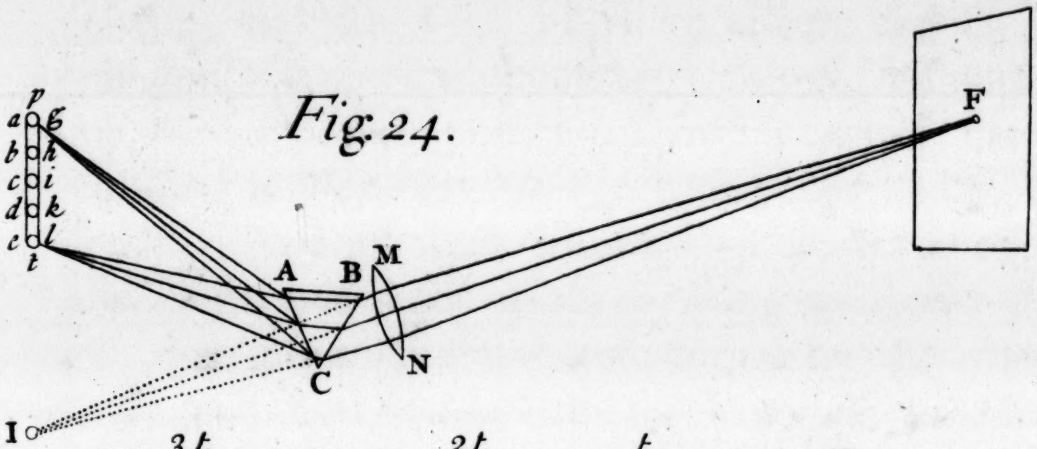


Fig: 25.

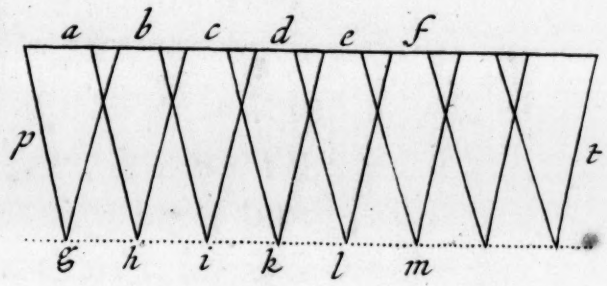


Fig: 26.

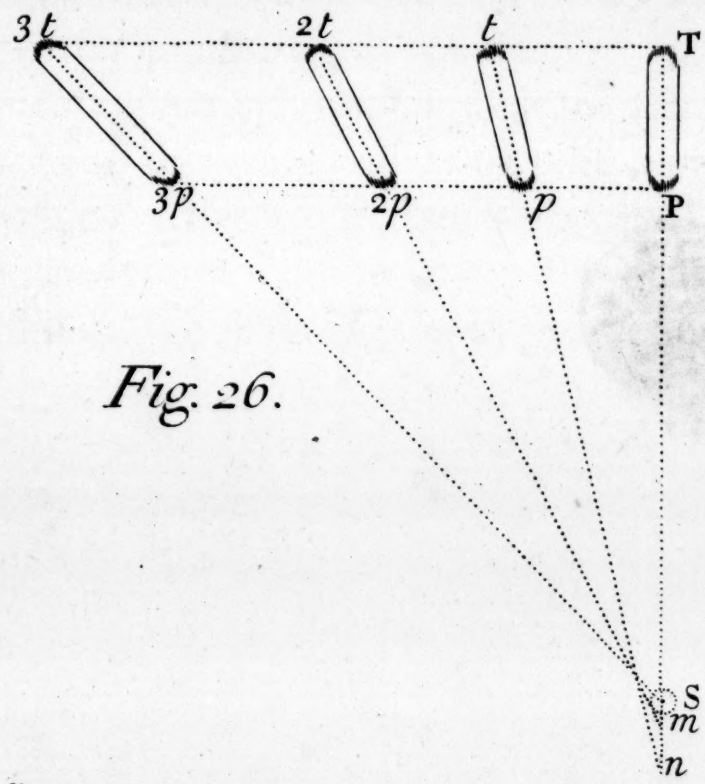


Fig: 27.

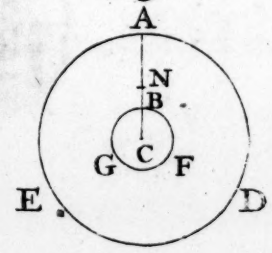
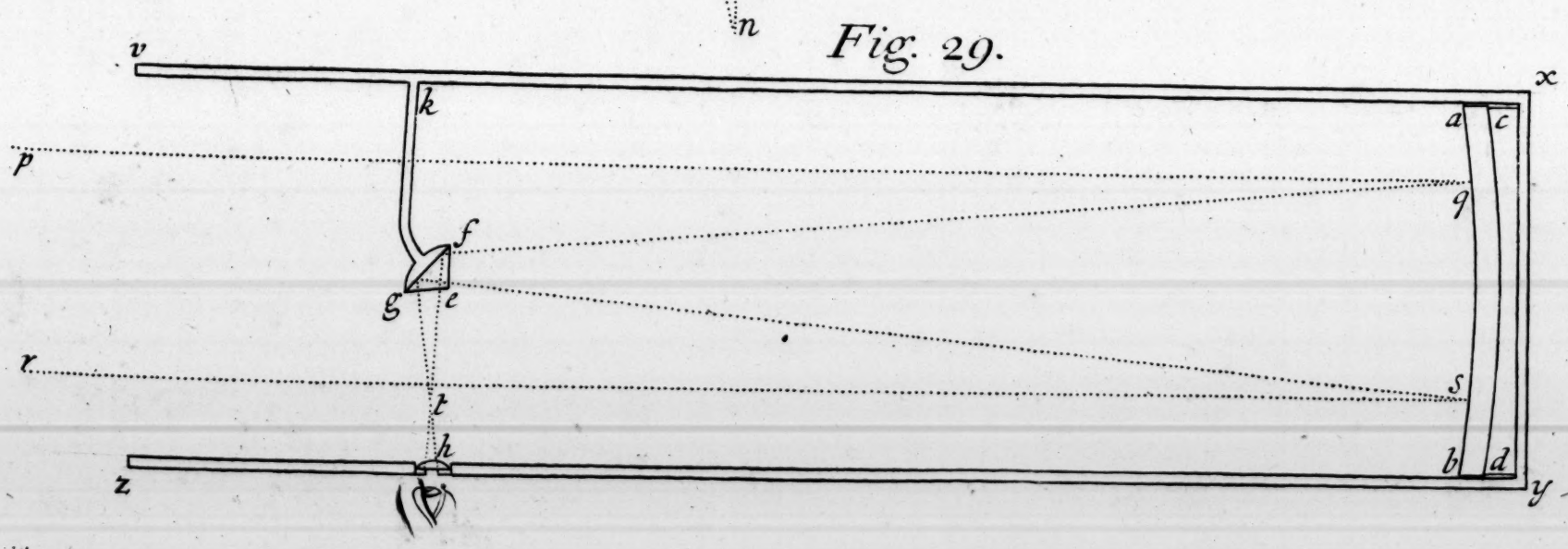


Fig: 28.

Fig: 29.



which teleſcopes could not perform. For the air, through which we look upon the ſtars, is in a perpetual tremor; as may be ſeen by the tremulous motion of ſhadows caſt from high towers, and by the twinkling of the fixed ſtars. But theſe ſtars do not twinkle when viewed through teleſcopes which have large apertures. For the rays of light, which paſs through divers parts of the aperture, tremble each of them apart; and by means of their various and ſometimes contrary tremors, fall at one and the ſame time upon different points in the bottom of the eye; and their trembling motions are too quick and confuſed to be perceived ſeverally. And all theſe illuminated points conſtitute one broad lucid point, compoſed of thoſe many trembling points confuſedly and inſenſibly mixed with one anther by very ſhort and ſwift tremors; and thereby cauſe the ſtar to appear broader than it is, and without any trembling of the whole. Long teleſcopes may cauſe objects to appear brighter and larger than ſhort ones can do; but they cannot be ſo formed as to take away that confuſion of the rays, which ariſes from the tremors of the atmoſphere. The only remedy is a moſt ſerene and quiet air, ſuch as may perhaps be found on the tops of the higheſt mountains above the groſſer clouds.

THE

FIRST BOOK

OF

OPTICS.

PART II.

PROP. I. THEOR. I.

The phænomena of colours in Refracted or Reflected Light are not caused by new modifications of the light variously impressed, according to the various terminations of the light and shadow.

The proof by Experiments.

Exper. 1. FOR if the sun shine into a very dark chamber through an oblong hole F [in *fig.* 1.] whose breadth is the sixth or eighth part of an inch, or something less; and his beam, FH, do afterwards pass first through a very large prism ABC, distant about 20 feet from the hole, and parallel to it, and then (with its white part) through an oblong hole H, whose breadth is about the fortieth or sixtieth part of an inch, and which is made in a black opake body GI, and placed at the distance of two or three feet from the prism, in a parallel situation both to the

the prism and to the former hole, and if this White light thus transmitted through the hole H, fall afterwards upon a white paper *pt*, placed after that hole H, at the distance of three or four feet from it, and there paint the usual colours of the prism, suppose Red at *t*, Yellow at *s*, Green at *r*, Blue at *q*, and Violet at *p*; you may with an iron wire, or any such like slender opake body, whose breadth is about the tenth part of an inch, by intercepting the rays at *k*, *l*, *m*, *n* & *o*, take away any one of the colours at *t*, *s*, *r*, *q* or *p*, whilst the other colours remain upon the paper as before; or with an obstacle something bigger you may take away any two, or three, or four colours together, the rest remaining. So that any one of the colours, as well as Violet, may become outmost in the confine of the shadow towards *p*, and any one of them as well as Red may become outmost in the confine of the shadow towards *t*, and any one of them may also border upon the shadow made within the colours by the obstacle, R, intercepting some intermediate part of the light; and, lastly, any one of them, by being left alone, may border upon the shadow on either hand. All the colours have themselves indifferently to any confines of shadow, and therefore the differences of these colours from one another, do not arise from the different confines of shadow, whereby light is variously modified, as has hitherto been the opinion of philosophers. In trying these things it is to be observed, that by how much the holes, F and H, are narrower, and the intervals between them and the prism greater, and the chamber darker, by so much the better doth the experiment succeed; provided the light be not so far diminished, but that the colours at *pt* be sufficiently visible. To procure a prism of solid glass large enough for this Experiment will be difficult, and therefore a prismatick vessel must be made of polished glass plates cemented together, and filled with salt water or clear oil.

Exper. 2. The sun's light let into a dark chamber through the round hole F [in *fig.* 2.] half an inch wide, passed first through the prism ABC placed at the hole; and then through a lens, PT, something more than four inches broad, and about eight feet distant from the prism; and thence converged to o the focus of the lens distant from it about three feet; and there fell upon a

Colour no effect

white paper DE. If that paper was perpendicular to that light incident upon it, as it is repreſented in the poſture DE, all the colours upon it, at o, appeared White. But if the paper being turned about an axis parallel to the priſm, became very much inclined to the light, as it is repreſented in the poſitions *de* and *δε*; the ſame light in the one caſe appeared Yellow and Red, in the other Blue. Here one and the ſame part of the light, in one and the ſame place, according to the various inclinations of the paper, appeared in one caſe White, in another Yellow or Red, in a third Blue; whilſt the confine of light and ſhadow, and the refractions of the priſm, in all theſe caſes, remained the ſame.

Exper. 3. Such another Experiment may be more eaſily tried as follows. Let a broad beam of the ſun's light, coming into a dark chamber through a hole in the window-ſhut, be refracted by a large priſm ABC [in *fig.* 3.] whoſe refracting angle C is more than 60 degrees; and ſo ſoon as it comes out of the priſm, let it fall upon the white paper, DE, glewed upon a ſtiff plane; and this light, when the paper is perpendicular to it, as it is repreſented in DE, will appear perfectly White upon the paper; but when the paper is very much inclined to it, in ſuch a manner as to keep always parallel to the axis of the priſm, the whiteneſs of the whole light upon the paper will, according to the inclination of the paper this way or that way, change either into Yellow and Red, as in the poſture *de*; or into Blue and Violet, as in the poſture *δε*. And if the light, before it fall upon the paper, be twice refracted the ſame way by two parallel priſms, theſe colours will become the more conſpicuous. Here all the middle parts of the broad beam of white light, which fell upon the paper, did, without any confine of ſhadow to modify it, become coloured all over with one uniform colour; the colour being always the ſame in the middle of the paper as at the edges; and this colour changed according to the various obliquity of the reflecting paper, without any change in the refractions or ſhadow, or in the light, which fell upon the paper. And therefore theſe colours are to be derived from ſome other cauſe, than the new modifications of light by refractions and ſhadows.

If it be aſked, What then is their cauſe? I anſwer, that the paper, in the poſture *de*, being more oblique to the more refrangible rays than to the leſs refrangible ones, is more ſtrongly illuminated by the latter than by the former; and therefore the leſs refrangible rays are predominant in the reflected light. And wherever they are predominant in any light they tinge it with Red or Yellow, as may in ſome meaſure appear by the firſt Propoſition of the firſt Book, and will more fully appear hereafter. And the contrary happens in the poſture of the paper *δε*, the more refrangible rays being then predominant, which always tinge light with Blues and Violets. of the confine of Light and Shade.

Exper. 4. The colours of bubbles with which children play are various, and change their ſituation variouſly, without any reſpect to any confine of ſhadow. If ſuch a bubble be covered with a concave glaſs, to keep it from being agitated by any wind or motion of the air, the colours will ſlowly and regularly change their ſituation, even whilſt the eye, and the bubble, and all bodies which emit any light, or caſt any ſhadow, remain unmoved. And therefore their colours ariſe from ſome regular cauſe, which depends not on any confine of ſhadow. What this cauſe is, will be ſhewed in the next Book.

To theſe Experiments may be added the tenth Experiment of the firſt Book; where the ſun's light in a dark room being trajected through the parallel ſuperficies of two priſms tied together in the form of a parallelopiped, became totally of one uniform Yellow or Red colour, at its emerging out of the priſms. Here, in the production of theſe colours, the confine of ſhadow can have nothing to do. For the light changes from White to Yellow, Orange and Red ſucceſſively, without any alteration of the confine of ſhadow: and at both edges of the emerging light, where the contrary confines of ſhadow ought to produce different effects, the colour is one and the ſame, whether it be White, Yellow, Orange or Red: and in the middle of the emerging light, where there is no confine of ſhadow at all, the colour is the very ſame as at the edges, the whole light at its very firſt emergence being of one uniform colour, whether White, Yellow, Orange or Red, and going on thence perpetually without any change of colour,

Colour no effect of confine of Light and Shade.

lour, ſuch as the confine of ſhadow is vulgarly ſuppoſed to work in refracted light after its emergence. Neither can theſe colours ariſe from any new modifications of the light by refractions; becauſe they change ſucceſſively from White to Yellow, Orange and Red, while the refractions remain the ſame; and alſo becauſe the refractions are made contrary ways by parallel ſuperficies, which deſtroy one anothers effects. They ariſe not therefore from any modifications of light made by refractions and ſhadows, but have ſome other cauſe. What that cauſe is we ſhewed above in this tenth Experiment, and need not here repeat it.

There is yet another material circumſtance of this Experiment. For this emerging light being by a third priſm HIK [in *fig.* 22. *Part* I.] refracted towards the paper PT, and there painting the uſual colours of the priſm, Red, Yellow, Green, Blue, Violet: if theſe colours aroſe from the refractions of that priſm modifying the light, they would not be in the light before its incidence on that priſm. And yet in that Experiment we found, that when, by turning the two firſt priſms about their common axis, all the colours were made to vaniſh but the Red; the light which makes that Red being left alone, appeared of the very ſame Red colour before its incidence on the third priſm. And in general we find by other Experiments, that when the rays, which differ in refrangibility, are ſeparated from one another, and any one ſort of them is conſidered apart; the colour of the light, which they compoſe, cannot be changed by any refraction or reflexion whatever; as it ought to be, were colours nothing elſe than modifications of light cauſed by refractions, and reflexions, and ſhadows. This unchangeableneſs of colour I am now to deſcribe in the following Propoſition.

PROP. II. THEOR. II.

All Homogeneal Light has its proper colour anſwering to its degree of refrangibility, and that colour cannot be changed by reflexions and refractions.

The colours of Homogeneal

In the Experiments of the fourth Propoſition of the firſt Book, when I had ſeparated the Heterogeneous Rays from one another, the

the ſpectrum *pt*, formed by the ſeparated rays, did in the progreſs from its end *p*, on which the moſt refrangible rays fell, unto its other end *t*, on which the leaſt refrangible rays fell, appear tinged with this ſeries of colours; Violet, Indigo, Blue, Green, Yellow, Orange, Red, together with all their intermediate degrees in a continual ſucceſſion perpetually varying. So that there appeared as many degrees of colours, as there were ſorts of rays differing in refrangibility. Light unchangeable.

Exper. 5. Now that theſe colours could not be changed by refraction, I knew by refracting with a priſm ſometimes one very little part of this light, ſometimes another very little part, as is deſcribed in the twelfth Experiment of the firſt Book. For by this refraction the colour of the light was never changed in the leaſt. If any part of the Red light was refracted, it remained totally of the ſame Red colour as before. No Orange, no Yellow, no Green or Blue, no other new colour was produced by that refraction. Neither did the colour any ways change by repeated refractions, but continued always the ſame Red entirely as at firſt. The like conſtancy and immutability I found alſo in the Blue, Green, and other colours. So alſo if I looked through a priſm upon any body illuminated with any part of this Heterogeneal light, as in the fourteenth Experiment of the firſt Book is deſcribed; I could not perceive any new colour generated this way. All bodies illuminated with Compound light appear through priſms confuſed (as was ſaid above) and tinged with various new colours; but thoſe illuminated with Homogeneal light appeared through priſms neither leſs diſtinct, nor otherwiſe coloured, than when viewed with the naked eyes. Their colours were not in the leaſt changed by the refraction of the interpoſed priſm. I ſpeak here of a ſenſible change of colour: for the light which I here call Homogeneal, being not abſolutely homogeneal, there ought to ariſe ſome little change of colour from its heterogeneity. But if that heterogeneity was ſo little, as it might be made by the ſaid Experiments of the fourth Propoſition, that change was not ſenſible; and therefore in Experiments, where ſenſe is judge, ought to be accounted none at all.

Exper. 6. And as theſe colours were not changeable by refractions, ſo neither were they by reflexions. For all White, Grey, Red, Yellow, Green, Blue, Violet bodies, as Paper, Aſhes, Red Lead, Orpiment, Indigo, Biſe, Gold, Silver, Copper, Graſs, Blue Flowers, Violets, bubbles of Water tinged with various colours, Peacock's Feathers, the tincture of *Lignum Nephriticum*, and ſuch like, in Red Homogeneal Light appeared totally red; in Blue light totally blue; in Green light totally green; and ſo of other colours. In the Homogeneal light of any colour they all appeared totally of that ſame colour; with this only difference, that ſome of them reflected that light more ſtrongly, others more faintly. I never yet found any body, which by reflecting Homogeneal light, could ſenſibly change its colour.

From all which it is manifeſt, that if the ſun's light conſiſted of but one ſort of rays, there would be but one colour in the whole world; nor would it be poſſible to produce any new colour by reflexions and refractions, and by conſequence that the variety of colours depends upon the compoſition of light.

DEFINITION.

The homogeneal light and rays which appear red, or rather make objects appear ſo, I call Rubrific or Red-making; thoſe which make objects appear yellow, green, blue and violet, I call Yellow-making, Green-making, Blue-making, Violet-making; and ſo of the reſt. And if at any time I ſpeak of light and rays as coloured, or endued with colours; I would be underſtood to ſpeak not philoſophically and properly, but groſsly, and according to ſuch conceptions as vulgar people, in ſeeing all theſe Experiments, would be apt to frame. For the rays, to ſpeak properly, are not coloured. In them there is nothing elſe, than a certain power and diſpoſition to ſtir up a ſenſation of this or that colour. For as ſound in a bell, or muſical ſtring, or other ſounding body, is nothing but a trembling motion; and in the air, nothing but that motion propagated from the object; and in the ſenſorium, it is a ſenſe of that motion under the form of ſound; ſo Colours in the Object are nothing but a diſpoſition to reflect this or that ſort of rays more copiouſly than the reſt; in the Rays, they are nothing but

but their diſpoſitions to propagate this or that motion into the ſenſorium; and in the ſenſorium, they are ſenſations of thoſe motions under the forms of colours.

PROP. III. PROB. I.

To define the refrangibility of the ſeveral ſorts of Homogeneal Light anſwering to the ſeveral colours.

The refrangibility of the ſeveral Rays defined.

For determining this Problem I made the following Experiment.

Exper. 7. When I had cauſed the rectilinear ſides AF, GM [in *fig.* 4.] of the ſpectrum of colours made by the priſm to be diſtinctly defined, as in the fifth Experiment of the firſt Part is deſcribed (a), there were found in it all the Homogeneal colours, in the ſame order and ſituation one among another as in the ſpectrum of ſimple light, deſcribed in the fourth Propoſition of that Part. For the circles, of which the ſpectrum of Compound Light PT is compoſed, and which in the middle parts of the ſpectrum interfere, and are intermixed with one another, are not intermixed in their outmoſt parts, where they touch thoſe rectilinear ſides AF and GM. And therefore in thoſe rectilinear ſides, when diſtinctly defined, there is no new colour generated by refraction. I obſerved alſo, that if any where between the two outmoſt circles, TMF and PGA, a right line, as $\gamma\delta$, was croſs to the ſpectrum, ſo as at both ends to fall perpendicularly upon its rectilinear ſides, there appeared one and the ſame colour, and degree of colour from one end of this line to the other. I delineated therefore in a paper the perimeter of the ſpectrum FAPGMT, and in trying the third Experiment of the firſt Book, I held the paper ſo that the ſpectrum might fall upon this delineated figure, and agree with it exactly; whilſt an aſſiſtant, whoſe eyes for diſtinguiſhing colours were more critical than mine, did by right lines $\alpha\beta$, $\gamma\delta$, $\varepsilon\zeta$, &c. drawn croſs the ſpectrum, note the confines of the colours; that is, of the Red M$\alpha\beta$F; of the Orange, $\alpha\gamma\delta\beta$; of the Yellow, $\gamma\varepsilon\zeta\delta$; of the Green, $\varepsilon\eta\theta\zeta$; of the Blue, $\eta\iota\kappa\theta$; of the Indigo,

(a) Namely, by placing a lens at the hole where the light was admitted, to take away the penumbra in the ſides of the image. Vid. p. 33.

The refrangibility of the

$\iota\lambda\mu\varkappa$; and of the Violet, λGAμ. And this operation being divers times repeated, both in the ſame and in ſeveral papers, I found that the obſervations agreed well enough with one another; and that the rectilinear ſides, MG and FA, were by the ſaid croſs lines divided after the manner of a Muſical Chord. Let GM be produced to x, that MX may be equal to GM, and conceive GX, λX, ιX, ηX, εX, γX, αX, MX, to be in proportion to one another, as the numbers, 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, $\frac{1}{2}$, and ſo to repreſent the chords of the key, and of a tone, a third minor, a fourth, a fifth, a ſixth major, a ſeventh and an eighth above that key: and the intervals Mα, $\alpha\gamma$, $\gamma\varepsilon$, $\varepsilon\eta$, $\eta\iota$, $\iota\lambda$, and λG, will be the ſpecies which the ſeveral colours (Red, Orange, Yellow, Green, Blue, Indigo, Violet) take up (b).

Now theſe intervals or ſpaces ſubtending the differences of the refractions of the rays going to the Limits of thoſe colours, that is, to the points M, α, γ, ε, η, ι, λ, G, may, without any ſenſible error, be accounted proportional to the differences of the ſines of refraction of thoſe rays having one common ſine of incidence; and therefore, ſince the common ſine of incidence of the moſt and leaſt refrangible rays out of glaſs into air, was by a method deſcribed above, found in proportion to their ſines of refraction, as 50 to 77 and 78; divide the difference between the ſines of refraction, 77 ad 78, as the line GM is divided by thoſe intervals; and you will have 77, $77\frac{1}{8}$, $77\frac{1}{5}$, $77\frac{1}{3}$, $77\frac{1}{2}$, $77\frac{2}{3}$, $77\frac{7}{9}$, 78, the ſines of refraction of thoſe rays out of glaſs into air, their common ſine of incidence being 50 (c). So then the ſines of the incidences of all the Red-making rays out of glaſs into air, were to the ſines of their refractions, not greater than 50 to 77, nor leſs than 50 to $77\frac{1}{8}$; but they varied from one another according to all intermediate proportions. And the ſines of the incidences of the Green-making rays were to the ſines of their refractions in all proportions from that of 50 to $77\frac{1}{3}$, unto that of 50 to $77\frac{1}{2}$. And

(b) Vid. Lect. Opt. Part II. § 119—123.

(c) Lect. Opt. Part II. § 124—127.

(a) —— *that it emergeth in lines parallel to thoſe in which it was incident.*] Rather, *that the ſeveral colorific rays emerge altogether, in a line parallel to that in which the compound ray was incident.* For the Author's meaning is, that if ſucceeding refractions collect the colorific rays, which were ſeparated one from another by the firſt, into a ſingle point upon the laſt ſurface, from which they

And by the like limits abovementioned were the refractions of the rays belonging to the reſt of the colours defined; the ſines of the Red-making rays extending from 77 to $77\frac{1}{8}$; thoſe of the Orange-making, from $77\frac{1}{8}$ to $77\frac{1}{5}$; thoſe of the Yellow-making, from $77\frac{1}{5}$ to $77\frac{1}{3}$; thoſe of the Green-making, from $77\frac{1}{3}$ to $77\frac{1}{2}$; thoſe of the Blue-making, from $77\frac{1}{2}$ to $77\frac{2}{3}$; thoſe of the Indigo-making, from $77\frac{2}{3}$ to $77\frac{7}{9}$, and thoſe of the Violet from $77\frac{7}{9}$ to 78. ſeveral Rays defined.

Theſe are the laws of the refractions made out of glaſs into air, and thence by the third Axiom of the firſt Part of this Book, the laws of the refractions made out of air into glaſs are eaſily derived.

Exper. 8. I found moveover that when light goes out of air through ſeveral contiguous refracting mediums, as through water and glaſs, and thence goes out again into air, whether the refracting ſuperficies be parallel or inclined to one another, that light as often as by contrary refractions it is ſo corrected, that it emergeth in lines parallel to thoſe in which it was incident (d), continues ever after to be White. But if the emergent rays be inclined to the incident, the whiteneſs of the emerging light will by degrees in paſſing on from the place of emergence, become tinged in its edges with colours. This I tried by refracting light with priſms of glaſs placed within a priſmatick veſſel of water. Now thoſe colours argue a diverging and ſeparation of the heterogeneous rays from one another by means of their unequal refractions; as in what follows will more fully appear. And on the contrary, the permanent Whiteneſs argues, that in like incidences of the rays there is no ſuch ſeparation of the emerging rays, and by conſequence no inequality of their whole refractions. Whence I ſeem to gather the two following Theorems.

I. The exceſſes of the ſines of refraction of ſeveral ſorts of rays above their common ſine of incidence, when the refractions are made out of divers denſer mediums immediately into one and

they are to emerge, and ſend them out from thence in a direction parallel to that, in which the compound ray was incident; the emergent light will ever after, unleſs reſolved again by ſome new refraction, continue to be white. If the colorific rays emerge from different points in the laſt refracting ſurface, notwithſtanding that they go forth in parallel directions, each will preſerve its proper colour: as our Author hath fully ſhewn in his Lect. Opt. Part II. Sect. IV.

The refrangibility of the

the ſame rarer medium, ſuppoſe of air, are to one another in a given proportion (e).

II. The proportion of the ſine of incidence to the ſine of refraction of one and the ſame ſort of rays out of one medium into another, is compoſed of the proportion of the ſine of incidence to the ſine of refraction out of the firſt medium into any third medium, and of the proportion of the ſine of incidence to the ſine of refraction out of that third medium into the ſecond medium (f).

By the firſt Theorem the refractions of the rays of every ſort, made out of any medium into air, are known by having the refraction of the rays of any one ſort. As for inſtance, if the refractions of the rays of every ſort out of rain-water into air be deſired, let the common ſine of incidence out of glaſs into air be ſubducted from the ſines of refraction, and the exceſſes will be 27, $27\frac{1}{8}$, $27\frac{1}{5}$, $27\frac{1}{3}$, $26\frac{1}{2}$, $27\frac{2}{3}$, $27\frac{7}{9}$, 28. Suppoſe now that the ſine of incidence of the leaſt refrangible rays be to their ſine of refraction out of rain-water into air as 3 to 4; and ſay as 1, the difference of thoſe ſines, is to 3, the ſine of incidence, ſo is 27, the leaſt of the exceſſes above-mentioned, to a fourth number, 81; and 81 will be the common ſine of incidence out of rain-water into air; to which ſine if you add all the above-mentioned exceſſes, you will have the deſired ſines of the refractions 108, $108\frac{1}{8}$, $108\frac{1}{5}$, $108\frac{1}{3}$, $108\frac{1}{2}$, $108\frac{2}{3}$, $108\frac{7}{9}$, 109.

By the latter Theorem the refraction out of one medium into another is gathered, as often as you have the refractions out of them both into any third medium. As if the ſine of incidence of any ray out of glaſs into air be to its ſine of refraction as 20 to 31, and the ſine of incidence of the ſame ray out of air into water be to its ſine of refraction as 4 to 3; the ſine of incidence of that ray out of glaſs into water will be to its ſine of refraction as 20 to 31, and 4 to 3 jointly; that is, as the factum of 20 and 4 to the factum of 31 and 3, or as 80 to 93.

And theſe Theorems being admitted into Opticks, there would be ſcope enough of handling that ſcience voluminouſly after a new manner; not only by teaching thoſe things which tend to the

(e) Vide Lect. Opt. Part. I. § 44.

perfection

perfection of viſion, but alſo by determining mathematically all kinds of phænomena of colours which could be produced by refractions. For to do this, there is nothing elſe requiſite than to find out the ſeparations of heterogeneous rays, and their various mixtures, and proportions in every mixture. By this way of arguing I invented almoſt all the phænomena deſcribed in theſe books, beſide ſome others leſs neceſſary to the argument; and by the ſucceſſes I met with in the trials, I dare promiſe, that to him who ſhall argue truely, and then try all things with good glaſſes and ſufficient circumſpection, the expected event will not be wanting. But he is firſt to know what colours will ariſe from any others mixed in any aſſigned proportion.

Several Rays defined.

PROP. IV. THEOR. III.

Colours may be produced by compoſition, which ſhall be like to the colours of Homogeneal Light as to the appearance of colour, but not as to the immutability of colour and conſtitution of light. And thoſe colours, by how much they are more compounded, by ſo much are they leſs full and intenſe; and, by too much compoſition, they may be diluted and weakened till they ceaſe, and the mixture becomes white or grey. There may be alſo colours produced by compoſition, which are not fully like any of the colours of Homogeneal Light.

Colours made by Compoſition.

For a mixture of Homogeneal Red and Yellow compounds an Orange, like in appearance of colour to that Orange, which in the ſeries of unmixed priſmatick colours lies between them; but the light of one Orange is Homogeneal as to refrangibility, that of the other is Heterogeneal; and the colour of the one, if viewed through a priſm, remains unchanged; that of the other is changed and reſolved into its component colours, Red and Yellow. And after the ſame manner other neighbouring homogeneal colours may compound new colours, like the intermediate homogeneal ones; as Yellow and Green, the colour between them both; and afterwards, if Blue be added, there will be made a Green, the middle colour of the three which enter the compoſi-

(f) Vide Lect. Opt. Part. I § 34 & 35.

tion:

Colours made by Compoſition.

tion: for the Yellow and Blue on either hand, if they are equal in quantity, they draw the intermediate Green equally towards themſelves in compoſition, and ſo keep it as it were *in æquilibrio*, that it verge not more to the Yellow on the one hand, than to the Blue on the other, but by their mixed actions remain ſtill a middle colour. To this mixed Green there may be farther added ſome Red and Violet; and yet the Green will not preſently ceaſe, but only grow leſs full and vivid; and by increaſing the Red and Violet it will grow more and more dilute, until, by the prevalence of the added colours, it be overcome and turned into whiteneſs, or ſome other colour. So if to the colour of any homogeneal light, the ſun's white light compoſed of all ſorts of rays be added, that colour will not vaniſh, or change its ſpecies, but be diluted; and, by adding more and more White, it will be diluted more and more perpetually. Laſtly, if Red and Violet be mingled, there will be generated, according to their various proportions, various Purples, ſuch as are not like in appearance to the colour of any Homogeneal Light; and of theſe Purples mixed with Yellow and Blue may be made other new colours.

PROP. V. THEOR. IV.

Whiteneſs, and all Grey colours between white and black, may be compounded of colours, and the whiteneſs of the ſun's light is compounded of all the Primary colours mixed in a due proportion.

The Proof by Experiments.

The whiteneſs of the Solar Light

Exper. 9. The ſun ſhining into a dark chamber through a little round hole in the window-ſhut, and his light being there refracted by a priſm to caſt his coloured image PT [in *fig.* 5.] upon the oppoſite wall: I held a white paper, v, to that image, in ſuch manner that it might be illuminated by the coloured light reflected from thence, and yet not intercept any part of that light in its paſſage from the priſm to the ſpectrum. And I found that when the paper was held nearer to any colour than to the reſt, it appeared of that colour to which it approached neareſt; but when it was equally or almoſt equally diſtant from all the colours, ſo that

that it might be equally illuminated by them all, it appeared White. And in this laſt ſituation of the paper, if ſome colours were intercepted, the paper loſt its White colour, and appeared of the colour of the reſt of the light which was not intercepted. So then the paper was illuminated with lights of various colours, namely, Red, Yellow, Green, Blue and Violet; and every part of the light retained its proper colour, until it was incident on the paper, and became reflected thence to the eye; ſo that if it had been either alone, the reſt of the light being intercepted, or if it had abounded moſt and been predominant in the light reflected from the paper, it would have tinged the paper with its own colour; and yet being mixed with the reſt of the colours in a due proportion, it made the paper look White, and therefore by a compoſition with the reſt produced that colour. The ſeveral parts of the coloured light reflected from the ſpectrum, whilſt they are propagated from thence through the air, do perpetually retain their proper colours; becauſe wherever they fall upon the eyes of any ſpectator, they make the ſeveral parts of the ſpectrum to appear under their proper colours. They retain therefore their proper colours, when they fall upon the paper, v, and ſo by the confuſion and perfect mixture of thoſe colours compound the Whiteneſs of the light reflected from thence. an effect of Compoſition.

Exper. 10. Let that ſpectrum or ſolar image PT [in *fig.* 6.] fall now upon the lens MN, above four inches broad, and about ſix feet diſtant from the priſm ABC, and ſo figured, that it may cauſe the coloured light, which divergeth from the priſm, to converge and meet again at its focus G; about ſix or eight feet diſtant from the lens, and there to fall perpendicularly upon a white paper, DE. And if you move this paper to and fro, you will perceive that near the lens, as at *de*, the whole ſolar image, ſuppoſe at *pt*, will appear upon it intenſely coloured after the manner above-explained; and that by receding from the lens, thoſe colours will perpetually come towards one another, and by mixing more and more dilute one another continually, until at length the paper come to the focus G, where by a perfect mixture they will wholly vaniſh, and be converted into Whiteneſs; the whole light appearing now upon the paper like a little white circle. And afterwards,

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wards, by receding farther from the lens, the rays, which before converged, will now croſs one another in the focus G, and diverge from thence, and thereby make the colours to appear again, but yet in a contrary order; ſuppoſe at $\delta\varepsilon$, where the Red t is now above which before was below, and the Violet p is below which before was above.

Let us now ſtop the paper at the focus G, where the light appears totally white and circular, and let us conſider its Whiteneſs. I ſay, that this is compoſed of the converging colours. For if any of thoſe colours be intercepted at the lens, the Whiteneſs will ceaſe, and degenerate into that colour, which ariſeth from the Compoſition of the other colours which are not intercepted. And then if the intercepted colours be let paſs and fall upon that compound colour, they mix with it, and by their mixture reſtore the Whiteneſs. So if the Violet, Blue and Green be intercepted, the remaining Yellow, Orange and Red will compound upon the paper an Orange; and then if the intercepted colours be let paſs, they will fall upon this compound Orange, and together with it decompound a White. So alſo if the Red and Violet be intercepted, the remaining Yellow, Green and Blue, will compound a Green upon the paper; and then the Red and Violet being let paſs will fall upon this Green, and together with it decompound a White. And that in this compoſition of White the ſeveral rays do not ſuffer any change in their colorific qualities, by acting upon one another, but are only mixed, and by a mixture of their colours produce White, may farther appear by theſe Arguments.

If the paper be placed beyond the focus G, ſuppoſe at $\delta\varepsilon$, and then the Red colour at the lens be alternately intercepted, and let paſs again, the Violet colour on the paper will not ſuffer any change thereby; as it ought to do, if the ſeveral ſorts of rays acted upon one another in the focus G, where they croſs. Neither will the Red upon the paper be changed by any alternate ſtopping, and letting paſs the Violet which croſſeth it.

And if the paper be placed at the focus G, and the white round image at G be viewed through the priſm HIK, and by the refraction of that priſm be tranſlated to the place rv, and there appear tinged with various colours, namely, the Violet at v and

Red

Red at *r*, and others between; and then the Red colour at the lens be often ſtopped and let paſs by turns, the Red at *r* will accordingly diſappear and return as often, but the Violet at *v* will not thereby ſuffer any change. And ſo by ſtopping and letting paſs alternately the Blue at the lens, the Blue at *r* will accordingly diſappear and return, without any change made in the Red at *r*. The Red therefore depends on one ſort of rays, and the Blue on another ſort, which in the focus G, where they are commixed, do not act on one another. And there is the ſame reaſon of the other colours. the effect of Compoſition.

I conſidered farther, that when the moſt refrangible rays, P*p*, and the leaſt refrangible ones, T*t*, are by converging inclined to one another; the paper, if held very oblique to thoſe rays in the focus, G, might reflect one ſort of them more copiouſly than the other ſort; and by that means the reflected light would be tinged in that focus with the colour of the predominant rays; provided thoſe rays ſeverally retained their colours or colorific qualities, in the compoſition of White made by them in that focus. But if they did not retain them in that White, but became all of them ſeverally endued there with a diſpoſition to ſtrike the ſenſe with the perception of White, then they could never loſe their Whiteneſs by ſuch reflexions. I inclined therefore the paper to the rays very obliquely, as in the ſecond Experiment of this Book, that the moſt refrangible rays might be more copiouſly reflected than the reſt, and the Whiteneſs at length changed ſucceſſively into Blue, Indigo and Violet. Then I inclined it the contrary way, that the leaſt refrangible rays might be more copious in the reflected light than the reſt, and the Whiteneſs turned ſucceſſively to Yellow, Orange and Red.

Laſtly, I made an inſtrument XY in faſhion of a comb, whoſe teeth, being in number ſixteen, were about an inch and an half broad, and the intervals of the teeth about two inches wide. Then by interpoſing ſucceſſively the teeth of this inſtrument near the lens, I intercepted part of the colours by the interpoſed tooth, whilſt the reſt of them went on through the interval of the teeth to the paper DE, and there painted a round Solar image. But the paper I had firſt placed ſo, that the image might appear

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White, as often as the comb was taken away; and then the comb being as was ſaid interpoſed, that Whiteneſs, by reaſon of the intercepted part of the colours at the lens, did always change into the colour compounded of thoſe colours which were not intercepted; and that colour was, by the motion of the comb, perpetually varied; ſo that in the paſſing of every tooth over the lens all theſe colours, Red, Yellow, Green, Blue and Purple, did always ſucced one another. I cauſed therefore all the teeth to paſs ſucceſſively over the lens, and when the motion was ſlow, there appeared a perpetual ſucceſſion of the colours upon the paper: but if I ſo much accelerated the motion, that the colours, by reaſon of their quick ſucceſſion, could not be diſtinguiſhed from one another, the appearance of the ſingle colours ceaſed. There was no Red, no Yellow, no Green, no Blue, nor Purple to be ſeen any longer, but from a confuſion of them all there aroſe one uniform White colour. Of the light which now by the mixture of all the colours appeared White, there was no part really White. One part was Red, another Yellow, a third Green, a fourth Blue, a fifth Purple, and every part retains its proper colour till it ſtrike the ſenſorium. If the impreſſions follow one another ſlowly, ſo that they may be ſeverally perceived, there is made a diſtinct ſenſation of all the colours one after another in a continual ſucceſſion. But if the impreſſions follow one another ſo quickly that they cannot be ſeverally perceived, there ariſeth out of them all one common ſenſation, which is neither of this colour alone nor of that alone; but hath itſelf indifferently to them all, and this is a ſenſation of Whiteneſs. By the quickneſs of the ſucceſſions, the impreſſions of the ſeveral colours are confounded in the ſenſorium, and out of that confuſion ariſeth a mixed ſenſation. If a burning coal be nimbly moved round in a circle with gyrations continually repeated, the whole circle will appear like fire; the reaſon of which is, that the ſenſation of the coal, in the ſeveral places of that circle, remains impreſſed on the ſenſorium, until the coal return again to the ſame place. And ſo in a quick conſecution of the colours, the impreſſion of every colour remains in the ſenſorium, until a revolution of all the colours be compleated, and that firſt colour return again. The impreſſions

therefore of all the ſucceſſive colours are at once in the ſenſorium, and jointly ſtir up a ſenſation of them all; and ſo it is manifeſt by this Experiment, that the commixed impreſſions of all the colours do ſtir up and beget a ſenſation of White, that is, that Whiteneſs is compounded of all the colours. the effect of Compoſition.

And if the comb be now taken away, that all the colours may at once paſs from the lens to the paper, and be there intermixed, and together reflected thence to the ſpectators eyes; their impreſſions on the ſenſorium, being now more ſubtilly and perfectly commixed there, ought much more to ſtir up a ſenſation of Whiteneſs.

You may inſtead of the lens uſe two priſms [*fig.* 7.] HIK and LMN, which by refracting the coloured light the contrary way to that of the firſt refraction, may make the diverging rays converge and meet again in G; as you ſee repreſented in the ſeventh figure. For where they meet and mix they will compoſe a White light, as when a lens is uſed.

Exper. 11. Let the ſun's coloured image PT [in *fig.* 8.] fall upon the wall of a dark chamber, as in the third Experiment of the firſt Book, and let the ſame be viewed through a priſm, *abc*, held parallel to the priſm, ABC, by whoſe refraction that image was made, and let it now appear lower than before, ſuppoſe in the place s, over againſt the Red colour T. And if you go near to the image PT, the ſpectrum s will appear oblong and coloured like the image PT; but if you recede from it, the colours of the ſpectrum, s, will be contracted more and more, and at length vaniſh; that ſpectrum s becoming perfectly round and white: and if you recede yet farther, the colours will emerge again, but in a contrary order. Now that ſpectrum s appears White, in that caſe when the rays of ſeveral ſorts, which converge from the ſeveral parts of the image, PT, to the priſm *abc*, are ſo refracted unequally by it, that in their paſſage from the priſm to the eye, they may diverge from one and the ſame point of the ſpectrum s, and ſo fall afterwards upon one and the ſame point in the bottom of the eye, and there be mingled.

And farther, if the comb be here made uſe of, by whoſe teeth the colours at the image, PT, may be ſucceſſively intercepted;

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the ſpectrum s, when the comb is moved ſlowly, will be perpetually tinged with ſucceſſive colours: but when by accelerating the motion of the comb, the ſucceſſion of the colours is ſo quick that they cannot be ſeverally ſeen, that ſpectrum s, by a confuſed and mixed ſenſation of them all, will appear White.

Exper. 12. The ſun ſhining through a large priſm ABC [in *fig.* 9.] upon a comb XY, placed immediately behind the priſm, his light, which paſſed through the interſtices of the teeth, fell upon a white paper DE. The breadths of the teeth were equal to their interſtices, and ſeven teeth together with their interſtices took up an inch in breadth. Now when the paper was about two or three inches diſtant from the comb, the light, which paſſed through its ſeveral interſtices, painted ſo many ranges of colours, *kl*, *mn*, *op*, *qr*, &c. which were parallel to one another and contiguous, and without any mixture of White. And theſe ranges of colours, if the comb was moved continually up and down with a reciprocal motion, aſcended and deſcended in the paper, and when the motion of the comb was ſo quick, that the colours could not be diſtinguiſhed from one another, the whole paper, by their confuſion and mixture in the ſenſorium, appeared White.

Let the comb now reſt; and let the paper be removed farther from the priſm, and the ſeveral ranges of colours will be dilated and expanded into one another more and more, and, by mixing their colours, will dilute one another, and at length, when the diſtance of the paper from the comb is about a foot, or a little more (ſuppoſe in the place 2D2E) they will ſo far dilute one another as to become White.

With any obſtacle let all the light be now ſtopped, which paſſes through any one interval of the teeth; ſo that the range of colours which comes from thence may be taken away: and you will ſee the light of the reſt of the ranges to be expanded into the place of the range taken away, and there to be coloured. Let the intercepted range paſs on as before, and its colours, falling upon the colours of the other ranges, and mixing with them, will reſtore the Whiteneſs.

Let the paper 2D2E be now very much inclined to the rays, ſo that the moſt refrangible rays may be more copiouſly reflected than

than the reſt; and the White colour of the paper, through the exceſs of thoſe rays, will be changed into Blue and Violet. Let the paper be as much inclined the contrary way, that the leaſt refrangible rays may be now more copiouſly reflected than the reſt, and by their exceſs the Whiteneſs will be changed into Yellow and Red. The ſeveral rays therefore in that White light do retain their colorific qualities, by which thoſe of any ſort, whenever they become more copious than the reſt, do by their exceſs and predominance cauſe their proper colour to appear. the effect of Compoſition.

And by the ſame way of arguing, applied to the third Experiment of this Book, it may be concluded, that the White colour of all refracted light, at its very firſt emergence, where it appears as white as before its incidence, is compounded of various colours.

Exper. 13. In the foregoing Experiment the ſeveral Intervals of the teeth of the comb do the office of ſo many priſms, every interval producing the phænomenon of one priſm. Whence inſtead of thoſe intervals uſing ſeveral priſms, I tried to compound Whiteneſs by mixing their colours; and did it by uſing only three priſms, as alſo by uſing only two as before. Let two priſms, ABC and *abc* [in *fig.* 10.] whoſe refracting angles, B and *b*, are equal, be ſo placed parallel to one another, that the refracting angle B of the one may touch the angle *c* at the baſe of the other, and their planes, CB and *cb*, at which the rays emerge, may lie *in directum*. Then let the light trajected through them fall upon the paper MN, diſtant about 8 or 12 inches from the priſms. And the colours generated by the interior limits, B and *c*, of the two priſms, will be mingled at PT, and there compound White. For if either priſm be taken away, the colours made by the other will appear in that place PT; and when the priſm is reſtored to its place again, ſo that its colours may there fall upon the colours of the other, the mixture of them both will reſtore the Whiteneſs.

This Experiment ſucceeds alſo, as I have tried, when the angle *b* of the lower priſm is a little greater than the angle B of the upper, and between the interior angles B and *c*, there intercedes ſome ſpace B*c*, as is repreſented in the figure, and the refracting planes, BC and *bc*, are neither *in directum* nor parallel to one another.

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other. For there is nothing more requiſite to the ſucceſs of this Experiment, than that the rays of all ſorts may be uniformly mixed upon the paper, in the place PT. If the moſt refrangible rays, coming from the ſuperior priſm, take up all the ſpace from M to P; the rays of the ſame ſort, which come from the inferior priſm, ought to begin at P, and take up all the reſt of the ſpace from thence towards N. If the leaſt refrangible rays, coming from the ſuperior priſm, take up the ſpace MT; the rays of the ſame kind, which come from the other priſm, ought to begin at T, and take up the remaining ſpace TN. If one ſort of the rays which have intermediate degrees of refrangibility, and come from the ſuperior priſm, be extended through the ſpace MQ, and another ſort of thoſe rays through the ſpace MR, and a third ſort of them through the ſpace MS; the ſame ſorts of rays, coming from the lower priſm, ought to illuminate the remaining ſpaces QN, RN, SN, reſpectively. And the ſame is to be underſtood of all the other ſorts of rays. For thus the rays of every ſort will be ſcattered uniformly and evenly through the whole ſpace MN, and ſo being every where mixed in the ſame proportion, they muſt every where produce the ſame colours. And therefore ſince by this mixture they produce White in the exterior ſpaces MP and TN, they muſt alſo produce White in the interior ſpace PT. This is the reaſon of the compoſition by which Whiteneſs was produced in this Experiment; and by what other way ſoever I made the like Compoſition, the reſult was Whiteneſs.

Laſtly, if with the teeth of a comb of a due ſize, the coloured lights of the two priſms, which fall upon the ſpace PT, be alternately intercepted; that ſpace PT, when the motion of the comb is ſlow, will always appear coloured. But by accelerating the motion of the comb ſo much, that the ſucceſſive colours cannot be diſtinguiſhed from one another, it will appear White.

Exper. 14. Hitherto I have produced Whiteneſs by mixing the colours of priſms. If now the colours of Natural bodies are to be mingled, let water a little thickened with ſoap be agitated to raiſe a froth; and after that froth has ſtood a little, there will appear to one, that ſhall view it intently, various colours every where in the ſurfaces of the ſeveral bubbles; but to one that

ſhall

ſhall go ſo far off that he cannot diſtinguiſh the colours from one another, the whole froth will grow White with a perfect whiteneſs. the effect of Compoſition.

Exper. 15. Laſtly, in attempting to compound a White by mixing the coloured powders, which painters uſe; I conſidered that all coloured powders do ſuppreſs and ſtop in them a very conſiderable part of the light, by which they are illuminated. For they become coloured by reflecting the light of their own colours more copiouſly, and that of all other colours more ſparingly; and yet they do not reflect the light of their own colours ſo copiouſly as White bodies do. If red lead, for inſtance, and a white paper, be placed in the red light of the coloured ſpectrum, made in a dark chamber by the refraction of a priſm, as is deſcribed in the third Experiment of the firſt Book; the paper will appear more lucid than the red lead, and therefore reflects the Red-making rays more copiouſly than red lead doth. And if they be held in the light of any other colour, the light reflected by the paper will exceed the light reflected by the red lead in a much greater proportion. And the like happens in powders of other colours. And therefore by mixing ſuch powders we are not to expect a ſtrong and full White, ſuch as is that of paper, but ſome duſky obſcure one, ſuch as might ariſe from a mixture of light and darkneſs, or from White and Black; that is, a Grey, or Dun, or Ruſſet-brown, ſuch as are the colours of a man's nail, of a mouſe, of aſhes, of ordinary ſtones, of mortar, of duſt and dirt in highways, and the like. And ſuch a dark White I have often produced by mixing coloured powders. For thus one part of red lead, and five parts of *Viride Æris*, compoſed a dun colour like that of a mouſe. For theſe two colours were ſeverally ſo compounded of others, that in both together were mixtures of all colours; and there were leſs red lead uſed than *Viride Æris*, becauſe of the fullneſs of its colour. Again, one part of red lead, and four parts of blue biſe, compoſed a dun colour verging a little to Purple; and by adding to this a certain mixture of Orpiment and *Viride Æris* in a due proportion, the mixture loſt its purple tincture, and became perfectly Dun. But the Experiment ſucceeded beſt without Minium thus. To Orpiment I

added

The Whiteneſs of the Solar Light

added by little and little a certain full bright Purple, which painters uſe, until the orpiment ceaſed to be Yellow, and became of a pale Red. Then I diluted that Red by adding a little *Viride Æris*, and a little more blue biſe than *Viride Æris*, until it became of ſuch a grey or pale White, as verged to no one of the colours more than to another. For thus it became of a colour equal in whiteneſs to that of aſhes, or of wood newly cut, or of a man's ſkin. The orpiment reflected more light than did any other of the powders, and therefore conduced more to the Whiteneſs of the compounded colour than they. To aſſign the proportions accurately may be difficult, by reaſon of the different goodneſs of powders of the ſame kind. Accordingly as the colour of any powder is more or leſs full and luminous, it ought to be uſed in a leſs or greater proportion.

Now conſidering that theſe grey and dun colours may be alſo produced by mixing Whites and Blacks, and by conſequence differ from perfect Whites not in ſpecies of colours, but only in degree of luminouſneſs, it is manifeſt that there is nothing more requiſite to make them perfectly white than to increaſe their light ſufficiently; and, on the contrary, if by increaſing their light they can be brought to perfect Whiteneſs, it will thence alſo follow, that they are of the ſame ſpecies of colour with the beſt Whites, and differ from them only in the quantity of light. And this I tried as follows. I took the third of the above-mentioned grey mixtures (that which was compounded of Orpiment, Purple, Biſe and *Viride Æris*) and rubbed it thickly upon the floor of my chamber, where the ſun ſhone upon it through the opened caſement; and by it, in the ſhadow, I laid a piece of white paper of the ſame bigneſs. Then going from them to the diſtance of 12 or 18 feet, ſo that I could not diſcern the unevenneſs of the ſurface of the powder, nor the little ſhadows let fall from the gritty particles thereof; the powder appeared intenſely White, ſo as to tranſcend even the paper itſelf in Whiteneſs, eſpecially if the paper were a little ſhaded from the light of the clouds; and then the paper compared with the powder appeared of ſuch a grey colour as the powder had done before. But by laying the paper where the ſun ſhines through the glaſs of the window, or by ſhutting

ſhutting the window, that the ſun might ſhine through the glaſs upon the powder, and by ſuch other fit means of increaſing or decreaſing the lights, wherewith the powder and paper were illuminated; the light wherewith the powder is illuminated may be made ſtronger, in ſuch a due proportion, than the light wherewith the paper is illuminated, that they ſhall both appear exactly alike in Whiteneſs. For when I was trying this, a friend coming to viſit me, I ſtopped him at the door; and before I told him what the colours were, or what I was doing, I aſked him, Which of the two Whites were the beſt, and wherein they differed? And after he had at that diſtance viewed them well, he anſwered, That they were both good Whites, and that he could not ſay which was beſt, nor wherein their colours differed. Now if you conſider, that this White of the powder in the ſun-ſhine was compounded of the colours which the component powders, Orpiment, Purple, Biſe, and *Viride Æris*, have in the ſame ſun-ſhine; you muſt acknowledge by this Experiment, as well as by the former, that perfect Whiteneſs may be compounded of colours. the effect of Compoſition.

From what has been ſaid it is alſo evident, that the Whiteneſs of the ſun's light is compounded of all the colours wherewith the ſeveral ſorts of rays, whereof that light conſiſts, when by their ſeveral refrangibilities they are ſeparated from one another, do tinge paper or any other white body whereon they fall. For thoſe colours, by Prop. II. are unchangeable, and whenever all thoſe rays, with thoſe their colours, are mixed again, they reproduce the ſame White light as before.

PROP. VI. PROB. II.

In a mixture of Primary Colours, the quantity and quality of each being given, to know the colour of the compound.

With the center O [in *fig.* 11.] and radius OD deſcribe a circle ADF; and diſtinguiſh its circumference into ſeven parts, DE, EF, FG, GA, AB, BC, CD, proportional to the ſeven muſical tones or intervals of the eight ſounds, *ſol*, *la*, *fa*, *ſol*, *la*, *mi*, *fa*, *ſol*, contained in an eight; that is, proportional to the numbers $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{10}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{16}$, $\frac{1}{9}$. Let the firſt part, DE, repreſent a Red colour; the ſe-

To aſcertain the Colour of a

cond, EF, Orange; the third, FG, Yellow; the fourth, GA, Green; the fifth, AB, Blue; the ſixth, BC, Indigo; and the ſeventh, CD, Violet. And conceive that theſe are all the colours of uncompounded light gradually paſſing into one another, as they do when made by priſms; the circumference DEFGABCD, repreſenting the whole ſeries of colours from one end of the ſun's coloured image to the other; ſo that from D to E be all degrees of Red; at E, the mean colour between Red and Orange; from E to F, all degrees of Orange; at F, the mean between Orange and Yellow; from F to G, all degrees of Yellow, and ſo on. Let *p* be the center of gravity of the arch DE, and *q*, *r*, *s*, *t*, *u*, *x*, the centers of gravity of the arches EF, FG, GA, AB, BC and CD reſpectively; and about thoſe centers of gravity let circles, proportional to the number of rays of each colour in the given mixture, be deſcribed; that is, the circle *p* proportional to the number of the Red-making rays in the mixture; the circle *q* proportional to the number of the Orange-making rays in the mixture, and ſo of the reſt. Find the common center of gravity of all thoſe circles *p*, *q*, *r*, *s*, *t*, *u*, *x*. Let that center be Z; and from the center of the circle ADF, through Z to the circumference, drawing the right line OY, the place of the point Y in the circumference ſhall ſhew the colour ariſing from the compoſition of all the colours in the given mixture; and the line OZ ſhall be proportional to the fulneſs or intenſeneſs of the colour, that is, to its diſtance from Whiteneſs. As if Y fall in the middle between F and G, the compounded colour ſhall be the beſt Yellow; if Y verge from the middle towards F or G, the compound colour ſhall accordingly be a Yellow, verging towards Orange or Green. If Z fall upon the circumference, the colour ſhall be intenſe and florid in the higheſt degree; if it fall in the mid-way between the circumference and center, it ſhall be but half ſo intenſe; that is, it ſhall be ſuch a colour as would be made by diluting the intenſeſt Yellow with an equal quantity of Whiteneſs; and if it fall upon the center O, the colour ſhall have loſt all its intenſeneſs, and become a White. But it is to be noted, that if the point Z fall in or near the line OD, the main ingredients being the Red and Violet, the colour compounded ſhall not be any of the Priſmatick colours, but a Purple, inclining

to

to Red or Violet, accordingly as the point z lieth on the ſide of the line DO towards E or towards C; and in general the compounded Violet is more bright and more fiery than the uncompounded. Alſo if only two of the Primary colours, which in the circle are oppoſite to one another, be mixed in an equal proportion, the point Z ſhall fall upon the center O, and yet the Colour compounded of thoſe two ſhall not be perfectly White, but ſome faint anonymous colour. For I could never yet by mixing only two Primary colours produce a perfect White. Whether it may be compounded of a mixture of three, taken at equal diſtances in the circumferences, I do not know; but of four or five I do not much queſtion but it may. But theſe are curioſities of little or no moment to the underſtanding the phænomena of Nature. For in all Whites, produced by Nature, there uſes to be a mixture of all ſorts of rays, and by conſequence a compoſition of all colours. Compound of Primary Colours.

To give an inſtance of this rule; ſuppoſe a colour is compounded of theſe Homogeneal colours, of Violet one part, of Indigo one part, of Blue two parts, of Green three parts, of Yellow five parts, of Orange ſix parts, and of Red ten parts. Proportional to theſe parts deſcribe the circles x, v, t, s, r, q, p, reſpectively; that is, ſo that if the circle x be one, the circle v may be one, the circle t two, the circle s three, and the circles r, q and p, five, ſix and ten. Then I find Z the common center of gravity of theſe circles; and through Z drawing the line OY, the point Y falls upon the circumference between E and F, ſome thing nearer to E than to F; and thence I conclude, that the colour compounded of theſe ingredients will be an Orange, verging a little more to Red than to Yellow. Alſo I find that OZ is a little leſs than one half of OY; and thence I conclude, that this Orange hath a little leſs than half the fulneſs or intenſeneſs of an uncompounded Orange; that is to ſay, that it is ſuch an Orange, as may be made by mixing an Homogeneal Orange with a good White, in the proportion of the line OZ to the line ZY; this proportion being not of the quantities of mixed orange and white powders, but of the quantities of the lights reflected from them.

This Rule I conceive accurate enough for practice, though not mathematically accurate; and the truth of it may be ſufficiently

proved to ſenſe, by ſtopping any of the colours at the lens in the tenth Experiment of this Book. For the reſt of the colours, which are not ſtopped, but paſs on to the focus of the lens, will there compound, either accurately or very nearly, ſuch a colour, as by this Rule ought to reſult from their mixture.

PROP. VII. THEOR. V.

All the colours in the univerſe which are made by light, and depend not on the power of imagination, are either the colours of Homogeneal Lights, or compounded of theſe; and that either accurately or very nearly, according to the Rule of the foregoing Problem.

All Colours either

For it has been proved (in Prop. I. Part II.) that the changes of colours, made by refractions, do not ariſe from any new modifications of the rays impreſſed by thoſe refractions, and by the various terminations of light and ſhadow, as has been the conſtant and general opinion of philoſophers. It has alſo been proved, that the ſeveral colours of the Homogeneal Rays do conſtantly anſwer to their degrees of refrangibility (Prop. I. Part I. and Prop. II. Part II.) and that their degrees of refrangibility cannot be changed by refractions and reflexions (Prop. II. Part I.) and by conſequence that thoſe their colours are likewiſe immutable. It has alſo been proved directly, by refracting and reflecting Homogeneal Lights apart, that their colours cannot be changed (Prop. II. Part II.) It has been proved alſo, that when the ſeveral ſorts of rays are mixed, and in croſſing paſs through the ſame ſpace, they do not act on one another ſo as to change each others colorific qualities (Exper. 10. Part II.) but, by mixing their actions in the ſenſorium, beget a ſenſation differing from what either would do apart, that is a ſenſation of a mean colour between their proper colours; and particularly when by the concourſe and mixtures of all ſorts of rays, a White colour is produced, the White is a mixture of all the colours, which the rays would have apart (Prop. v. Part. II.) The rays in that mixture do not loſe or alter their ſeveral colorific qualities, but by all their various kinds of actions, mixed in the ſenſorium, beget a ſenſation of a middling colour between

between all their colours, which is Whiteneſs. For Whiteneſs is a mean between all colours, having itſelf indifferently to them all, ſo as with equal facility to be tinged with any of them. A Red powder mixed with a little Blue, or a Blue with a little Red, doth not preſently loſe its colour; but a White powder mixed with any colour is preſently tinged with that colour, and is equally capable of being tinged with any colour whatever. It has been ſhewed alſo, that as the ſun's light is mixed of all ſorts of rays, ſo its Whiteneſs is a mixture of the colours of all ſorts of rays; thoſe rays having from the beginning their ſeveral colorific qualities, as well as their ſeveral refrangibilities, and retaining them perpetually unchanged, notwithſtanding any refractions or reflexions they may at any time ſuffer; and that whenever any ſort of the ſun's rays is by any means (as by reflexion in Exper. 9 and 10. Part I. or by refraction as happens in all refractions) ſeparated from the reſt, then they manifeſt their proper colours. Theſe things have been proved: and the ſum of all this amounts to the Propoſition here to be proved. For if the ſun's light is mixed of ſeveral ſorts of rays, each of which have originally their ſeveral refrangibilities and colorific qualities, and notwithſtanding their refractions and reflexions, and their various ſeparations or mixtures, keep thoſe their original properties perpetually the ſame without alteration; then all the colours in the world muſt be ſuch, as conſtantly ought to ariſe from the original colorific qualities of the rays, whereof the lights conſiſt, by which thoſe colours are ſeen. And therefore if the reaſon of any colour whatever be required, we have nothing elſe to do, than to conſider how the rays in the ſun's light have, by reflexions or refractions, or other cauſes, been parted from one another, or mixed together; or otherwiſe to find out what ſorts of rays are in the light, by which that colour is made, and in what proportion; and then, by the laſt Problem, to learn the colour which ought to ariſe by mixing thoſe rays, or their colours, in that proportion. I ſpeak here of colours ſo far as they ariſe from light. For they appear ſometimes by other cauſes; as when by the power of phantaſy we ſee colours in a dream; or a mad-man ſees things before him which are not there; or when we ſee fire

Homogeneal or Compounded.

by ſtriking the eye; or ſee colours like the eye of a peacock's feather, by preſſing our eyes in either corner, whilſt we look the other way. Where theſe and ſuch like cauſes interpoſe not, the colour always anſwers to the ſort or ſorts of the rays, whereof the light conſiſts; as I have conſtantly found in whatever phænomena of colours I have hitherto been able to examine. I ſhall in the following Propoſitions give inſtances of this in the phænomena of chiefeſt note.

PROP. VIII. PROB. III.

By the diſcovered properties of light to explain the colours made by priſms.

The Priſmatic Let ABC [in *fig.* 12.] repreſent a priſm refracting the light of the ſun, which comes into a dark chamber, through a hole, FG, almoſt as broad as the priſm; and let MN repreſent a white paper, on which the refracted light is caſt; and ſuppoſe the moſt refrangible, or deepeſt Violet-making, rays fall upon the ſpace Pπ; the leaſt Refrangible or deepeſt Red-making rays, upon the ſpace Tϛ; the middle ſort between the Indigo-making and Blue-making rays, upon the ſpace Qχ; the middle ſort of the Green-making rays upon the ſpace Rϱ; the middle ſort between the Yellow-making and Orange-making rays, upon the ſpace sσ; and other intermediate ſorts, upon intermediate ſpaces. For ſo the ſpaces, upon which the ſeveral ſorts adequately fall, will, by reaſon of the different refrangibility of thoſe ſorts, be one lower than another. Now if the paper, MN, be ſo near the priſm, that the ſpaces PT and πϛ do not interfere with one another; the diſtance between them, Tπ, will be illuminated by all the ſorts of rays, in that proportion to one another which they have at their very firſt coming out of the priſm, and conſequently be White. But the ſpaces PT and πϛ on either hand, will not be illuminated by them all, and therefore will appear coloured. And particularly at P, where the outmoſt Violet-making rays fall alone, the colour muſt be the deepeſt Violet. At Q, where the Violet-making and Indigo-making rays are mixed, it muſt be a Violet inclining much to Indigo. At R, where the Violet-making, Indigo-making, Blue-making,

making, and one half of the Green-making rays are mixed, their colours muſt (by the conſtruction of the ſecond Problem) compound a middle colour between Indigo and Blue. At s, where all the rays are mixed except the Red-making and Orange-making, their colours ought by the ſame rule to compound a faint Blue, verging more to Green than Indigo. And in the progreſs from s to T, this Blue will grow more and more faint and dilute, till at T, where all the colours begin to be mixed, it ends in Whiteneſs. Colours explained.

So again, on the other ſide of the White, at τ, where the leaſt refrangible or outmoſt Red-making rays are alone, the colour muſt be the deepeſt Red. At σ, the mixture of Red and Orange will compound a Red inclining to Orange. At ϱ, the mixture of Red, Orange, Yellow, and one half of the Green muſt compound a middle colour between Orange and Yellow. At χ, the mixture of all colours but Violet and Indigo will compound a faint Yellow, verging more to Green than to Orange. And this Yellow will grow more faint and dilute continually in its progreſs from χ to π, where by a mixture of all ſorts of rays it will become White.

Theſe colours ought to appear, were the ſun's light perfectly White: but becauſe it inclines to Yellow, the exceſs of the Yellow-making rays, whereby it is tinged with that colour, being mixed with the faint Blue between s and T, will draw it to a faint Green. And ſo the colours in order from P to τ ought to be Violet, Indigo, Blue, very faint Green, White, faint Yellow, Orange, Red. Thus it is by the computation: and they that pleaſe to view the colours made by a priſm, will find it ſo in Nature.

Theſe are the colours on both ſides the White, when the paper is held between the priſm and the point X, where the colours meet and the interjacent white vaniſhes. For if the paper be held ſtill farther off from the priſm, the moſt refrangible and leaſt refrangible rays will be wanting in the middle of the light, and the reſt of the rays which are found there, will by mixture produce a fuller Green than before. Alſo the Yellow and Blue will now become leſs compounded, and by conſequence more intenſe than before. And this alſo agrees with experience.

And

The Priſmatic

And if one look through a priſm upon a white object encompaſſed with blackneſs or darkneſs, the reaſon of the colours ariſing on the edges is much the ſame; as will appear to one that ſhall a little conſider it. If a black object be encompaſſed with a white one, the colours which appear through the priſm are to be derived from the light of the white one, ſpreading into the regions of the black; and therefore they appear in a contrary order to that, when a white object is ſurrounded with black. And the ſame is to be underſtood when an object is viewed, whoſe parts are ſome of them leſs luminous than others. For in the borders of the more and leſs luminous parts, colours ought always, by the ſame principles, to ariſe from the exceſs of the light of the more luminous, and to be of the ſame kind as if the darker parts were black, but yet to be more faint and dilute.

What is ſaid of colours made by priſms may be eaſily applied to colours made by the glaſſes of teleſcopes or microſcopes, or by the humours of the eye. For if the object-glaſs of a teleſcope be thicker on one ſide than on the other, or if one half of the glaſs, or one half of the pupil of the eye be covered with any opake ſubſtance: the object-glaſs, or that part of it, or of the eye, which is not covered, may be conſidered as a wedge with crooked ſides; and every wedge of glaſs, or other pellucid ſubſtance, has the effect of a priſm in refracting the light which paſſes through it (g).

How the colours in the ninth and tenth Experiments of the firſt Part ariſe from the different reflexibility of light, is evident by what was there ſaid. But it is obſervable in the ninth Experiment, that whilſt the ſun's direct light is Yellow, the exceſs of the Blue-making rays in the reflected beam of light MN, ſuffices only to bring that Yellow to a pale White inclining to Blue, and not to tinge it with a manifeſtly Blue colour. To obtain therefore a better Blue, I uſed inſtead of the Yellow light of the ſun the White light of the clouds, by varying a little the Experiment, as follows.

Exper. 16. Let HFG [in *fig.* 13.] repreſent a priſm in the open air; and s, the eye of the ſpectator, viewing the clouds by their light coming into the priſm at the plane ſide FIGK, and reflected

(g) Vide Lect. Opt. Part II. Sect. v.

in

in it by its baſe HEIG, and thence going out through its plane ſide HEFK to the eye. And when the priſm and eye are conveniently placed, ſo that the angles of incidence and reflexion at the baſe may be about 40 degrees; the ſpectator will ſee a bow MN of a Blue colour, running from one end of the baſe to the other, with the concave ſide towards him; and the part of the baſe IMNG beyond this bow will be brighter than the other part, EMNH, on the other ſide of it. This Blue colour MN being made by nothing elſe than by Reflexion of a ſpecular ſuperficies, ſeems ſo odd a phænomenon, and ſo difficult to be explained by the vulgar hypotheſis of philoſophers, that I could not but think it deſerved to be taken notice of. Now for underſtanding the reaſon of it, ſuppoſe the plane ABC to cut the plane ſides and baſe of the priſm perpendicularly. From the eye to the line BC, wherein that plane cuts the baſe, draw the lines s*p* and s*t*, in the angles s*pc* 50 degr. $\frac{1}{9}$, and s*tc* 49 degr. $\frac{1}{28}$: and the point *p* will be the limit, beyond which none of the moſt refrangible rays can paſs through the baſe of the priſm, and be refracted, whoſe incidence is ſuch that they may be reflected to the eye; and the point *t* will be the like limit for the leaſt refrangible rays, that is, beyond which none of them can paſs through the baſe, whoſe incidence is ſuch that by reflexion they may come to the eye. And the point *r* taken in the middle way between *p* and *t*, will be the like limit for the meanly refrangible rays. And therefore all the leaſt refrangible rays which fall upon the baſe beyond *t*, that is, between *t* and B, and can come from thence to the eye, will be reflected thither: but on this ſide *t*, that is, between *t* and C, many of theſe rays will be tranſmitted through the baſe. And all the moſt refrangible rays which fall upon the baſe beyond *p*, that is, between *p* and B, and can by reflexion come from thence to the eye, will be reflected thither; but every where between *p* and C, many of theſe rays will get through the baſe and be refracted: and the ſame is to be underſtood of the meanly refrangible rays on either ſide of the point *r*. Whence it follows, that the baſe of the priſm muſt every where between *t* and B, by a total reflexion of all ſorts of rays to the eye, look white and bright. And every where between *p* and C, by reaſon of the tranſmiſ-

Colours explained.

ſion of many rays of every ſort, look more pale, obſcure and dark. But at *r*, and in other places between *p* and *t*, where all the more refrangible rays are reflected to the eye, and many of the leſs refrangible are tranſmitted, the exceſs of the moſt refrangible in the Reflected Light will tinge that light with their colour, which is Violet and Blue. And this happens by taking the line c*prt* any where between the ends of the priſm, HG and EI ([h]).

PROP. IX. PROB. IV.

By the diſcovered Properties of Light to explain the Colours of the Rain-bow.

The Rainbow. This bow never appears, but where it rains in the ſun-ſhine, and may be made artificially by ſpouting up water which may break aloft, and ſcatter into drops, and fall down like rain. For the ſun ſhining upon theſe drops certainly cauſes the bow to appear to a ſpectator ſtanding in a due poſition to the rain and ſun. And hence it is now agreed upon, that this bow is made by refraction of the ſun's light in drops of falling rain. This was underſtood by ſome of the ancients, and of late more fully diſcovered and explained by the famous *Antonius de Dominis* archbiſhop of *Spalato*, in his book *De Radiis Visûs & Lucis*, publiſhed by his friend *Bartolùs* at *Venice*, in the year 1611, and written above 20 years before. For he teaches there how the interior bow is made in round drops of rain by two refractions of the ſun's light, and one reflexion between them; and the exterior, by two refractions and two ſorts of reflexions between them in each drop of water; and proves his explications by Experiments made with a phial full of water, and with globes of glaſs filled with water, and placed in the ſun to make the colours of the two bows appear in them. The ſame explication *Des-Cartes* hath purſued in his Meteors, and mended that of the exterior bow. But whilſt

([h]) Vid. Lect. Opt. Part II. § 145—149.
([i]) Vid. Lect. Opt. Part I. Sect IV. Prop. xxxv. Cor. 1 & 2.
([k]) Lect. Opt. Part I. Sect. IV. Prop. xxxvi. Cor. 1 & 2.

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they underſtood not the true origin of colours, it is neceſſary to purſue it here a little farther. For underſtanding therefore how the bow is made, let a drop of rain, or any other ſpherical tranſparent body, be repreſented by the ſphere BNFG [in *fig.* 14.] deſcribed with the center C, and ſemi-diameter CN. And let AN be one of the ſun's rays, incident upon it at N, and thence refracted to F; where let it either go out of the ſphere by refraction towards V, or be reflected to G; and at G, let it either go out by refraction to R, or be reflected to H; and at H, let it go out by refraction towards S, cutting the incident ray in Y. Produce AN and RG till they meet in X; and upon AX and NF let fall the perpendiculars CD and CE; and produce CD, till it fall upon the circumference at L. Parallel to the incident ray, AN, draw the diameter BQ; and let the ſine of incidence out of air into water be to the ſine of refraction as I to R. Now if you ſuppoſe the point of incidence, N, to move from the point B continually, till it come to L, the arch QF will firſt increaſe and then decreaſe; and ſo will the angle AXR, which the rays AN and GR contain; and the arch QF, and angle AXR, will be biggeſt, when ND is to CN as $\sqrt{\text{II} - \text{RR}}$ to $\sqrt{3\text{RR}}$; in which caſe NE will be to ND as 2R to I (i). Alſo the angle AYS, which the rays AN and HS contain, will firſt decreaſe, and then increaſe; and grow leaſt, when ND is to CN as $\sqrt{\text{II}-\text{RR}}$ to $\sqrt{8\text{RR}}$; in which caſe NE will be to ND as 3R to I (k). And ſo the angle, which the next emergent ray (that is, the emergent ray after three reflexions) contains with the incident ray AN, will come to its limit, when ND is to CN as $\sqrt{\text{II}-\text{RR}}$ to $\sqrt{15\text{RR}}$; in which caſe NE will be to ND as 4R to I. And the angle, which the ray next after that enmergent, that is, the ray emergent after four reflexions, contains with the incident, will come to its limit, when ND is to CN as $\sqrt{\text{II}-\text{RR}}$ to $\sqrt{24\text{RR}}$; in which caſe NE will be to ND as 5R to I: and ſo on infinitely, the numbers 3, 8, 15, 24, &c. being gathered by continual addition of the terms of the Arithmetical Progreſſion 3, 5, 7, 9, &c. The truth of all this Mathematicians will eaſily examine (l).

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(l) It all follows very evidently from the demonſtrations our author hath given of the two Propoſitions referred to in Note i and k.

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Now it is to be obſerved, that as when the ſun comes to his tropicks, days increaſe and decreaſe but a very little for a great while together; ſo when by increaſing the diſtance CD, theſe angles come to their limits, they vary their quantity but very little for ſome time together; and therefore a far greater number of the rays, which fall upon all the points N in the quadrant BL, ſhall emerge in the limits of theſe angles, than in any other inclinations. And farther it is to be obſerved, that the rays, which differ in refrangibility, will have different limits of their angles of emergence; and, by conſequence, according to their different degrees of refrangibility, emerge moſt copiouſly in different angles, and being ſeparated from one another appear each in their proper colours. And what thoſe angles are, may be eaſily gathered from the foregoing Theorem by computation.

For in the leaſt refrangible rays the ſines I and R (as was found above) are 108 and 81, and thence by computation the greateſt angle AXR will be found 42 degrees and 2 minutes; and the leaſt angle AYS 50 degrees and 57 minutes. And in the moſt refrangible rays the ſines I and R are 109 and 81; and thence by computation the greateſt angle AXR will be found 40 degrees and 17 minutes, and the leaſt angle AYS 54 degrees and 7 minutes.

Suppoſe now that O [in *fig.* 15.] is the ſpectator's eye, and OP a line drawn parallel to the ſun's rays; and let POE, POF, POG, POH, be angles of 40 degr. 17 min. 42 degr. 2 min. 50 degr. 57 min. and 54 degr. 7 min. reſpectively; and theſe angles turned about their common ſide OP, ſhall with their other ſides, OE, OF, OG, OH, deſcribe the verges of two rain-bows AFBE and CHDG. For if E, F, G, H be drops, placed any where in the conical ſuperficies deſcribed by OE, OF, OG, OH, and be illuminated by the ſun's rays SE, SF, SG, SH; the angle SEO being equal to the angle POE or 40 deg. 17 min. ſhall be the greateſt angle, in which the moſt refrangible rays can, after one reflexion, be refracted to the eye; and therefore all the drops in the line OE ſhall ſend the moſt refrangible rays moſt copiouſly to the eye, and thereby ſtrike the ſenſes with the deepeſt Violet colour in that region. And in like manner the angle SFO being equal to the angle POF, or 42 degr. 2 min. ſhall be the greateſt, in which the leaſt refrangible rays, after

after one reflexion, can emerge out of the drops; and therefore those rays ſhall come moſt copiouſly to the eye from the drops in the line OF, and ſtrike the ſenſes with the deepeſt Red colour in that region. And by the ſame argument, the rays, which have intermediate degrees of refrangibility, ſhall come moſt copiouſly from drops between E and F, and ſtrike the ſenſes with the intermediate colours, in the order which their degrees of refrangibity require; that is in the progreſs from E to F, or from the inſide of the bow to the outſide, in this order; Violet, Indigo, Blue, Green, Yellow, Orange, Red. But the Violet, by the mixture of the White light of the clouds, will appear faint, and incline to Purple. The RAINBOW.

Again, the angle SGO being equal to the angle POG, or 50 gr. 51 min. ſhall be the leaſt angle, in which the leaſt refrangible rays can, after two reflexions, emerge out of the drops; and therefore the leaſt refrangible rays ſhall come moſt copiouſly to the eye from the drops in the line OG, and ſtrike the ſenſe with the deepeſt Red in that region. And the angle SHO being equal to the angle POH, or 54 gr. 7 min. ſhall be the leaſt angle, in which the moſt refrangible rays, after two reflexions, can emerge out of the drops; and therefore thoſe rays ſhall come moſt copiouſly to the eye from the drops in the line OH, and ſtrike the ſenſes with the deeped Violet in that region. And by the ſame argument, the drops in the regions between G and H ſhall ſtrike the ſenſe with the intermediate colours, in the order which their degrees of refrangibility require; that is, in the progreſs from G to H, or from the inſide of the bow to the outſide, in this order; Red, Orange, Yellow, Green, Blue, Indigo, Violet. And ſince theſe four lines OE, OF, OG, OH, may be ſituated any where in the abovementioned Conical Superficies; what is ſaid of the drops and colours in theſe lines is to be underſtood of the drops and colours every where in thoſe ſuperficies.

Thus ſhall there be made two bows of colours, an interior and ſtronger, by one reflexion in the drops, and an exterior and fainter by two; for the light becomes fainter by every reflexion. And their colours ſhall lie in a contrary order to one another, the Red of both bows bordering upon the ſpace, GF, which is between the

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the bows. The breadth of the interior bow, EOF, measured croſs the colours, ſhall be 1 deg. 45 min. and the breadth of the exterior, GOH, ſhall be 3 deg. 10 min. and the diſtance between them GOF ſhall be 8 gr. 55 min. the greateſt ſemi-diameter of the innermoſt, that is, the angle POF being 42 gr. 2 min. and the leaſt ſemi-diameter of the outermoſt POG, being 50 gr. 57 min. Theſe are the meaſures of the bows, as they would be, were the ſun but a point; for by the breadth of his body the breadth of the bows will be increaſed and their diſtance decreaſed by half a degree, and ſo the breadth of the interior Iris will be 2 deg. 15 min. that of the exterior 3 deg. 40 min. their diſtance 8 deg. 25 min. the greateſt ſemi-diameter of the interior bow 42 deg. 17 min. and the leaſt of the exterior 50 deg. 42 min. And ſuch are the dimenſions of the bows in the heavens found to be very nearly, when their colours appear ſtrong and perfect. For once, by ſuch means as I then had, I meaſured the greateſt ſemi-diameter of the interior Iris about 42 degrees; and the breadth of the Red, Yellow and Green in that Iris, 63 or 64 minutes; beſides the outmoſt faint Red obſcured by the brightneſs of the clouds, for which we may allow 3 or 4 minutes more. The breadth of the Blue was about 40 minutes more beſides the Violet, which was ſo much obſcured by the brightneſs of the clouds, that I could not meaſure its breadth. But ſuppoſing the breadth of the Blue and Violet together to equal that of the Red, Yellow and Green together, the whole breadth of this iris will be about $2\frac{1}{4}$ degrees, as above. The leaſt diſtance between this iris and the exterior iris was about 8 degrees and 30 minutes. The exterior iris was broader than the interior, but ſo faint, eſpecially on the Blue ſide, that I could not meaſure its breadth diſtinctly. At another time when both bows appeared more diſtinct, I meaſured the breadth of the interior iris 2 gr. 10, and the breadth of the Red, Yellow and Green in the exterior iris, was to the breadth of the ſame colours in the interior as 3 to 2.

This explication of the rain-bow is yet farther confirmed by the known experiment, made by *Antonius de Dominis* and *Des-Cartes*, of hanging up any where in the ſun-ſhine a glaſs globe filled with water; and viewing it in ſuch a poſture, that the rays,

which come from the globe to the eye, may contain with the ſun's rays an angle of either 42 or 50 degrees. For if the angle be about 42 or 43 degrees, the ſpectator, ſuppoſe at O, ſhall ſee a full Red colour in that ſide of the globe oppoſed to the ſun, as it is repreſented at F; and if that angle become leſs, ſuppoſe by depreſſing the globe to E, there will appear other colours, Yellow, Green and Blue ſucceſſively in the ſame ſide of the globe. But if the angle be made about 50 degrees, ſuppoſe by lifting up the globe to G, there will appear a Red colour in that ſide of the globe towards the ſun; and if the angle be made greater, ſuppoſe by lifting up the globe to H, the Red will turn ſucceſſively to the other colours, Yellow, Green and Blue. The ſame thing I have tried by letting a globe reſt, and raiſing or depreſſing the eye, or otherwiſe moving it to make the angle of a juſt magnitude. The Rainbow.

I have heard it repreſented, that if the light of a candle be refracted by a priſm to the eye; when the Blue colour falls upon the eye, the ſpectator ſhall ſee Red in the priſm, and when the Red falls upon the eye he ſhall ſee Blue: and if this were certain, the colours of the globe and rain-bow ought to appear in a contrary order to what we find. But the colours of the candle being very faint, the miſtake ſeems to ariſe from the difficulty of diſcerning what colours fall on the eye. For, on the contrary, I have ſometimes had occaſion to obſerve in the ſun's light, refracted by a priſm, that the ſpectator always ſees that colour in the priſm, which falls upon his eye. And the ſame I have found true alſo in candle-light. For when the priſm is moved ſlowly from the line which is drawn directly from the candle to the eye, the Red appears firſt in the priſm, and then the Blue; and therefore each of them is ſeen when it falls upon the eye. For the Red paſſes over the eye firſt, and then the Blue.

The light which comes through drops of rain by two refractions without any reflexion, ought to appear ſtrongeſt at the diſtance of about 26 degrees from the ſun, and to decay gradually both ways, as the diſtance from him increaſes and decreaſes. And the ſame is to be underſtood of light tranſmitted through ſpherical hail-ſtones. And if the hail be a little flatted, as it often Halos and Parhelia.

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Halos and Parhelia.

is, the light tranſmitted may grow ſo ſtrong at a little leſs diſtance than that of 26 degrees, as to form a halo about the ſun or moon; which halo, as often as the hail-ſtones are duly figured, may be coloured. And then it muſt be Red within by the leaſt refrangible rays, and Blue without by the moſt refrangible ones; eſpecially if the hail-ſtones have opake globules of ſnow in their center to intercept the light within the halo (as *Hugenius* has obſerved) and make the inſide thereof more diſtinctly defined than it would otherwiſe be. For ſuch hail-ſtones, though ſpherical, by terminating the light by the ſnow, may make a halo red within and colourleſs without, and darker in the red than without, as halos uſe to be. For of thoſe rays which paſs cloſe by the ſnow, the rubriform will be leaſt refracted, and ſo come to the eye in the directeſt lines.

The light which paſſes through a drop of rain after two refractions, and three or more reflexions, is ſcarce ſtrong enough to cauſe a ſenſible bow; but in thoſe cylinders of ice by which *Hugenius* explains the *Parhelia*, it may perhaps be ſenſible.

PROP. X. PROB. V.

By the diſcovered properties of Light to explain the Permanent Colours of Natural Bodies.

Origin of the permanent Colours

Theſe colours ariſe from hence; that ſome natural bodies reflect ſome ſorts of rays, others other ſorts more copiouſly than the reſt. Minium reflects the leaſt refrangible or Red-making rays more copiouſly, and thence appears Red. Violets reflect the moſt refrangible, moſt copiouſly, and thence have their colour, and ſo of other bodies. Every body reflects the rays of its own colour more copiouſly than the reſt, and from their exceſs and predominance in the Reflected Light has its colour.

Exper. 17. For if in the Homogeneal Lights, obtained by the ſolution of the Problem propoſed in the fourth Propoſition of the firſt Part of this Book, you place bodies of ſeveral colours; you will find, as I have done, that every body looks moſt ſplendid and luminous in the light of its own colour. Cinnaber in the Homogeneal Red light is moſt reſplendent, in the Green light it is manifeſtly

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leſs reſplendent, and in the Blue light ſtill leſs. Indigo in the Violet-blue light is moſt reſplendent, and its ſplendor is gradually diminiſhed as it is removed thence by degrees through the Green and Yellow light to the Red. By a leek the Green light, and next that the Blue and Yellow which compound Green, are more ſtrongly reflected than the other colours, Red and Violet, and ſo of the reſt. But to make theſe experiments the more manifeſt, ſuch bodies ought to be choſen as have the fulleſt and moſt vivid colours, and two of thoſe bodies are to be compared together. Thus, for inſtance, if Cinnaber and Ultra-marine blue, or ſome other full blue, be held together in the Red homogeneal light, they will both appear Red; but the Cinnaber will appear of a ſtrongly luminous and reſplendent Red, and the Ultra-marine blue of a faint obſcure and dark Red; and if they be held together in the Blue homogeneal light they will both appear Blue, but the Ultra-marine will appear of a ſtrongly luminous and reſplendent Blue, and the Cinnaber of a faint and dark Blue. Which puts it out of diſpute, that the Cinnaber reflects the Red light more copiouſly than the Ultra-marine doth, and the Ultra-marine reflects the Blue much more copiouſly than the Cinnaber doth. The ſame experiment may be tried ſucceſsfully with Red Lead and Indigo, or with any other two coloured bodies, if due allowance be made for the different ſtrength and weakneſs of their colour and light. of Natural Bodies.

And as the reaſon of the colours of Natural Bodies is evident by theſe experiments, ſo it is farther confirmed and put paſt diſpute by the two firſt Experiments of the firſt Part; whereby it was proved in ſuch bodies, that the reflecting lights, which differ in colours, do differ alſo in degrees of refrangibility. For thence it is certain, that ſome bodies reflect the more refrangible, others the leſs refrangible rays more copiouſly.

And that this is not only a true reaſon of theſe colours, but even the only reaſon, may appear farther from this conſideration, that the colour of Homogeneal Light cannot be changed by the reflexion of Natural Bodies.

For if bodies by reflexion cannot in the leaſt change the colour of any one ſort of rays, they cannot appear coloured by

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any other means, than by reflecting thoſe which either are of their own colour, or which by mixture muſt produce it.

But in trying experiments of this kind, care muſt be had that the light be ſufficiently Homogeneal. For if bodies be illuminated by the ordinary Priſmatick Colours, they will appear neither of their own day-light colours, nor of the colour of the light caſt on them; but of ſome middle colour between both, as I have found by experience. Thus red lead, for inſtance, illuminated with the ordinary Priſmatick Green, will not appear either Red or Green, but Orange or Yellow, or between Yellow and Green, accordingly as the Green light, by which it is illuminated, is more or leſs compounded. For becauſe red lead appears Red, when illuminated with White light, wherein all ſorts of rays are equally mixed, and in the Green light all ſorts of rays are not equally mixed; the exceſs of the Yellow-making, Green-making and Blue-making rays in the incident Green light, will cauſe thoſe rays to abound ſo much in the Reflected light, as to draw the colour from Red towards their colour. And becauſe the red lead reflects the Red-making rays moſt copiouſly in proportion to their number, and next after them the Orange-making and Yellow-making rays; theſe rays in the Reflected light will be more, in proportion to the light, than they were in the incident Green light, and thereby will draw the Reflected light from Green towards their colours. And therefore the Red lead will appear neither Red nor Green, but of a colour between both.

In tranſparently coloured liquors it is obſervable, that their colour uſes to vary with their thickneſs. Thus, for inſtance, a Red liquor in a Conical glaſs held between the light and the eye, looks of a pale and dilute Yellow at the bottom, where it is thin; and a little higher, where it is thicker, grows Orange; and where it is ſtill thicker, becomes Red; and where it is thickeſt, the Red is deepeſt and darkeſt ([m]). For it is to be conceived that ſuch a liquor ſtops the Indigo-making and Violet-making rays moſt eaſily, the Blue-making rays more difficultly, the Green-making rays ſtill more difficultly, and the Red-making moſt difficultly: and that if the thickneſs of the liquor be only ſo much

([m]) Vide Lect. Opt. Part II. § 97.

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as ſuffices to ſtop a competent number of the Violet-making and Indigo-making rays, without diminiſhing much the number of the reſt, the reſt muſt (by Prop. VI. Part I.) compound a pale Yellow. But if the liquor be ſo much thicker, as to ſtop alſo a great number of the Blue-making rays, and ſome of the Green-making, the reſt muſt compound an Orange; and where it is ſo thick, as to ſtop alſo a great number of the Green-making, and a conſiderable number of the Yellow-making, the reſt muſt begin to compound a Red; and this Red muſt grow deeper and darker as the Yellow-making and Orange-making rays are more and more ſtopped by increaſing the thickneſs of the liquor, ſo that few rays beſides the Red-making can get through. of Natural Bodies.

Of this kind is an experiment lately related to me by Mr. *Halley*; who, in diving deep into the ſea in a diving-veſſel, found in a clear ſun-ſhine day, that when he was ſunk many fathoms deep into the water, the upper part of his hand, on which the ſun ſhone directly through the water, and through a ſmall glaſs-window in the veſſel, appeared of a Red colour like that of a damaſk roſe; and the water below, and the upper part of his hand, illuminated by light reflected from the water below, look Green. For thence it may be gathered, that the ſea-water reflects back the Violet and Blue making rays moſt eaſily, and lets the Red-making rays paſs moſt freely and copiouſly to great depths. For thereby the ſun's direct light at all great depths, by reaſon of the predominating Red-making rays, muſt appear Red; and the greater the depth is, the fuller and intenſer muſt that Red be. And at ſuch depths as the Violet-making rays ſcarce penetrate unto, the Blue-making, Green-making and Yellow-making rays, being reflected from below more copiouſly than the Red-making ones, muſt compound a Green.

Now if there be two liquors of full colours, ſuppoſe a Red and a Blue, and both of them ſo thick as ſuffices to make their colours ſufficiently full; though either liquor be ſufficiently tranſparent apart, yet will you not be able to ſee through both together. For if only the Red-making rays paſs through one liquor, and only the Blue-making through the other, no rays can paſs through both. This Mr. *Hook* tried caſually with glaſs wedges

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filled with Red and Blue liquors, and was ſurprized at the unexpected event; the reaſon of it being then unknown: which makes me truſt the more to his Experiment, though I have not tried it myſelf. But he that would repeat it, muſt take care the liquors be of very good and full colours.

Now whilſt bodies become coloured by reflecting or tranſmitting this or that ſort of rays more copiouſly than the reſt; it is to be conceived that they ſtop and ſtifle in themſelves the rays, which they do not reflect or tranſmit. For if gold be foliated, and held between your eye and the light, the light looks of a greyiſh Blue; and therefore maſſy gold lets into its body the Blue-making rays, to be reflected to and fro within it, till they be ſtopped and ſtifled; whilſt it reflects the Yellow-making outwards, and thereby looks Yellow. And much after the ſame manner that leaf-gold is Yellow by reflected, and Blue by tranſmitted light, and maſſy gold is Yellow in all poſitions of the eye; there are ſome liquors, as the tincture of *Lignum Nepriticum*, and ſome ſorts of glaſs, which tranſmit one ſort of light moſt copiouſly, and reflect another ſort; and thereby look of ſeveral colours, according to the poſition of the eye to the light. But if theſe liquors or glaſſes were ſo thick and maſſy, that no light could get through them; I queſtion not but they would, like all other opake bodies, appear of one and the ſame colour in all poſitions of the eye, though this I cannot yet affirm by experience. For all coloured bodies, ſo far as my obſervation reaches, may be ſeen through, if made ſufficiently thin; and therefore are in ſome meaſure Tranſparent, and differ only in degrees of tranſparency from tinged tranſparent liquors; theſe liquors, as well as thoſe bodies, by a ſufficient thickneſs becoming Opake. A tranſparent body which looks of any colour by tranſmitted light, may alſo look of the ſame colour by reflected light; the light of that colour being reflected by the farther ſurface of the body, or by the air beyond it. And then the reflected colour will be diminiſhed, and perhaps ceaſe, by making the body very thick, and pitching it on the backſide to diminiſh the reflexion of its farther ſurface, ſo that the light reflected from the tinging particles may predominate. In ſuch caſes, the colour of the reflected light

light will be apt to vary from that of the light tranſmitted. But whence it is that tinged bodies and liquors reflect ſome ſort of rays, and intromit or tranſmit other ſorts, ſhall be ſaid in the next Book. In this Propoſition I content myſelf to have put it paſt diſpute, that bodies have ſuch properties, and thence appear coloured. of Natural Bodies.

PROP. XI. PROB. VI.

By mixing coloured lights to compound a beam of light, of the ſame colour and nature with a beam of the ſun's direct light, and therein to experience the truth of the foregoing Propoſitions.

Let ABC*abc* [in *fig.* 16.] repreſent a priſm by which the ſun's light, let into a dark chamber through the hole F, may be refracted towards the lens MN, and paint upon it at *p*, *q*, *r*, *s* and *t*, the uſual colours, Violet, Blue, Green, Yellow and Red; and let the diverging rays, by the refraction of this lens, converge again towards x, and there, by the mixture of all thoſe their colours, compound a White, according to what was ſhewn above. Then let another priſm DEG*deg*, parallel to the former, be placed at x, to refract that White light upwards towards Y. Let the Refracting Angles of the priſms, and their diſtances from the lens be equal; ſo that the rays which converged from the lens towards x, and, without refraction, would there have croſſed and diverged again; may, by the refraction of the ſecond priſm, be reduced into parallelism, and diverge no more. For then thoſe rays will recompoſe a beam of White light, XY. If the Refracting Angle of either priſm be the bigger, that priſm muſt be ſo much the nearer to the lens. You will know when the priſms and the lens are well ſet together, by obſerving if the beam of light XY, which comes out of the ſecond priſm, be perfectly White to the very edges of the light, and at all diſtances from the priſm continue perfectly and totally White like a beam of the ſun's light. For till this happens, the poſition of the priſms and lens to one another muſt be corrected; and then if by the help of a long beam of wood, as is repreſented in the figure, or by a tube, or ſome other ſuch inſtrument made for that purpoſe, they be made faſt To recompoſe the Solar Light.

To recompoſe

faſt in that ſituation, you may try all the ſame experiments in this compounded beam of light XY, which have been made in the ſun's direct light. For this compounded beam of light has the ſame appearance, and is endowed with all the ſame properties with a direct beam of the ſun's light, ſo far as my obſervation reaches. And in trying experiments in this beam, you may, by ſtopping any of the colours *p*, *q*, *r*, *s* and *t* at the lens, ſee how the colours produced in the experiments are no other than thoſe which the rays had at the lens, before they entered the compoſition of this beam: and by conſequence, that they ariſe not from any new modifications of the light by refractions and reflexions, but from the various ſeparations and mixtures of the rays originally endowed with their colour-making qualities.

So, for inſtance, having with a lens $4\frac{1}{4}$ inches broad, and two priſms on either hand $6\frac{1}{4}$ feet diſtant from the lens, made ſuch a beam of compounded light: to examine the reaſon of the colours made by priſms, I refracted this compounded beam of light XY with another priſm HIK*bk*, and thereby caſt the uſual Priſmatick Colours, PQRST, upon the paper, LV, placed behind. And then by ſtopping any of the colours *p*, *q*, *r*, *s*, *t* at the lens, I found that the ſame colour would vaniſh at the paper. So if the Purple *p* was ſtopped at the lens, the Purple P upon the paper would vaniſh, and the reſt of the colours would remain unaltered; unleſs perhaps the Blue, ſo far as ſome Purple, latent in it at the lens, might be ſeparated from it by the following refractions. And ſo by intercepting the Green upon the lens, the Green R upon the paper would vaniſh, and ſo of the reſt; which plainly ſhews, that as the White beam of light, XY, was compounded of ſeveral lights variouſly coloured at the lens, ſo the colours, which afterwards emerge out of it by new refractions, are no other than thoſe of which its Whiteneſs was compounded. The refraction of the priſm HIK*kb* generates the colours PQRST upon the paper, not by changing the colorific qualities of the rays, but by ſeparating the rays, which had the very ſame colorific qualities before they entered the compoſition of the refracted beam of White light XY. For otherwiſe the rays which were of one colour at the lens might be of another upon the paper, contrary to what we find.

So

Fig. 1.

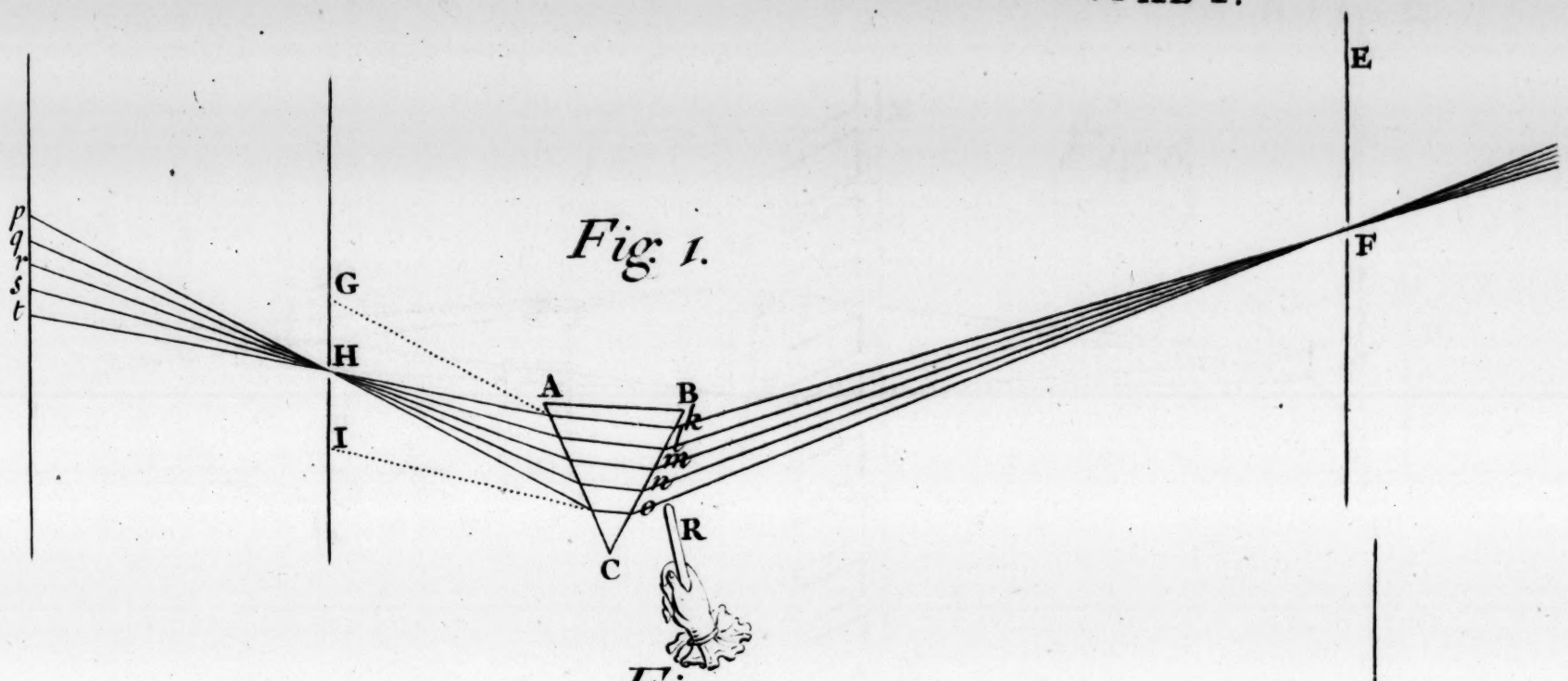


Fig. 2.

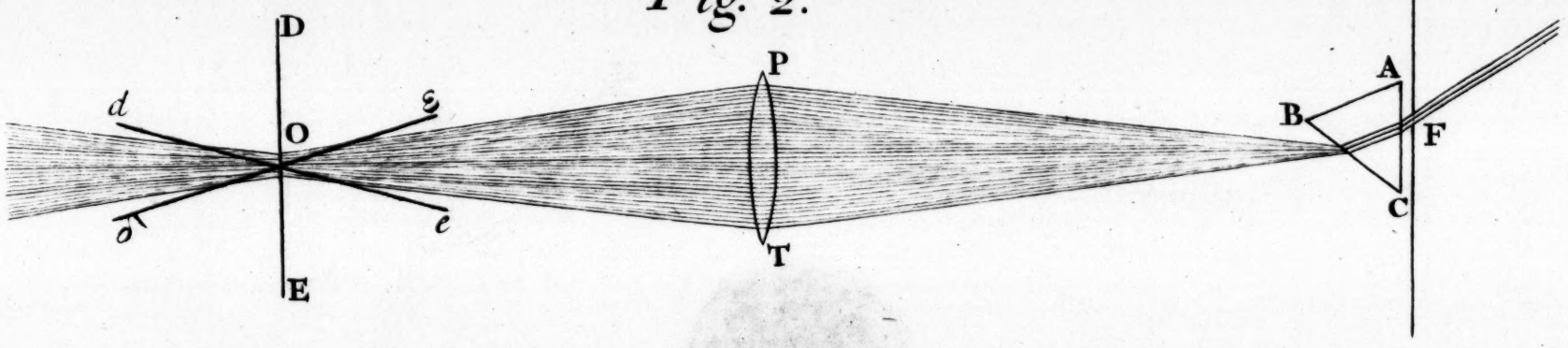
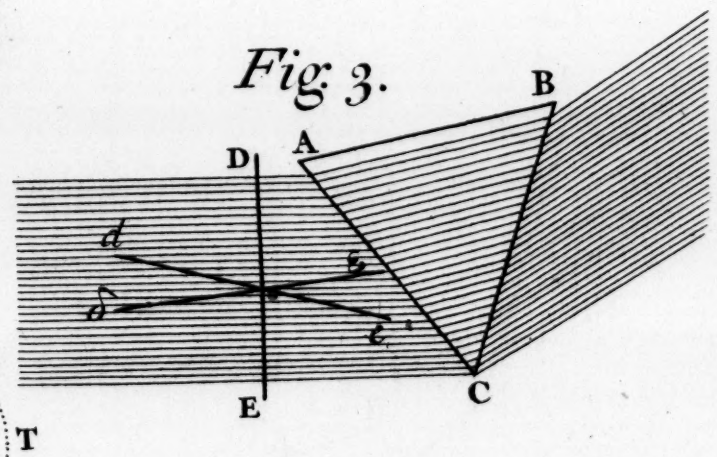


Fig. 3.



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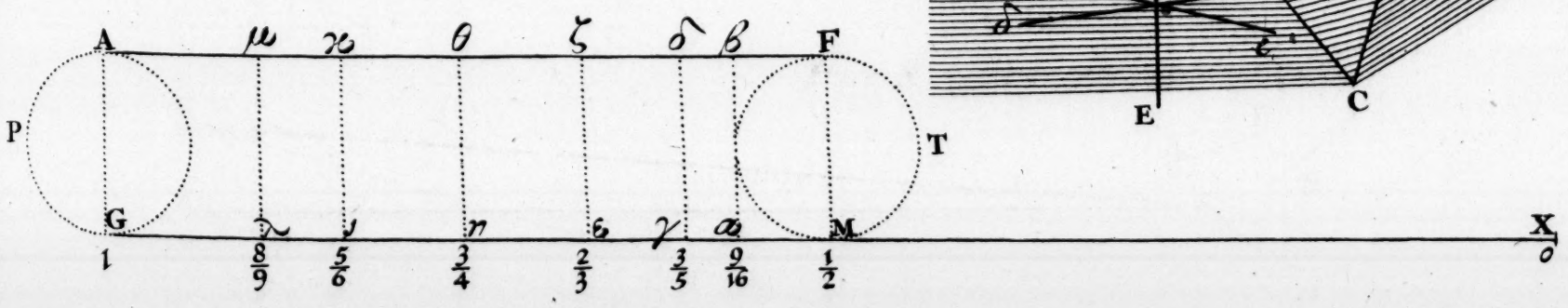

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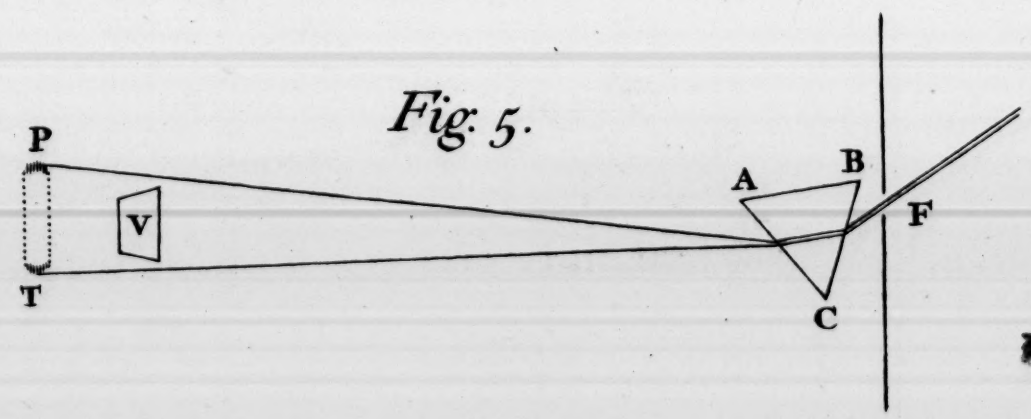

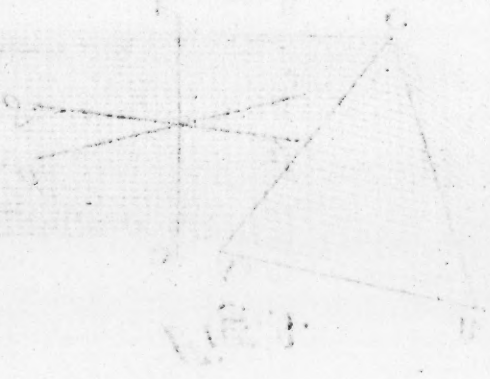

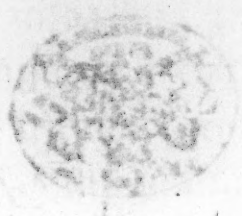

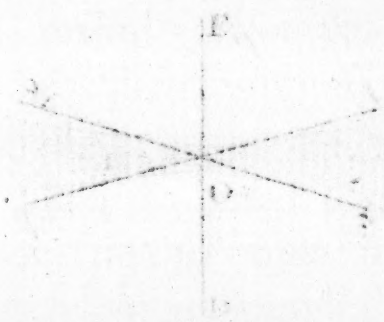

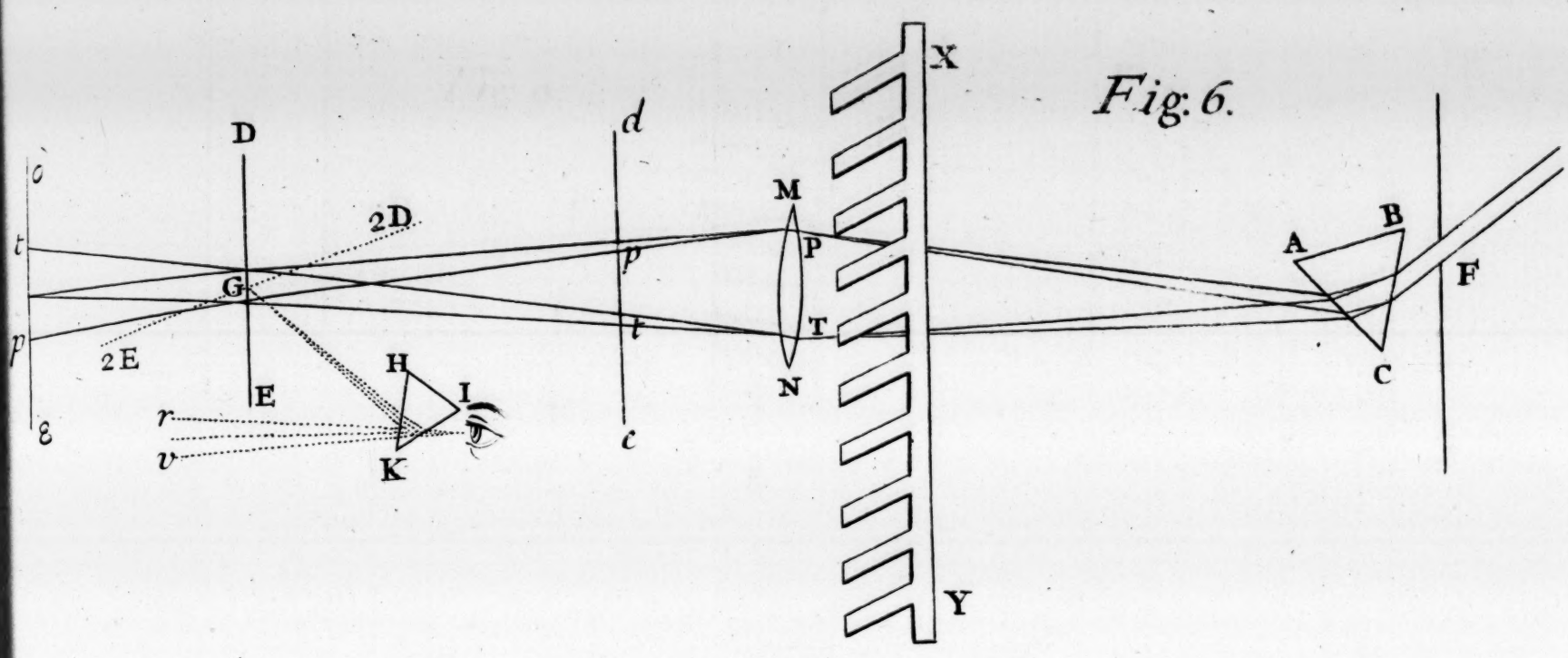

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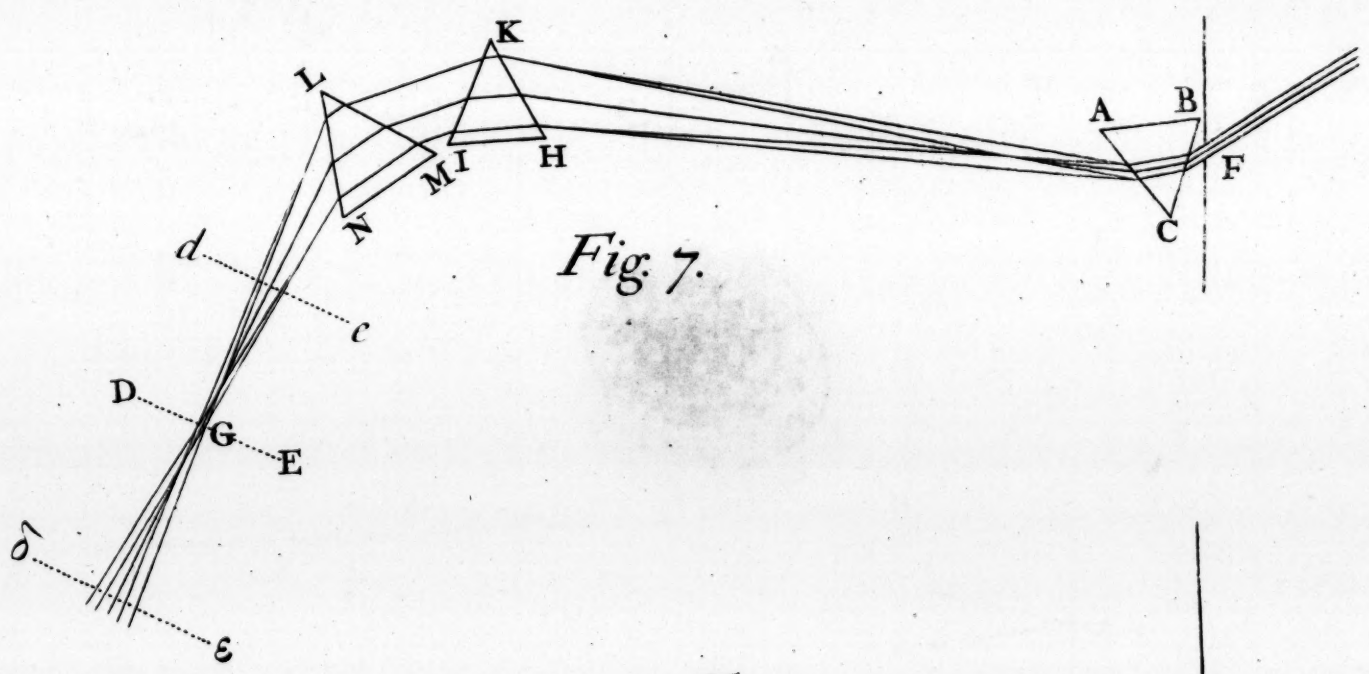

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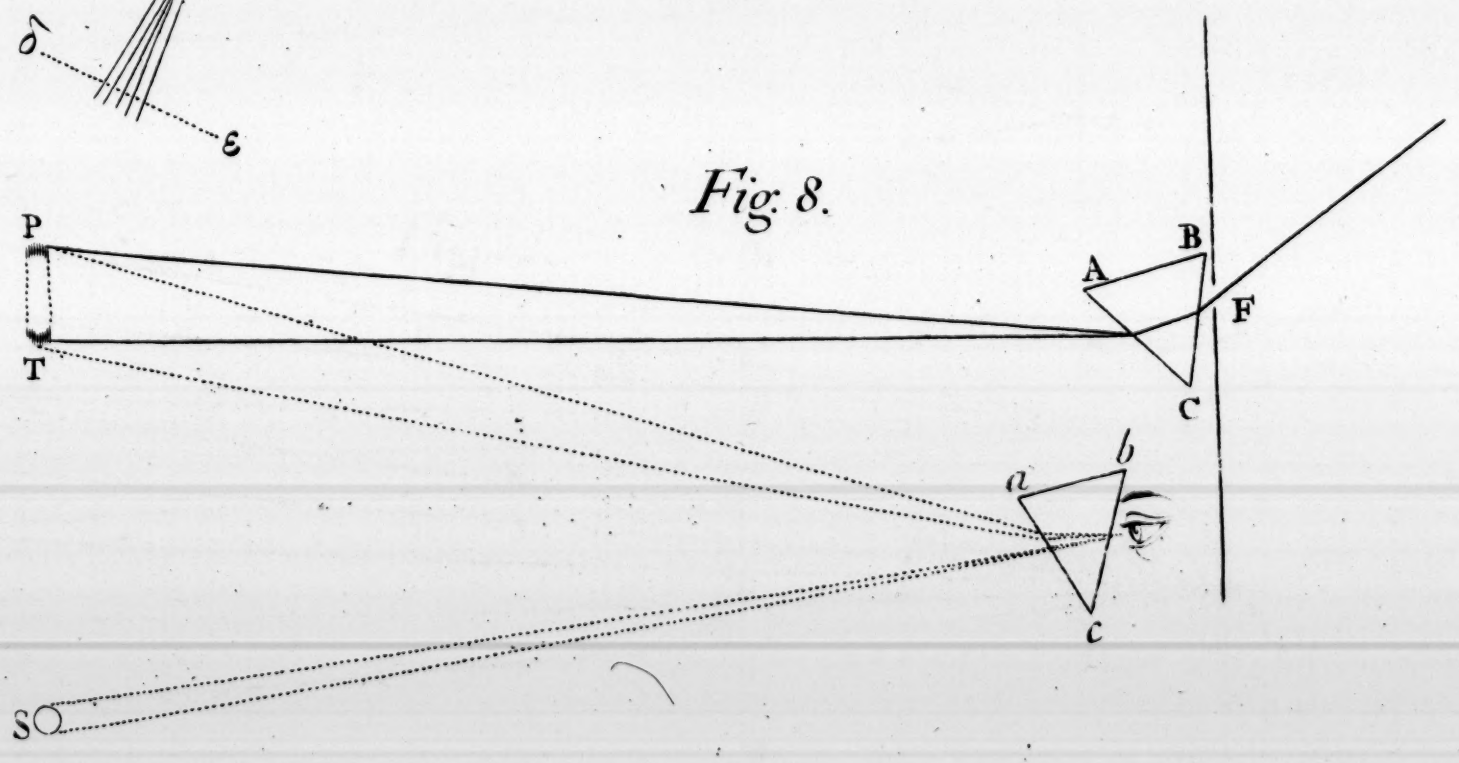

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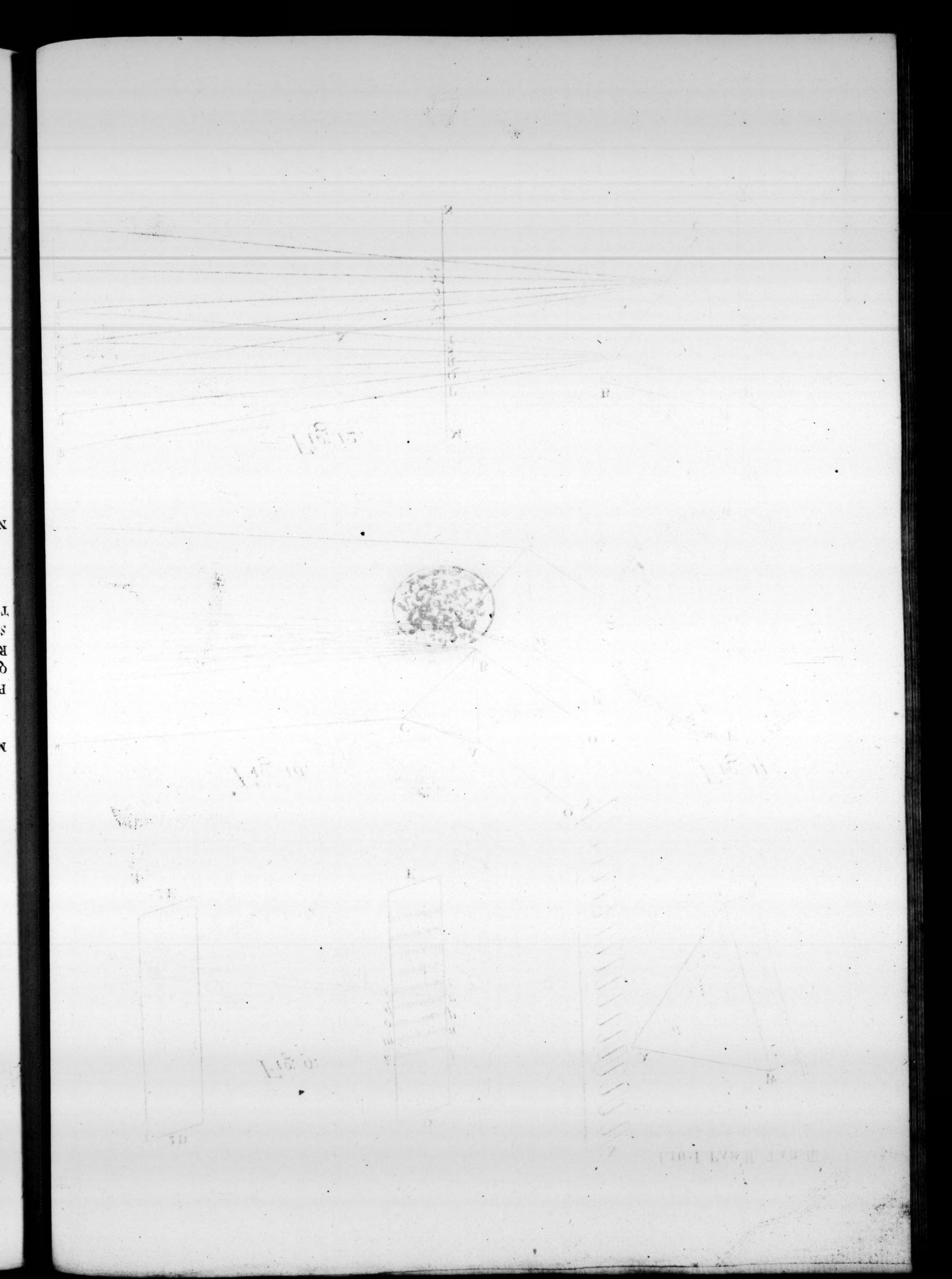


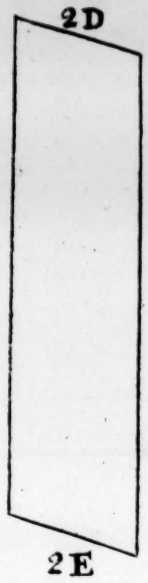


Fig. 9.

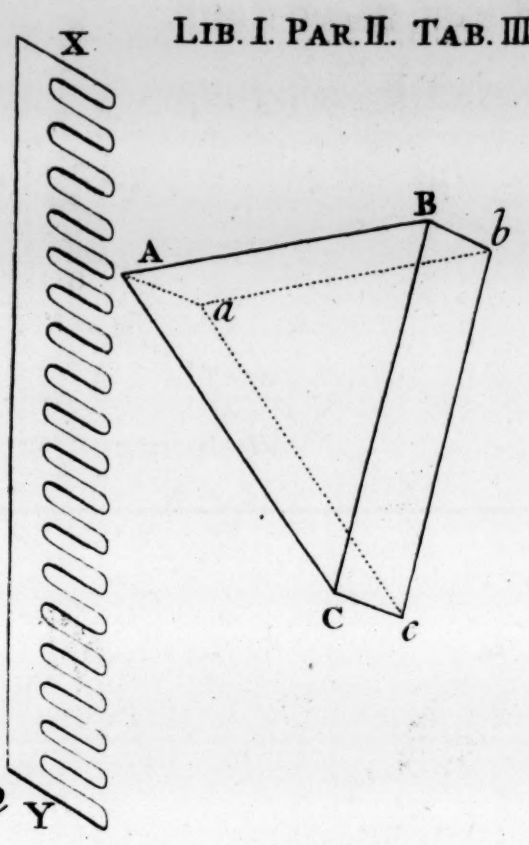
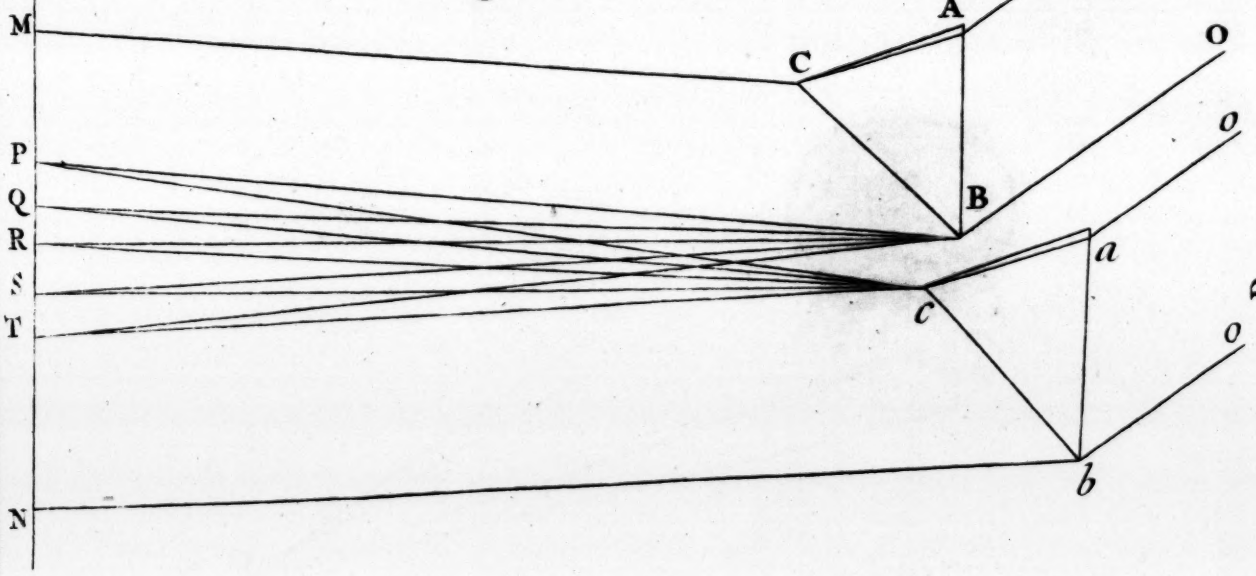


Fig. 10.



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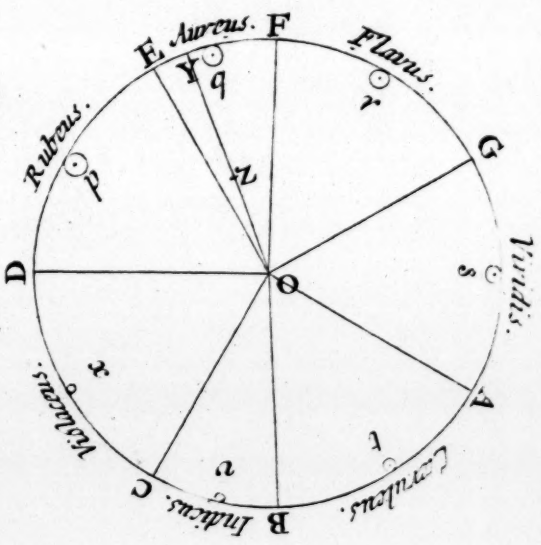

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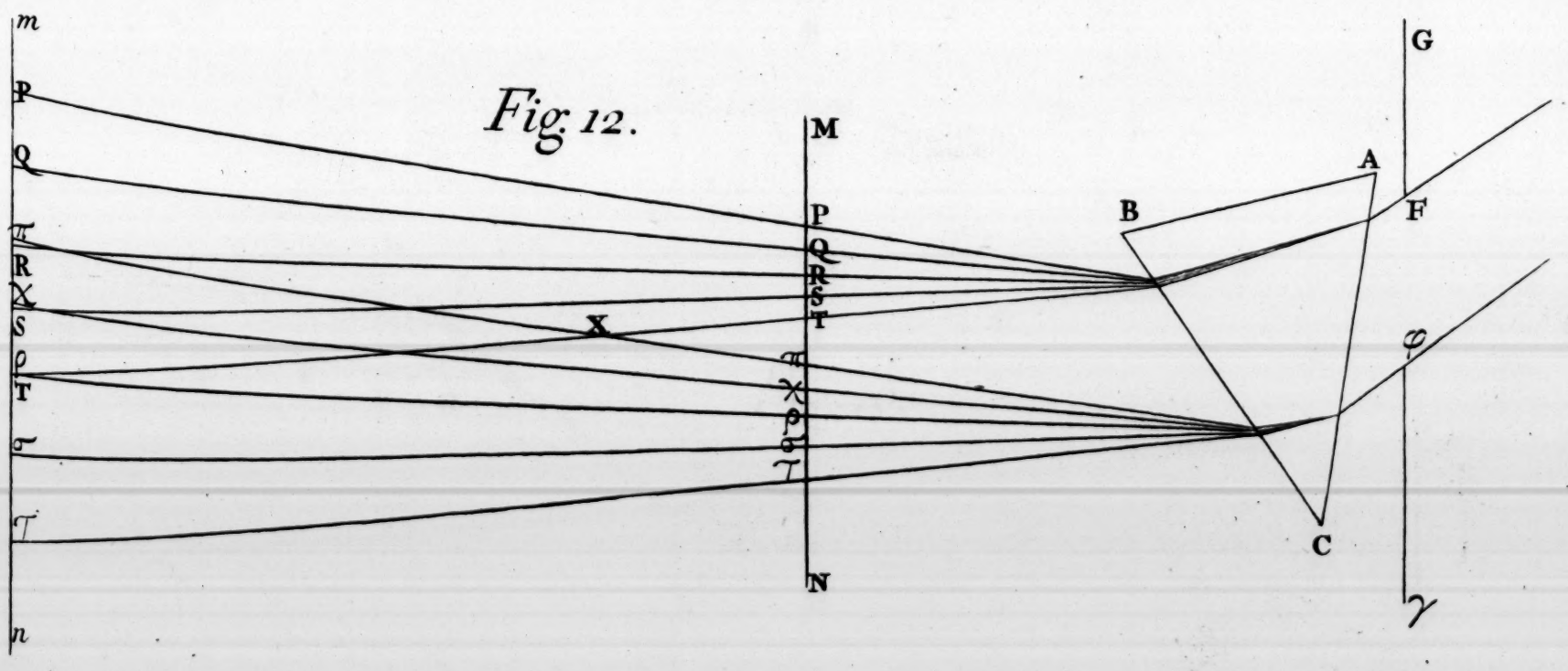

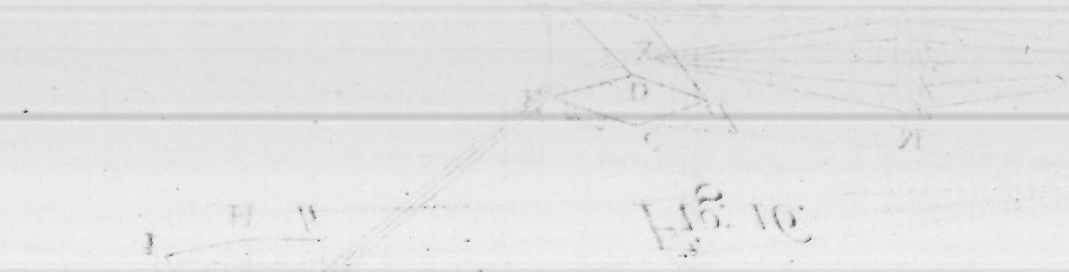

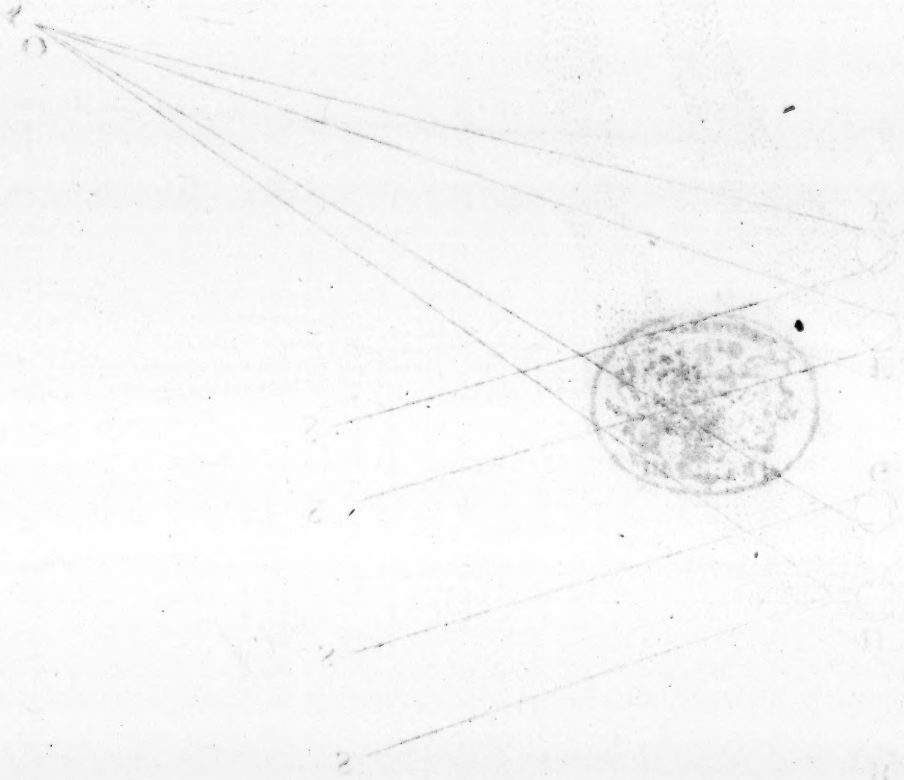

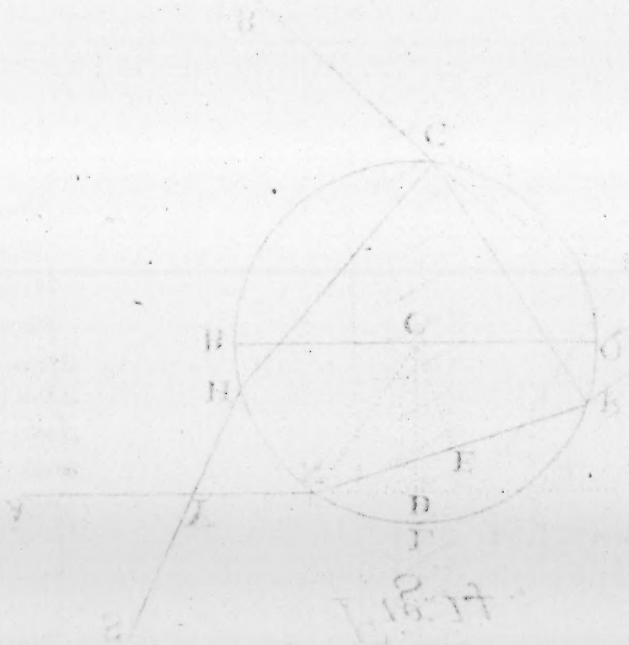

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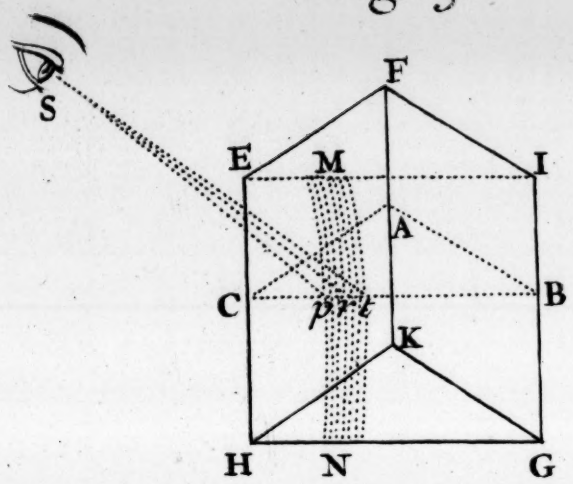
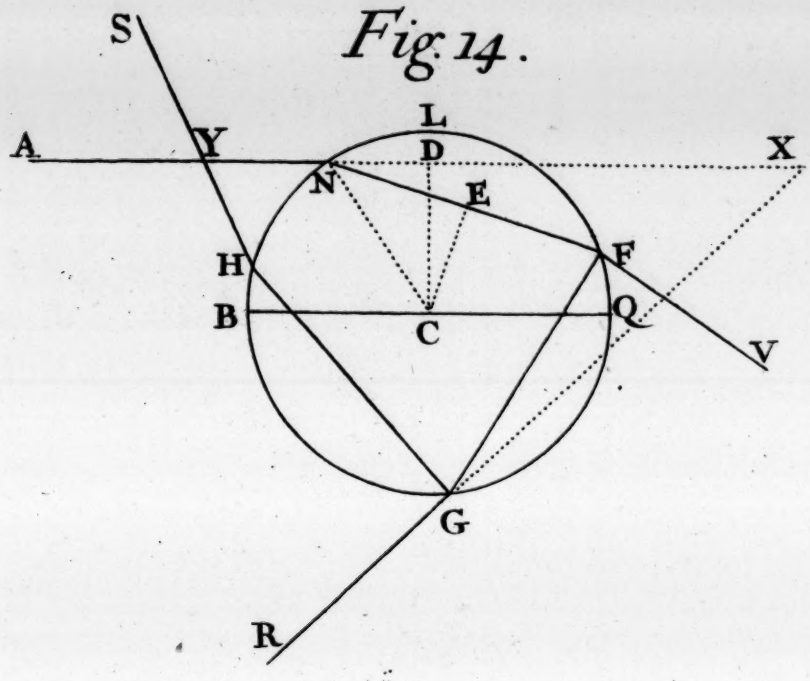


Fig. 14.



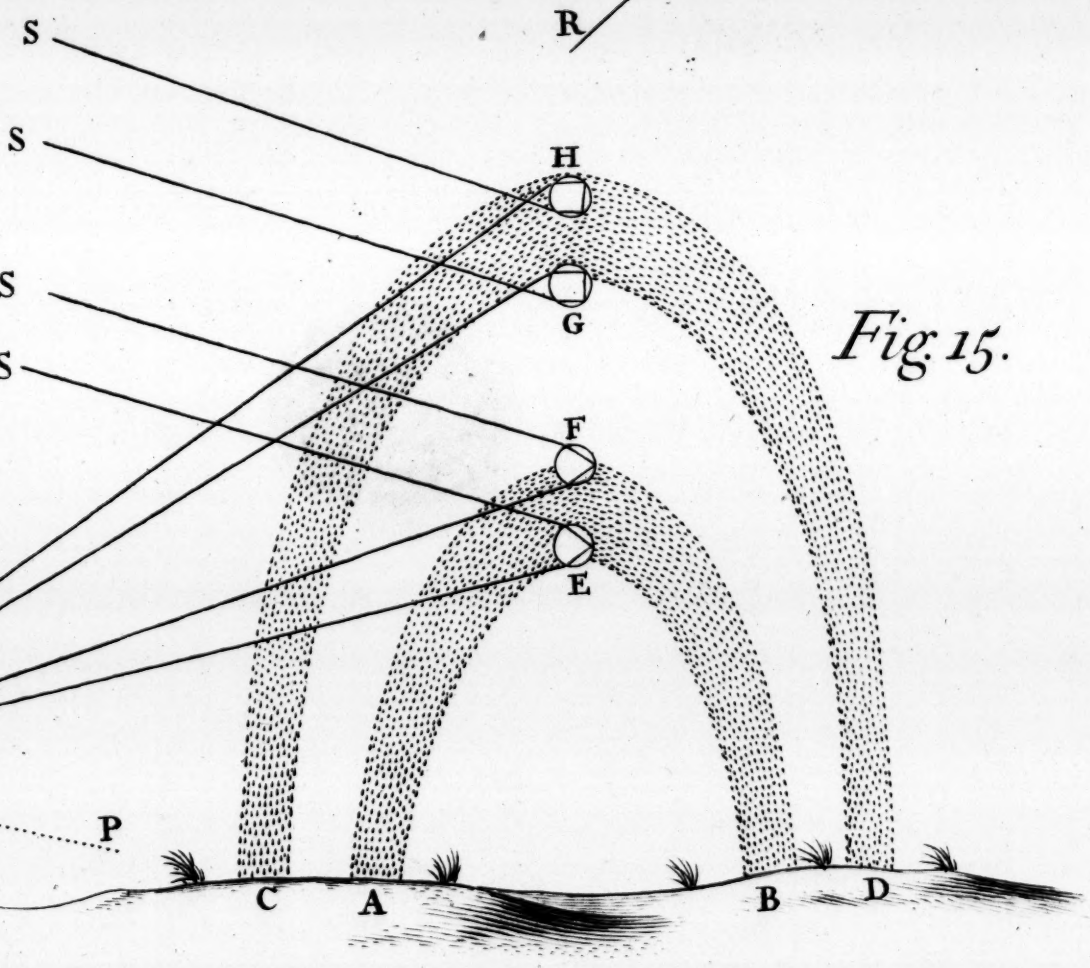


Fig. 15.

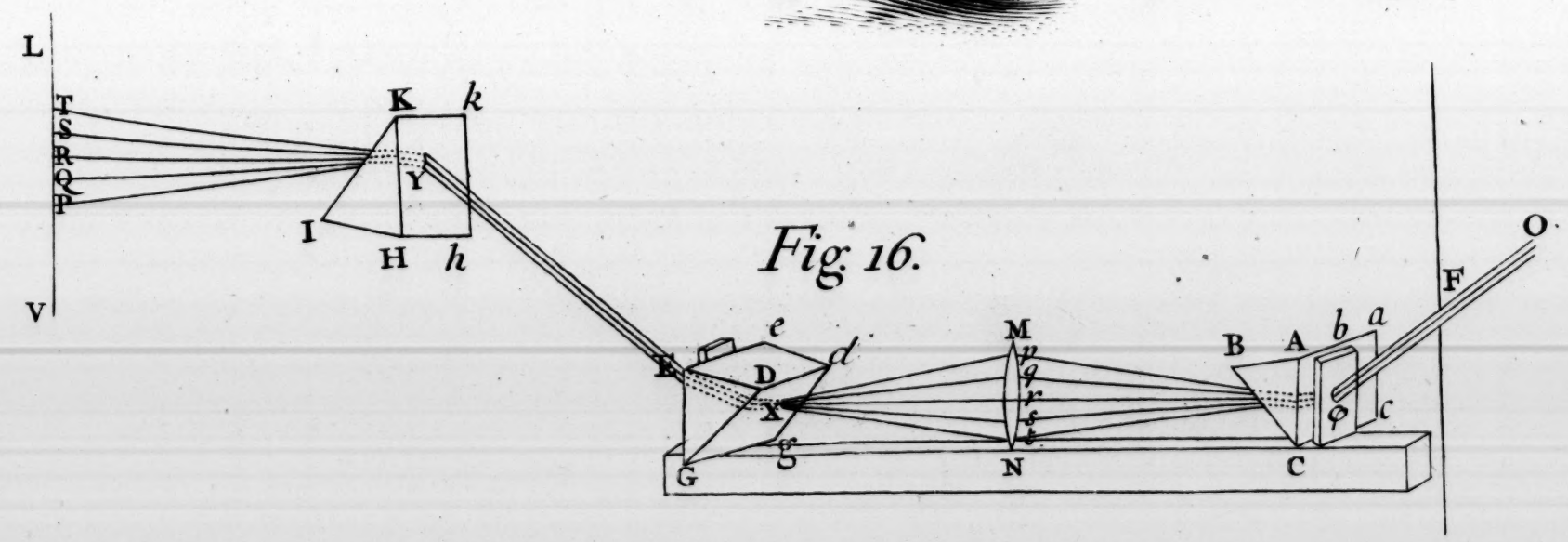


Fig. 16.

So again, to examine the reaſon of the colours of Natural Bodies, I placed ſuch bodies in the beam of light XY, and found that they all appeared there of thoſe their own colours which they have in day-light; and that thoſe colours depend upon the rays, which had the ſame colours at the lens, before they entered the compoſition of that beam. Thus, for inſtance, Cinnaber illuminated by this beam appears of the ſame Red colour as in day-light; and if at the lens you intercept the Green-making and Blue-making rays, its redneſs will become more full and lively: but if you there intercept the Red-making rays, it will not any longer appear Red, but become Yellow or Green, or of ſome other colour, according to the ſorts of rays which you do not intercept. So Gold in this light, XY, appears of the ſame Yellow colour as in day-light; but by intercepting at the lens a due quantity of the Yellow-making rays, it will appear White like ſilver, as I have tried; which ſhews that its Yellowneſs ariſes from the exceſs of the intercepted rays tinging that Whiteneſs with their colour, when they are let paſs. So the infuſion of *Lignum Nephriticum*, as I have alſo tried, when held in this beam of light, XY, looks Blue by the reflected part of the light, and Red by the tranſmitted part of it, as when it is viewed in day-light: but, if you intercept the Blue at the lens, the infuſion will loſe its reflected Blue colour, whilſt its tranſmitted Red remains perfect, and by the loſs of ſome Blue-making rays, wherewith it was allayed, becomes more intenſe and full. And, on the contrary, if the Red and Orange-making rays be intercepted at the lens, the infuſion will loſe its tranſmitted Red, whilſt its Blue will remain, and become more full and perfect. Which ſhews, that the infuſion does not tinge the rays with Blue and Red, but only tranſmit thoſe moſt copiouſly which were Red-making before, and reflects thoſe moſt copiouſly which were Blue-making before. And after the ſame manner may the reaſons of other phænomena be examined, by trying them in this artificial beam of light, XY. the Solar Light.

THE

THE SECOND BOOK OF OPTICKS.

PART I.

Obſervations concerning the Reflexions, Refractions and Colours of thin tranſparent Bodies.

IT has been obſerved by others, that Tranſparent Subſtances, as Glaſs, Water, Air, &c. when made very thin by being blown into bubbles, or otherwiſe formed into plates, do exhibit various colours according to their various thinneſs; although at a greater thickneſs they appear very clear and colourleſs. In the former Book I forbore to treat of theſe colours; becauſe they ſeemed of a more difficult conſideration, and were not neceſſary for eſtabliſhing the properties of light there diſcourſed of. But becauſe they may conduce to farther diſcoveries for compleating the theory of light, eſpecially as to the conſtitution of the parts of Natural Bodies, on which their colours or tranſparency depend; I have here ſet down an account of them. To render this diſcourſe ſhort and diſtinct, I have firſt deſcribed the principal of my obſervations, and then conſidered and made uſe of them. The obſervations are theſe.

Obſ. 1. Compreſſing two priſms hard together, that their ſides, which by chance were a very little convex, might ſomewhere touch one another: I found the place in which they touched to become abſolutely tranſparent, as if they had there been one continued piece of glaſs. For when the light fell ſo obliquely on the air, which in other places was between them, as to be all reflected, it ſeemed in that place of contact to be wholly tranſmitted; inſomuch that when looked upon, it appeared like a black or dark ſpot, by reaſon that little or no ſenſible light was reflected from thence, as from other places; and, when looked through, it ſeemed as it were a hole in that air, which was formed into a thin plate by being compreſſed between the glaſſes. And through this hole objects, that were beyond, might be ſeen diſtinctly, which could not at all be ſeen through other parts of the glaſſes where the air was interjacent. Although the glaſſes were a little convex, yet this tranſparent ſpot was of a conſiderable breadth, which breadth ſeemed principally to proceed from the yielding inwards of the parts of the glaſſes, by reaſon of their mutual preſſure. For by preſſing them very hard together it would become much broader than otherwiſe.

Priſms compreſſed. Central Spot.

Obſ. 2. When the plate of air, by turning the priſms about their common axis, became ſo little inclined to the incident rays, that ſome of them began to be tranſmitted; there aroſe in it many ſlender arcs of colours which at firſt were ſhaped almoſt like the conchoid, as you ſee them delineated in the firſt figure. And by continuing the motion of the priſms, theſe arcs increaſed, and bended more and more about the ſaid tranſparent ſpot, till they were completed into circles or rings incompaſſing it, and afterwards continually grew more and more contracted.

Rings of Colours.

Theſe arcs at their firſt appearance were of a Violet and Blue colour, and between them were White arcs of circles, which preſently, by continuing the motion of the priſms, became a little tinged in their inward limbs with Red and Yellow, and to their outward limbs the Blue was adjacent. So that the order of theſe colours, from the central dark ſpot, was at that time White, Blue, Violet; Black, Red, Orange, Yellow, White, Blue, Violet,

Order of the Colours.

&c. But the Yellow and Red were much fainter than the Blue and Violet.

The motion of the prisms about their axis being continued, these colours contracted more and more, shrinking towards the Whiteness on either side of it, until they totally vanished into it. And then the circles in those parts appeared Black and White, without any other colours intermixed. But by farther moving the prisms about, the colours again emerged out of the Whiteness, the Violet and Blue at its inward limb, and at its outward limb the Red and Yellow. So that now their order from the central spot was White, Yellow, Red, Black, Violet, Blue, White, Yellow, Red, &c. contrary to what it was before.

Number of the Rings.

Obs. 3. When the rings or some parts of them appeared only Black and White, they were very distinct and well defined, and the Blackness seemed as intense as that of the central spot. Also in the borders of the rings, where the colours began to emerge out of the Whiteness, they were pretty distinct, which made them visible to a very great multitude. I have sometimes numbered above thirty successions, reckoning every Black and White ring for one succession, and seen more of them, which by reason of their smallness I could not number. But in other positions of the prisms, at which the rings appeared of many colours, I could not distinguish above eight or nine of them, and the exterior of those were very confused and dilute.

In these two Observations to see the rings distinct, and without any other colour than Black and White, I found it necessary to hold my eye at a good distance from them. For by approaching nearer, although in the same inclination of my eye to the plane of the rings, there emerged a Blueish colour out of the White; which, by dilating itself more and more into the Black, rendered the circles less distinct, and left the White a little tinged with Red and Yellow. I found also by looking through a slit or oblong hole, which was narrower than the pupil of my eye, and held close to it, parallel to the prisms; I could see the circles much distincter, and visible to a far greater number than otherwise.

Object-glasses compressed.

Obs. 4. To observe more nicely the order of the circles, which arose out of the White circles, as the rays became less and less inclined

inclined to the plate of air; I took two object-glaſſes, the one a plano-convex for a fourteen foot teleſcope, and the other a large double convex for one of about fifty foot; and upon this, laying the other with its plane ſide downwards, I preſſed them ſlowly together, to make the colours ſucceſſively emerge in the middle of the circles, and then ſlowly lifted the upper glaſs from the lower to make them ſucceſſively vaniſh again in the ſame place. The colour, which by preſſing the glaſſes together emerged laſt in the middle of the other colours, would, upon its firſt appearance, look like a circle of a colour almoſt uniform from the circumference to the center; and, by compreſſing the glaſſes ſtill more, grow continually broader until a new colour emerged in its center, and thereby it became a ring encompaſſing that new colour. And by compreſſing the glaſſes ſtill more, the diameter of this ring would increaſe, and the breadth of its orbit or perimeter decreaſe, until another new colour emerged in the center of the laſt: and ſo on until a third, a fourth, a fifth, and other following new colours ſucceſſively emerged there, and became rings encompaſſing the innermoſt colour, the leaſt of which was the Black ſpot. And, on the contrary, by lifting up the upper glaſs from the lower, the diameter of the rings would decreaſe, and the breadth of their orbit increaſe, until their colours reached ſucceſſively to the center; and then they being of a conſiderable breadth, I could more eaſily diſcern and diſtinguiſh their ſpecies than before. And by this means I obſerved their ſucceſſion and quantity to be as followeth.

Next to the pellucid central ſpot, made by the contact of the glaſſes, ſucceeded Blue, White, Yellow and Red. The Blue was ſo little in quantity that I could not diſcern it in the circles made by the priſms, nor could I well diſtinguiſh any Violet in it; but the Yellow and Red were pretty copious, and ſeemed about as much in extent as the White, and four or five times more than the Blue. The next circuit in order of colours, immediately encompaſſing theſe, were Violet, Blue, Green, Yellow and Red: and theſe were all of them copious and vivid, excepting the Green, which was very little in quantity, and ſeemed much more fair and dilute than the other colours. Of the other four, the Violet was

Succeſſionand Quantity of the Colours.

the leaſt in extent, and the Blue leſs than the Yellow or Red. The third circuit or order was Purple, Blue, Green, Yellow and Red; in which the Purple ſeemed more reddiſh than the Violet in the former circuit, and the Green was much more conſpicuous, being as briſk and copious as any of the other colours, except the Yellow; but the Red began to be a little faded, inclining very much to Purple. After this ſucceeded the fourth circuit of Green and Red. The Green was very copious and lively, inclining on the one ſide to Blue, and on the other ſide to Yellow. But in this fourth circuit there was neither Violet, Blue, nor Yellow, and the Red was very imperfect and dirty. Alſo the ſucceeding colours became more and more imperfect and dilute, till after three or four revolutions they ended in perfect Whiteneſs. Their form, when the glaſſes were moſt compreſſed ſo as to make the Black ſpot appear in the center, is delineated in the ſecond figure; where *a*, *b*, *c*, *d*, *e* : *f*, *g*, *h*, *i*, *k* : *l*, *m*, *n*, *o*, *p* : *q*, *r* : *s*, *t* : *v*, *x* : *y*, *z* denote the colours reckoned in order from the center; Black, Blue, White, Yellow, Red: Violet, Blue, Green, Yellow, Red: Purple, Blue, Green, Yellow, Red: Green, Red: greeniſh Blue, Red: greeniſh Blue, pale Red: greeniſh Blue, reddiſh White.

Thickneſs of the Plates of Air producing

Obſ. 5. To determine the interval of the glaſſes, or thickneſs of the interjacent air, by which each colour was produced, I meaſured the diameters of the firſt ſix rings at the moſt lucid part of their orbits; and ſquaring them, I found their ſquares to be in the arithmetical progreſſion of the odd numbers, 1, 3, 5, 7, 9, 11. And ſince one of theſe glaſſes was plane, and the other ſpherical, their intervals at thoſe rings muſt be in the ſame progreſſion. I meaſured alſo the diameters of the dark or faint rings between the more lucid colours, and found their ſquares to be in the arithmetical progreſſion of the even numbers, 2, 4, 6, 8, 10, 12. And it being very nice and difficult to take theſe meaſures exactly; I repeated them divers times at divers parts of the glaſſes, that by their agreement I might be confirmed in them. And the ſame method I uſed in determining ſome others of the following Obſervations.

Obſ.

Obſ. 6. The diameter of the ſixth ring, at the moſt lucid part of its orbit, was $\frac{58}{100}$ parts of an inch; and the diameter of the ſphere on which the double convex object-glaſs was ground was about 102 feet; and hence I gathered the thickneſs of the air, or aereal interval of the glaſſes at that ring. But ſome time after, ſuſpecting that in making this Obſervation I had not determined the diameter of the ſphere with ſufficient accurateneſs, and being uncertain whether the plano-convex glaſs was truly plane, and not ſomething concave or convex on that ſide which I accounted plane; and whether I had not preſſed the glaſſes together, as I often did, to make them touch; (for by preſſing ſuch glaſſes together their parts eaſily yield inwards, and the rings thereby become ſenſibly broader than they would be, did the glaſſes keep their figures) I repeated the experiment; and found the diameter of the ſixth lucid ring about $\frac{55}{100}$ parts of an inch. I repeated the experiment alſo with ſuch an object-glaſs of another teleſcope as I had at hand. This was a double convex, ground on both ſides to one and the ſame ſphere, and its focus was diſtant from it $83\frac{2}{5}$ inches. And thence, if the ſines of incidence and refraction of the bright Yellow light be aſſumed in proportion as 11 to 17, the diameter of the ſphere, to which the glaſs was figured, will by computation be found 182 inches. This glaſs I laid upon a flat one, ſo that the Black ſpot appeared in the middle of the rings of colours, without any other preſſure than that of the weight of the glaſs. And now meaſuring the diameter of the fifth dark circle, as accurately as I could, I found it the fifth part of an inch preciſely. This meaſure was taken with the points of a pair of compaſſes on the upper ſurface on the upper glaſs; and my eye was about eight or nine inches diſtant from the glaſs, almoſt perpendicularly over it; and the glaſs was $\frac{1}{6}$ of an inch thick: and thence it is eaſy to collect that the true diameter of the ring between the glaſſes was greater than its meaſured diameter above the glaſſes, in the proportion of 80 to 79, or thereabouts; and by conſequence equal to $\frac{16}{79}$ part of an inch, and its true ſemidiameter equal to $\frac{8}{79}$ parts. Now as the diameter of the ſphere, 182 inches, is to the ſemi-diameter of this fifth dark ring, $\frac{8}{79}$ parts of an inch, ſo is this ſemi-diameter to the thick- the ſeveral Colours.

Thickneſs of the Plates of Air

neſs of the air at this fifth dark ring; which is therefore $\frac{32}{567931}$ or $\frac{100}{1774784}$ parts of an inch; and the fifth part thereof, viz. the $\frac{1}{88739}$ part of an inch, is the thickneſs of the air at the firſt of theſe dark rings.

The ſame experiment I repeated with another double convex object-glaſs, ground on both ſides to one and the ſame ſphere. Its focus was diſtant from it $168\frac{1}{2}$ inches, and therefore the diameter of that ſphere was 184 inches. The glaſs being laid upon the ſame plain glaſs, the diameter of the fifth of the dark rings, when the black ſpot in their center appeared plainly without preſſing the glaſſes, was, by the meaſure of the compaſſes upon the upper glaſs, $\frac{121}{600}$ parts of an inch; and by conſequence between the glaſſes it was $\frac{1222}{6000}$. For the upper glaſs was $\frac{1}{8}$ of an inch thick, and my eye was diſtant from it 8 inches. And a third proportional to half this from the diameter of the ſphere is $\frac{5}{88850}$ parts of an inch. This is therefore the thickneſs of the air at this ring, and a fifth part thereof, viz. the $\frac{1}{88850}$th part of an inch is the thickneſs thereof at the firſt of the rings, as above.

I tried the ſame thing by laying theſe object-glaſſes upon flat pieces of a broken looking-glaſs, and found the ſame meaſures of the rings. Which makes me rely upon them, till they can be determined more accurately by glaſſes ground to larger ſpheres; though in ſuch glaſſes greater care muſt be taken of a true plane.

Theſe dimenſions were taken when my eye was placed almoſt perpendicularly over the glaſſes, being about an inch, or an inch and a quarter, diſtant from the incident rays, and eight inches diſtant from the glaſs; ſo that the rays were inclined to the glaſs in an angle of about four degrees. Whence by the following Obſervation you will underſtand, that had the rays been perpendicular to the glaſſes, the thickneſs of the air at theſe rings would have been leſs, in the proportion of the radius to the ſecant of four degrees, that is of 10000 to 10024. Let the thickneſſes found be therefore diminiſhed in this proportion, and they will become $\frac{1}{88952}$ and $\frac{1}{89063}$; or, to uſe the neareſt round number, the $\frac{1}{89000}$th

([n]) $84\frac{1}{13}$] I give this number as I find it in the third edition; which was the laſt publiſhed in our Author's life-time. The fourth edition, publiſhed in 1730 after his death, as was ſaid from a copy

producing the Colours ſeverally.

$\frac{1}{89000}$th part of an inch. This is the thickneſs of the air at the darkeſt part of the Firſt dark ring made by perpendicular rays: and half this thickneſs multiplied by the progreſſion 1, 3, 5, 7, 9, 11, &c. gives the thickneſſes of the air at the moſt luminous parts of all the brighteſt rings, viz. $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, &c. their arithmetical means $\frac{2}{178000}$, $\frac{4}{178000}$, $\frac{6}{178000}$, &c. being its thickneſſes at the darkeſt parts of all the dark ones.

Obſ. 7. The rings were leaſt, when my eye was placed perpendicularly over the glaſſes in the axis of the rings: and when I viewed them obliquely, they became bigger; continually ſwelling as I removed my eye farther from the axis. And partly by meaſuring the diameter of the ſame circle at ſeveral obliquities of my eye, partly by other means, as alſo by making uſe of the two priſms for very great obliquities, I found its diameter, and conſequently the thickneſs of the air at its perimeter, in all thoſe obliquities, to be very nearly in the proportions expreſſed in this Table.

Angle of Incidence on the Air.	Angle of Refraction into the Air.	Diameter of the Ring.	Thickneſs of the Air.
Degr. Min.			
00 00	00 00	10	10
06 26	10 00	$10\frac{1}{13}$	$10\frac{2}{13}$
12 45	20 00	$10\frac{1}{3}$	$10\frac{2}{3}$
18 49	30 00	$10\frac{3}{4}$	$11\frac{1}{2}$
24 30	40 00	$11\frac{2}{5}$	13
29 37	50 00	$12\frac{1}{2}$	$15\frac{1}{2}$
33 58	60 00	14	20
35 47	65 00	$15\frac{1}{4}$	$23\frac{1}{4}$
37 19	70 00	$16\frac{4}{5}$	$28\frac{1}{4}$
38 33	75 00	$19\frac{1}{4}$	37
39 27	80 00	$22\frac{6}{7}$	$52\frac{1}{4}$
40 00	85 00	29	$84\frac{1}{13}$ (n)
40 11	90 00	35	$122\frac{1}{2}$

In the two firſt columns are expreſſed the obliquities of the incident and emergent rays to the plate of the air; that is, their angles of incidence and refraction. In the third column, the diameter of any coloured ring at thoſe obliquities is expreſſed in parts, of which ten conſtitute that diameter, when the rays are perpendicular. And in the fourth column, the Thickneſs of the air, at the circumference of that ring, is expreſſed in parts of which alſo ten conſtitute its thickneſs, when the rays are perpendicular.

And from theſe meaſures I ſeem to gather this rule: That the Thickneſs of the air is proportional to the ſecant of an angle,

(n) a copy of the third edition corrected by the Author's own hand, and left before his death with the Bookſeller, has $84\frac{1}{12}$. But the better reading I take to be $84\frac{1}{10}$, which was the reading both of the firſt and ſecond edition, and of both the Latin editions of Dr. Clark.

whoſe

Thickneſs of the Plates of Air.

whoſe ſine is a certain mean proportional between the ſines of incidence and refraction. And that mean proportional, ſo far as by theſe meaſures I can determine it, is the firſt of an hundred and ſix arithmetical mean proportionals between thoſe ſines, counted from the bigger ſine; that is, from the ſine of refraction, when the refraction is made out of the glaſs into the plate of air; or from the ſine of incidence, when the refraction is made out of the plate of air into the glaſs.

Obſ. 8. The dark ſpot in the middle of the rings increaſed alſo by the obliquation of the eye, although almoſt inſenſibly. But if inſtead of the object-glaſſes the priſms were made uſe of, its increaſe was more manifeſt, when viewed ſo obliquely that no colours appeared about it. It was leaſt, when the rays were incident moſt obliquely on the interjacent air, and as the obliquity decreaſed, it increaſed more and more until the coloured rings appeared; and then decreaſed again, but not ſo much as it increaſed before. And hence it is evident, that the tranſparency was not only at the abſolute contact of the glaſſes, but alſo where they had ſome little interval. I have ſometimes obſerved the diameter of that ſpot to be between half and two-fifth parts of the diameter of the exterior circumference of the Red in the firſt circuit or revolution of colours, when viewed almoſt perpendicularly; whereas, when viewed obliquely, it hath wholly vaniſhed, and become opake and White like the other parts of the glaſs: whence it may be collected that the glaſſes did then ſcarcely, or not at all, touch one another; and that their interval at the perimeter of that ſpot, when viewed perpendicularly, was about a fifth or ſixth part of their interval at the circumference of the ſaid Red.

Ring of Colours made

Obſ. 9. By looking through the two contiguous object-glaſſes, I found that the interjacent air exhibited rings of colours, as well by tranſmitting light as by reflecting it. The central ſpot was now White, and from it the Order of the colours were yellowiſh Red; Black, Violet, Blue, White, Yellow, Red; Violet, Blue, Green, Yellow, Red, &c. But theſe colours were very faint and dilute, unleſs when the light was trajected very obliquely through the glaſſes: for by that means they became pretty vivid. Only the

the firſt yellowiſh Red, like the Blue in the fourth Obſervation, was ſo little and faint as ſcarcely to be diſcerned. Comparing the coloured rings made by reflexion, with theſe made by Tranſmiſſion of the light; I found that White was oppoſite to Black, Red to Blue, Yellow to Violet, and Green to a compound of Red and Violet. That is, thoſe parts of the glaſs were black when looked through, which when looked upon appeared White, and on the contrary. And ſo thoſe which in one caſe exhibited Blue, did in the other caſe exhibit Red. And the like of the other colours. The manner you have repreſented in the third figure, where AB, CD are the ſurfaces of the glaſſes contiguous at E, and the black lines between them are their diſtances in arithmetical progreſſion, and the colours written above are ſeen by Reflected light, and thoſe below by light Tranſmitted. by tranſmitted Light.

Obſ. 10. Wetting the object-glaſſes a little at their edges, the water crept in ſlowly between them; and the circles thereby became leſs, and the colours more faint: inſomuch that as the water crept along, one half of them at which it firſt arrived, would appear broken off from the other half, and contracted into a leſs room. By meaſuring them, I found the proportions of their diameters to the diameters of the like circles made by air to be about ſeven to eight; and conſequently the intervals of the glaſſes at like circles, cauſed by thoſe two mediums water and air, are as about three to four. Perhaps it may be a general rule, that if any other medium more or leſs denſe than water be compreſſed between the glaſſes, their intervals, at the rings cauſed thereby, will be to their intervals cauſed by interjacent air, as the ſines are, which meaſure the refraction made out of that medium into air. Rings of Colours made by Plates of Water.

Obſ. 11. When the water was between the glaſſes, if I preſſed the upper glaſs variouſly at its edges, to make the rings move nimbly from one place to another, a little white ſpot would immediately follow the center of them, which upon creeping in of the ambient water into that place would preſently vaniſh. Its appearance was ſuch as interjacent air would have cauſed, and it exhibited the ſame colours. But it was not air, for where any bubbles of air were in the water they would not vaniſh. The reflexion muſt have rather been cauſed by a ſubtiler medium, Indication of a Medium more ſubtil than Air.

which could recede through the glaſſes at the creeping in of the water.

The Rings viewed

Obſ. 12. Theſe obſervations were made in the open air. But farther to examine the effects of coloured light falling on the glaſſes, I darkened the room, and viewed them by reflexion of the colours of a priſm caſt on a ſheet of white paper; my eye being ſo placed that I could ſee the coloured paper by reflexion in the glaſſes, as in a looking-glaſs. And by this means the rings became diſtincter and viſible to a far greater number than in the open air. I have ſometimes ſeen more than twenty of them, whereas in the open air I could not diſcern above eight or nine.

Obſ. 13. Appointing an aſſiſtant to move the priſm to and fro about its axis, that all the colours might ſucceſſively fall on that part of the paper, which I ſaw by reflexion, from that part of the glaſſes, where the circles appeared, ſo that all the colours might be ſucceſſively reflected from the circles to my eye, whilſt I held it immoveable; I found the circles which the Red light made to be manifeſtly bigger than thoſe which were made by the Blue and Violet. And it was very pleaſant to ſee them gradually ſwell or contract accordingly as the colour of the light was changed. The interval of the glaſſes at any of the rings, when they were made by the outmoſt Red light, was to their interval at the ſame ring when made by the outmoſt Violet, greater than as 3 to 2, and leſs than as 13 to 8. By the moſt of my obſervations it was as 14 to 9. And this proportion ſeemed very nearly the ſame in all obliquities of my eye; unleſs when two priſms were made uſe of inſtead of the object-glaſſes. For then, at a certain great obliquity of my eye, the rings made by the ſeveral colours ſeemed equal; and at a greater obliquity, thoſe made by the Violet would be greater than the ſame rings made by the Red: the refraction of the priſm in this caſe cauſing the moſt refrangible rays to fall more obliquely on that plate of the air than the leaſt refrangible ones. Thus the experiment ſucceeded in the coloured light, which was ſufficiently ſtrong and copious to make the rings ſenſible. And thence it may be gathered, that if the moſt refrangible and leaſt refrangible rays had been copious enough to make the rings ſenſible without the mixture of other rays, the proportion

proportion which here was 14 to 9 would have been a little greater, ſuppoſe $14\frac{1}{4}$ or $14\frac{1}{3}$ to 9. by Priſmatic Light.

Obſ. 14. Whilſt the priſm was turned about its axis with an uniform motion, to make all the ſeveral colours fall ſucceſſively upon the object-glaſſes, and thereby to make the rings contract and dilate: the contraction or dilatation of each ring, thus made by the variation of its colour, was ſwifteſt in the Red, and ſloweſt in the Violet, and in the intermediate colours it had intermediate degrees of celerity. Comparing the quantity of contraction and dilatation made by all the degrees of each colour, I found that it was greateſt in the Red; leſs in the Yellow; ſtill leſs in the Blue; and leaſt in the Violet. And to make as juſt an eſtimation as I could of the proportions of their contractions or dilatations, I obſerved that the whole contraction or dilatation of the diameter of any ring, made by all the degrees of Red, was to that of the diameter of the ſame ring, made by all the degrees of Violet, as about four to three, or five to four; and that when the light was of the middle colour between Yellow and Green, the diameter of the ring was very nearly an arithmetical mean between the greateſt diameter of the ſame ring made by the outmoſt Red, and the leaſt diameter thereof made by the outmoſt Violet; contrary to what happens in the colours of the oblong ſpectrum made by the refraction of a priſm, where the Red is moſt contracted, the Violet moſt expanded, and in the midſt of all the colours is the comfine of Green and Blue. And hence I ſeem to collect that the Thickneſſes of the air between the glaſſes there, where the ring is ſucceſſively made by the limits of the five principal colours, Red, Yellow, Green, Blue, Violet, in order; that is, by the extreme Red, by the limit of Red and Yellow in the middle of the Orange, by the limit of Yellow and Green, by the limit of Green and Blue, by the limit of Blue and Violet in the middle of the Indigo, and by the extreme Violet; are to one another very nearly as the ſix lengths of a chord which ſound the notes in a ſixth major, *ſol*, *la*, *mi*, *fa*, *ſol*, *la*. But it agrees ſomething better with the obſervation to ſay, that the Thickneſſes of the air between the glaſſes there, where the rings are ſucceſſively made by the limits of the ſeven colours, Red, Orange,

The Rings viewed by Priſmatic Light.

Yellow, Green, Blue, Indigo, Violet in order, are to one another as the cube-roots of the ſquares of the eight lengths of a chord, which ſound the notes in an eighth, *ſol*, *la*, *fa*, *ſol*, *la*, *mi*, *fa*, *ſol*; that is, as the cube-roots of the ſquares of the numbers, 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, $\frac{1}{2}$.

Obſ. 15. Theſe rings were not of various colours, like thoſe made in the open air, but appeared all over of that Priſmatick colour only with which they were illuminated. And by projecting the Priſmatick colours immediately upon the glaſſes, I found that the light which fell on the dark ſpaces, which were between the coloured rings, was tranſmitted through the glaſſes without any variation of colour. For on a white paper placed behind, it would paint rings of the ſame colour with thoſe which were reflected, and of the bigneſs of their immediate (°) ſpaces. And from thence the origin of theſe rings is manifeſt; namely, that the air between the glaſſes, according to its various Thickneſs, is diſpoſed in ſome places to reflect, and in others to tranſmit the light of any one colour, as you may ſee repreſented in the fourth figure; and in the ſame place to reflect that of one colour, where it tranſmits that of another.

Obſ. 16. The ſquares of the diameters of theſe rings, made by any priſmatick colour, were in arithmetical progreſſion, as in the fifth Obſervation. And the diameter of the ſixth circle, when made by the Citrine Yellow, and viewed almoſt perpendicularly, was about $\frac{58}{100}$ parts of an inch, or a little leſs, agreeable to the ſixth Obſervation.

The precedent Obſervations were made with a rarer thin medium, terminated by a denſer, ſuch as was air or water compreſſed between two glaſſes. In thoſe that follow are ſet down the appearances of a denſer medium thinned within a rarer; ſuch as are plates of Muſcovy glaſs, bubbles of water, and ſome other thin ſubſtances terminated on all ſides with air.

Rings of Colours

Obſ. 17. If a bubble be blown with water firſt made tenacious by diſſolving a little ſoap in it, it is a common obſervation, that after a while it will appear tinged with a great variety of colours. To defend theſe bubbles from being agitated by the external air

(°) Perhaps *intermediate*.

(whereby their colours are irregularly moved one among another, ſo that no accurate obſervation can be made of them) as ſoon as I had blown any of them I covered it with a clear glaſs, and by that means its colours emerged in a very regular order, like ſo many concentrick rings encompaſſing the top of the bubble. And as the bubble grew thinner, by the continual ſubſiding of the water, theſe rings dilated ſlowly and overſpread the whole bubble, deſcending in order to the bottom of it, where they vaniſhed ſucceſſively. In the mean while, after all the colours were emerged at the top, there grew in the center of the rings a ſmall round Black ſpot, like that in the firſt Obſervation; which continually dilated itſelf till it became ſometimes more than $\frac{1}{2}$ or $\frac{3}{4}$ of an inch in breadth, before the bubble broke. At firſt I thought there had been no light reflected from the water in that place; but obſerving it more curiouſly, I ſaw within it ſeveral ſmaller round ſpots, which appeared much blacker and darker than the reſt; whereby I knew that there was ſome reflexion at the other places which were not ſo dark as thoſe ſpots. And by farther trial I found that I could ſee the images of ſome things (as of a candle or the ſun) very faintly reflected, not only from the great Black ſpot, but alſo from the little darker ſpots which were within in it. in bubbles of Water.

Beſides the aforeſaid coloured rings there would often appear ſmall ſpots of colours, aſcending and deſcending up and down the ſides of the bubble, by reaſon of ſome inequalities in the ſubſiding of the water. And ſometimes ſmall Black ſpots, generated at the ſides, would aſcend up to the larger Black ſpot at the top of the bubble, and unite with it.

Obſ. 18. Becauſe the colours of theſe bubbles were more extended and lively than thoſe of the air thinned between two glaſſes, and ſo more eaſy to be diſtinguiſhed, I ſhall here give you a farther deſcription of their order, as they were obſerved in viewing them by reflexion of the ſkies, when of a White colour, whilſt a black ſubſtance was placed behind the bubble. And they were theſe; Red, Blue; Red, Blue; Red, Blue; Red, Green; Red, Yellow, Green, Blue, Purple; Red, Yellow, Green, Blue, Violet; Red, Yellow, White, Blue, Black.

The

Rings of Colours

The three firſt ſucceſſions of Red and Blue were very dilute and dirty, eſpecially the firſt, where the Red ſeemed in a manner to be White. Among theſe there was ſcarce any other colour ſenſible beſides Red and Blue, only the Blues, and principally the ſecond Blue, inclined a little to Green.

The fourth Red was alſo dilute and dirty, but not ſo much as the former three; after that ſucceeded little or no Yellow, but a copious Green, which at firſt inclined a little to Yellow, and then became a pretty briſk and good Willow Green, and afterwards changed to a blueiſh colour; but there ſucceeded neither Blue nor Violet.

The fifth Red at firſt inclined very much to Purple, and afterwards became more bright and briſk, but yet not very pure. This was ſucceeded with a very bright and intenſe Yellow, which was but little in quantity, and ſoon changed to Green: but that Green was copious, and ſomething more pure, deep and lively, than the former Green. After that followed an excellent Blue of a bright ſky-colour, and then a Purple, which was leſs in quantity than the Blue, and much inclined to Red.

The ſixth Red was at firſt of a very fair and lively Scarlet, and ſoon after of a brighter colour, being very pure and briſk, and the beſt of all the Reds. Then, after a lively Orange, followed an intenſe bright and copious Yellow; which was alſo the beſt of all the Yellows, and this changed firſt to a greeniſh Yellow, and then to a greeniſh Blue: but the Green between the Yellow and the Blue, was very little and dilute, ſeeming rather a greeniſh White than a Green. The Blue which ſucceeded became very good, and of a very fair bright ſky-colour, but yet ſomething inferior to the former Blue; and the Violet was intenſe and deep with little or no Redneſs in it. And leſs in quantity than the Blue.

In the laſt Red appeared a tincture of Scarlet next to Violet, which ſoon changed to a brighter colour, inclining to an Orange; and the Yellow which followed was at firſt pretty good and lively, but afterwards it grew more dilute, until by degrees it ended in perfect Whiteneſs. And this Whiteneſs, if the water was very tenacious and well tempered, would ſlowly ſpread and dilate itſelf over

over the greater part of the bubble, continually growing paler at the top; where at length it would crack in many places; and thoſe cracks, as they dilated, would appear of a pretty good, but yet obſcure and dark ſky-colour; the White between the Blue ſpots diminiſhing, until it reſembled the threads of an irregular net-work, and ſoon after vaniſhed and left all the upper part of the bubble of the ſaid dark Blue colour. And this colour, after the aforeſaid manner, dilated itſelf downwards, until ſometimes it hath overſpread the whole bubble. In the mean while at the top, which was of a darker Blue than the bottom, and appeared alſo full of many round blue ſpots, ſomething darker than the reſt, there would emerge one or more very Black ſpots; and within thoſe, other ſpots of an intenſer Blackneſs, which I mentioned in the former Obſervation; and theſe continually dilated themſelves until the bubble broke. in bubbles of Water.

If the water was not very tenacious, the Black ſpots would break forth in the White, without any ſenſible intervention of the Blue. And ſometimes they would break forth within the precedent Yellow, or Red, or perhaps within the Blue of the ſecond order, before the intermediate colours had time to diſplay themſelves.

By this deſcription you may perceive, how great an affinity theſe colours have with thoſe of air deſcribed in the fourth Obſervation, although ſet down in a contrary order; by reaſon that they begin to appear when the bubble is thickeſt, and are moſt conveniently reckoned from the loweſt and thickeſt part of the bubble upwards.

Obſ. 19. Viewing, in ſeveral oblique poſitions of my eye, the rings of colours emerging on the top of the bubble, I found that they were ſenſibly dilated by increaſing the Obliquity, but yet not ſo much by far as thoſe made by thinned air in the ſeventh Obſervation. For there they were dilated ſo much as, when viewed moſt obliquely, to arrive at a part of the plate more than twelve times thicker, than that where they appeared when viewed perpendicularly; whereas in this caſe, the thickneſs of the water, at which they arrived when viewed moſt obliquely, was to that thickneſs which exhibited them by perpendicular rays, ſomething leſs than as 8 to 5. By the beſt of my obſervations

it

Rings of Colours

it was between 15 and 15½ to 10; an increaſe about 24 times leſs than in the other caſe.

Sometimes the bubble would become of an uniform thickneſs all over, except at the top of it near the Black ſpot; as I knew, becauſe it would exhibit the ſame appearance of colours in all poſitions of the eye. And then the colours, which were ſeen at its apparent circumference by the obliqueſt rays, would be different from thoſe that were ſeen in other places, by rays leſs oblique to it. And divers ſpectators might ſee the ſame part of it of differing colours, by viewing it at very differing obliquities. Now obſerving how much the colours at the ſame places of the bubble, or at divers places of equal thickneſs, were varied by the ſeveral obliquities of the rays; by the aſſiſtance of the 4th, 14th, 16th and 18th Obſervations, as they are hereafter explained, I collect the Thickneſs of the water requiſite to exhibit any one and the ſame colour, at ſeveral obliquities, to be very nearly in the proportion expreſſed in this Table.

Incidence on the Water.		Refraction into the Water.		Thickneſs of the Water.
Degr.	Min.	Degr.	Min.	
00	00	00	00	10
15	00	11	11	10¼
30	00	22	1	10⅘
45	00	32	2	11⅘
60	00	40	30	13
75	00	46	25	14½
90	00	48	35	15⅕

In the two firſt columns are expreſſed the obliquities of the rays to the ſuperficies of the water, that is, their angles of incidence and refraction. Where I ſuppoſe that the ſines, which meaſure them, are in round numbers as 3 to 4; though probably the diſſolution of ſoap in the water, may a little alter its Refractive virtue. In the third column the Thickneſs of the bubble, at which any one colour is exhibited in thoſe ſeveral obliquities, is expreſſed in parts, of which ten conſtitute its Thickneſs when the rays are perpendicular. And the rule found by the ſeventh Obſervation agrees well with theſe meaſures, if duly applied; namely, that the Thickneſs of a plate of water requiſite to exhibit one and the ſame colour at ſeveral obliquities of the eye, is proportional to the ſecant of an angle, whoſe ſine is the firſt of an hundred and ſix arithmetical mean proportionals between the ſines of incidence and refraction counted from the leſſer ſine,

ſine, that is, from the ſine of refraction, when the refraction is made out of air into water, otherwiſe from the ſine of incidence. in bubbles of Water.

I have ſometimes obſerved, that the colours which ariſe on poliſhed ſteel by heating it, or on bell-metal, and ſome other metalline ſubſtances, when melted and poured on the ground, where they may cool in the open air, have, like the colours of water-bubbles, been a little changed by viewing them at divers obliquities: and particularly that a deep Blue, or Violet, when viewed very obliquely, hath been changed to a deep Red. But the changes of theſe colours are not ſo great and ſenſible, as of thoſe made by water. For the *Scoria*, or vitrified part of the metal, which moſt metals when heated or melted do continually protrude, and ſend out to their ſurface, and which, by covering the metals in form of a thin glaſſy ſkin, cauſes theſe colours, is much denſer than water; and I find that the change, made by the obliquation of the eyes, is leaſt in colours of the denſeſt thin ſubſtances. Change of Colour by Obliquation of the Eye leſs in the denſer ſubſtances.

Obſ. 20. As in the ninth Obſervation ſo here, the bubble, by tranſmitted light, appeared of a contrary colour to that which it exhibited by reflexion. Thus when the bubble, being looked on by the light of the clouds reflected from it, ſeemed Red at its apparent circumference; if the clouds at the ſame time, or immediately after, were viewed through it, the colour at its circumference would be Blue. And, on the contrary, when by reflected light it appeared Blue, it would appear Red by tranſmitted light. Rings of Colours in bubbles of Water by tranſmitted Light.

Obſ. 21. By wetting very thin plates of Muſcovy glaſs, whoſe thinneſs make the like colours appear, the colours became more faint and languid; eſpecially by wetting the plates on that ſide oppoſite to the eye: but I could not perceive any variation of their ſpecies. So then the Thickneſs of a plate requiſite to produce any colour, depends only on the denſity of the plate, and not on that of the ambient medium. And hence, by the 10th and 16th Obſervations, may be known the Thickneſs which bubbles of water, or plates of Muſcovy glaſs, or other ſubſtances, have at any colour produced by them. Species of Colour depends on the denſity of the Plate not on that of the ambient Medium.

Obſ. 22. A thin tranſparent body, which is denſer than its ambient medium, exhibits more briſk and vivid colours than that

which is ſo much rarer; as I have particularly obſerved in the air and glaſs. For blowing glaſs very thin at a lamp-furnace, thoſe plates encompaſſed with air did exhibit colours much more vivid than thoſe of air made thin between two glaſſes.

Obſ. 23. Comparing the Quantity of light reflected from the ſeveral rings, I found that it was moſt copious from the firſt or inmoſt, and in the exterior rings became gradually leſs and leſs. Alſo the Whiteneſs of the firſt ring was ſtronger than that reflected from thoſe parts of the thin medium or plate, which were without the rings; as I could manifeſtly perceive, by viewing at a diſtance the rings made by the two object-glaſſes; or by comparing two bubbles of water blown at diſtant times, in the firſt of which the Whiteneſs appeared, which ſucceeded all the colours, and in the other, the Whiteneſs which preceded them all.

Rings viewed

Obſ. 24. When the two object-glaſſes were laid upon one another, ſo as to make the rings of the colours appear, though with my naked eye I could not diſcern above eight or nine of thoſe rings, yet by viewing them through a priſm I have ſeen a far greater multitude; inſomuch that I could number more than forty, beſides many others, that were ſo very ſmall and cloſe together, that I could not keep my eye ſteady on them ſeverally ſo as to number them, but by their extent I have ſometimes eſtimated them to be more than an hundred. And I believe the Experiment may be improved to the diſcovery of far greater numbers. For they ſeem to be really unlimited, though viſible only ſo far as they can be ſeparated by the refraction of the priſm, as I ſhall hereafter explain.

But it was but one ſide of theſe rings, namely, that towards which the refraction was made, which by that refraction was rendered diſtinct; and the other ſide became more confuſed than when viewed by the naked eye: inſomuch that there I could not diſcern above one or two, and ſometimes none of thoſe rings, of which I could diſcern eight or nine with my naked eye. And their ſegments or arcs, which on the other ſide appeared ſo numerous, for the moſt part exceeded not the third part of a circle. If the refraction was very great, or the priſm very diſtant from the object-

object-glaſſes, the middle part of thoſe arcs became alſo confuſed, ſo as to diſappear and conſtitute an even Whiteneſs: whilſt on either ſide, their ends, as alſo the whole arcs fartheſt from the center, became diſtincter than before, appearing in the form as you ſee them deſigned in the fifth figure. by Priſmatic Light.

The arcs, where they ſeemed diſtincteſt, were only White and Black ſucceſſively, without any other colours intermixed. But in other places there appeared colours, whoſe order was inverted by the refraction in ſuch manner, that if I firſt held the priſm very near the object-glaſſes, and then gradually removed it farther off towards my eye, the colours of the 2d, 3d, 4th, and following rings ſhrunk towards the White that emerged between them, until they wholly vaniſhed into it at the middle of the arcs, and afterwards emerged again in a contrary order. But at the ends of the arcs they retained their order unchanged.

I have ſometimes ſo laid one object-glaſs upon the other, that to the naked eye they have all over ſeemed uniformly White, without the leaſt appearance of any of the coloured rings; and yet by viewing them through a priſm, great multitudes of thoſe rings have diſcovered themſelves. And in like manner plates of Muſcovy glaſs, and bubbles of glaſs blown at a lamp-furnace, which were not ſo thin as to exhibit any colours to the naked eye, have through the priſm exhibited a great variety of them ranged irregularly up and down in the form of waves. And ſo bubbles of water, before they began to exhibit their colours to the naked eye of a by-ſtander, have appeared through a priſm, girded about with many parallel and horizontal rings; to produce which effect, it was neceſſary to hold the priſm parallel, or very nearly parallel to the horizon, and to diſpoſe it ſo, that the rays might be refracted upwards.

THE

SECOND BOOK

OF

OPTICKS.

PART II.

Remarks upon the foregoing Obſervations.

Generation of the Rings

HAVING given my obſervations of theſe colours, before I make uſe of them to unfold the cauſes of the colours of Natural Bodies, it is convenient that by the ſimpleſt of them, ſuch as are the 2d, 3d, 4th, 9th, 12th, 18th, 20th, and 24th, I firſt explain the more compounded. And firſt to ſhew how the colours in the fourth and eighteenth Obſervations are produced, let there be taken in any right line from the point Y [in *fig.* 6.] the lengths YA, YB, YC, YD, YE, YF, YG, YH, in proportion to one another, as the cube-roots of the ſquares of the numbers, $\frac{1}{2}$, $\frac{9}{16}$, $\frac{3}{5}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{5}{6}$, $\frac{8}{9}$, 1, whereby the lengths of a Muſical chord to ſound all the notes in an eighth are repreſented; that is, in the proportion of the numbers 6300, 6814, 7114, 7631, 8255, 8855, 9343, 10000. And at the points A, B, C, D, E, F, G, H, let perpendiculars Aα, Bβ, &c. be erected, by whoſe intervals the extent of the ſeveral colours ſet underneath againſt them, is to be repreſented.

repreſented. Then divide the line Aα in ſuch proportion as the numbers 1, 2, 3, 5, 6, 7, 9, 10, 11, &c. ſet at the points of diviſion denote. And through thoſe diviſions from Y draw lines 1I, 2K, 3L, 5M, 6N, 7O, &c. of reflected Colours.

Now if A2 be ſuppoſed to repreſent the thickneſs of any thin tranſparent body, at which the outmoſt Violet is moſt copiouſly reflected in the firſt ring, or ſeries of colours; then, by the 13th Obſervation (°), HK will repreſent its thickneſs, at which the outmoſt Red is moſt copiouſly reflected in the ſame ſeries. Alſo by the 5th and 16th Obſervations, A6 and HN will denote the thickneſſes at which thoſe extreme colours are moſt copiouſly reflected in the ſecond ſeries; and A10 and HQ the thickneſſes, at which they are moſt copiouſly reflected in the third ſeries, and ſo on. And the thickneſs at which any of the intermediate colours are reflected moſt copiouſly, will, according to the 14th Obſervation, be defined by the diſtance of the line AH from the intermediate parts of the lines 2K, 6N, 10Q, &c. againſt which the names of thoſe colours are written below.

But farther, to define the latitude of theſe colours in each ring or ſeries, let A1 deſign the leaſt thickneſs, and A3 the greateſt thickneſs, at which the extreme Violet in the Firſt ſeries is reflected; and let HI, and HL, deſign the like limits for the extreme Red; and let the intermediate colours be limited by the intermediate parts of the lines 1I, and 3L, againſt which the names of thoſe colours are written, and ſo on: but yet with this caution, that the reflexions be ſuppoſed ſtrongeſt at the intermediate ſpaces, 2K, 6N, 10Q, &c. and from thence to decreaſe gradually towards theſe limits, 1I, 3L, 5M, 7O, &c. on either ſide; where you muſt not conceive them to be preciſely limited, but to decay indefinitely. And whereas I have aſſigned the ſame latitude to every ſeries; I did it, becauſe although the colours in the firſt ſeries ſeem to be a little broader than the reſt, by reaſon of a ſtronger reflexion there, yet that inequality is ſo inſenſible as ſcarcely to be determined by obſervation.

Now according to this deſcription, conceiving that the rays, originally of ſeveral colours, are by turns reflected at the ſpaces

(°) — *by the 13th Obſervation.*] Rather by the 14th.

Generation of the Rings

1IL3, 5MO7, 9PR11, &c. and tranſmitted at the ſpaces AHI1, 3LM5, 7OP9, &c. it is eaſy to know what colour muſt, in the open air, be exhibited at any Thickneſs of a tranſparent thin body. For if a ruler be applied parallel to AH, at that diſtance from it, by which the thickneſs of the body is repreſented, the alternate ſpaces, 1IL3, 5MO7, &c. which it croſſeth, will denote the reflected original colours, of which the colour, exhibited in the open air, is compounded. Thus if the conſtitution of the Green in the Third ſeries of colours be deſired, apply the ruler as you ſee at $\pi\rho\sigma\phi$; and by its paſſing through ſome of the Blue at π, and Yellow at σ, as well as through the Green at ρ, you may conclude that the Green exhibited at that thickneſs of the body is principally conſtituted of original Green, but not without a mixture of ſome Blue and Yellow.

By this means you may know how the colours, from the center of the rings outward, ought to ſucceed in order as they were deſcribed in the 4th and 18th Obſervations. For if you move the ruler gradually from AH through all diſtances, having paſſed over the firſt ſpace, which denotes little or no reflexion to be made by thinneſt ſubſtances, it will firſt arrive at 1 the Violet; and then very quickly at the Blue and Green, which together with that Violet compound Blue; and then at the Yellow and Red, by whoſe farther addition that Blue is converted into Whiteneſs, which whiteneſs continues during the tranſit of the edge of the ruler from I to 3; and after that, by the ſucceſſive deficience of its component colours, turns firſt to compound Yellow, and then to Red, and laſt of all the Red ceaſeth at L. Then begin the colours of the ſecond ſeries, which ſucceed in order during the tranſit of the edge of the ruler from 5 to O, and are more lively than before, becauſe more expanded and ſevered. And for the ſame reaſon, inſtead of the former White, there intercedes between the Blue and Yellow a mixture of Orange, Yellow, Green, Blue and Indigo, all which together ought to exhibit a dilute and imperfect Green. So the colours of the third ſeries all ſucceed in order; firſt, the Violet, which a little interferes with the Red of the ſecond order, and is thereby inclined to a reddiſh Purple; then the Blue and Green, which are leſs mixed with other colours,

lours, and conſequently more lively than before, eſpecially the Green: then follows the Yellow, ſome of which towards the Green is diſtinct and good, but that part of it towards the ſucceeding Red, as alſo that Red is mixed with the Violet and Blue of the fourth ſeries; whereby various degrees of Red very much inclining to Purple are compounded. The Violet and Blue, which ſhould ſucceed this Red, being mixed with, and hidden in it, there ſucceeds a Green. And this at firſt is much inclined to Blue, but ſoon becomes a good Green, the only unmixed and lively colour in this Fourth ſeries. For as it verges towards the Yellow, it begins to interfere with the colours of the Fifth ſeries; by whoſe mixture the ſucceeding Yellow and Red are very much diluted and made dirty, eſpecially the Yellow, which, being the weaker colour, is ſcarce able to ſhew itſelf. After this the ſeveral ſeries interfere more and more, and their colours become more and more intermixed, till after three or four more revolutions (in which the Red and Blue predominate by turns) all ſorts of colours are in all places pretty equally blended, and compound an even Whiteneſs. of reflected Colours.

And ſince by the 15th Obſervation the rays endued with one colour are tranſmitted, where thoſe of another colour are reflected, the reaſon of the colours made by the tranſmitted light in the 9th and 20th Obſervations is from hence evident. Of Tranſmitted Colours.

If not only the order and ſpecies of theſe colours, but alſo the preciſe Thickneſs of the plate, or thin body at which they are exhibited, be deſired in parts of an inch, that may be alſo obtained by aſſiſtance of the 6th or 16th Obſervations. For according to thoſe Obſervations the thickneſs of the thinned air, which between two glaſſes exhibited the moſt luminous parts of the firſt ſix rings, were $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, $\frac{9}{178000}$, $\frac{11}{178000}$ parts of an inch. Suppoſe the light reflected moſt copiouſly at theſe thickneſſes be the bright citrine Yellow, or confine of Yellow and Orange; and theſe thickneſſes will be $F\lambda$, $F\mu$, $F\xi$, Fo, $F\pi$. And this being known, it is eaſy to determine what Thickneſs of air is repreſented by $G\rho$, or by any other diſtance of the ruler from AH. Thickneſs of coloured Plates.

3 But

Thickneſs of coloured Plates.

But farther, ſince by the 10th Obſervation the thickneſs of air was to the thickneſs of water, which between the ſame glaſſes exhibited the ſame colour, as 4 to 3; and by the 21ſt Obſervation the colours of thin bodies are not varied by varying the ambient medium: the Thickneſs of a bubble of water, exhibiting any colour, will be $\frac{3}{4}$ of the thickneſs of air producing the ſame colour. And ſo according to the ſame 10th and 21ſt Obſervations the Thickneſs of a plate of glaſs, whoſe refraction of the mean refrangible ray is meaſured by the proportion of the ſines 31 to 20, may be $\frac{20}{31}$ of the thickneſs of air producing the ſame colours; and the like of other mediums. I do not affirm, that this proportion of 20 to 31 holds in all the rays; for the ſines of other ſorts of rays have other proportions. But the differences of thoſe proportions are ſo little, that I do not here conſider them. On theſe grounds I have compoſed the following Table, wherein the Thickneſs of air, water, and glaſs, at which each colour is moſt intenſe and ſpecific, is expreſſed in parts of an inch divided into ten hundred thouſand equal parts.

The

The thickneſs of coloured Plates and Particles of

and particles of Air, Water, and Glaſs.

		Air.	Water.	Glaſs.
Their Colours of the firſt Order,	Very Black	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{10}{31}$
	Black	1	$\frac{3}{4}$	$\frac{20}{31}$
	Beginning of Black	2	$1\frac{1}{2}$	$1\frac{2}{7}$
	Blue	$2\frac{2}{5}$	$1\frac{4}{5}$	$1\frac{11}{20}$
	White	$5\frac{1}{4}$	$3\frac{7}{8}$	$3\frac{2}{5}$
	Yellow	$7\frac{1}{9}$	$5\frac{1}{3}$	$4\frac{3}{5}$
	Orange	8	6	$5\frac{1}{6}$
	Red	9	$6\frac{3}{4}$	$5\frac{4}{5}$
Of the ſecond Order,	Violet	$11\frac{1}{6}$	$8\frac{3}{8}$	$7\frac{1}{5}$
	Indigo	$12\frac{5}{6}$	$9\frac{5}{8}$	$8\frac{2}{11}$
	Blue	14	$10\frac{1}{2}$	9
	Green	$15\frac{1}{8}$	$11\frac{1}{3}$	$9\frac{5}{7}$
	Yellow	$16\frac{2}{7}$	$12\frac{1}{5}$	$10\frac{2}{5}$
	Orange	$17\frac{2}{9}$	13	$11\frac{1}{9}$
	Bright Red	$18\frac{1}{5}$ (P)	$13\frac{3}{4}$	$11\frac{5}{6}$
	Scarlet	$19\frac{2}{3}$	$14\frac{3}{4}$	$12\frac{2}{3}$
Of the third Order,	Purple	21	$15\frac{3}{4}$	$13\frac{11}{20}$
	Indigo	$22\frac{1}{10}$	$16\frac{4}{7}$	$14\frac{1}{4}$
	Blue	$23\frac{2}{5}$	$17\frac{11}{20}$	$15\frac{1}{10}$
	Green	$25\frac{1}{5}$	$18\frac{9}{10}$	$16\frac{1}{4}$
	Yellow	$27\frac{1}{7}$	$20\frac{1}{3}$	$17\frac{1}{2}$
	Red	29	$21\frac{3}{4}$	$18\frac{5}{7}$
	Blueiſh Red	32	24	$20\frac{2}{3}$
Of the fourth Order,	Blueiſh Green	24	$25\frac{1}{2}$	22
	Green	$35\frac{2}{7}$	$26\frac{1}{6}$	$22\frac{3}{4}$
	Yellowiſh Green	36	27	$23\frac{2}{9}$
	Red	$40\frac{1}{3}$	$30\frac{1}{4}$	26
Of the fifth Order,	Greeniſh Blue	46	$34\frac{1}{2}$	$29\frac{2}{3}$
	Red	$52\frac{1}{2}$	$39\frac{3}{8}$	34
Of the ſixth Order,	Greeniſh Blue	$58\frac{3}{4}$	44	38
	Red	65	$48\frac{3}{4}$	42
Of the ſeventh Order,	Greeniſh Blue	71	$53\frac{1}{4}$	$45\frac{4}{5}$
	Ruddy White	77	$57\frac{3}{4}$	$49\frac{2}{3}$

Now if this Table be compared with the 6th Scheme, you will there ſee the conſtitution of each colour, as to its ingredients, or the original colours of which it is compounded; and thence be enabled to judge of its intenſeneſs or imperfection; which may ſuffice in explication of the 4th and 18th Obſervations, unleſs it be farther deſired to delineate the manner how the colours ap-

(P) *Bright red* $18\frac{1}{5}$.] So the third edition. All the reſt $18\frac{1}{5}$.

pear, when the two object-glaſſes are laid upon one another. To do which, let there be deſcribed a large arc of a circle, and a ſtreight line which may touch that arc, and parallel to that tangent ſeveral occult lines, at ſuch diſtances from it, as the numbers ſet againſt the ſeveral colours in the Table denote. For the arc, and its tangent, will repreſent the ſuperficies of the glaſſes terminating the interjacent air; and the places where the occult lines cut the arc will ſhow at what diſtances from the center, or point of contact, each colour is reflected.

There are alſo other uſes of this Table: for by its aſſiſtance the thickneſs of the bubble, in the 19th Obſervation, was determined by the colours which it exhibited. And ſo the bigneſs of the parts of Natural Bodies may be conjectured by their colours, as ſhall be hereafter ſhewn. Alſo if two or more very thin plates be laid upon one another, ſo as to compoſe one plate equalling them all in thickneſs, the reſulting colour may be hereby determined. For inſtance, Mr. *Hook* obſerved, as is mentioned in his *Micrographia*, that a faint yellow plate of Muſcovy glaſs, laid upon a blue one, conſtituted a very deep Purple. The Yellow of the Firſt order is a faint one, and the thickneſs of the plate exhibiting it, according to the Table, is $4\frac{3}{5}$; to which add 9, the thickneſs exhibiting Blue of the Second order, and the ſum will be $13\frac{3}{5}$, which is the thickneſs exhibiting the Purple of the Third order.

Tranſmutation of the Colours

To explain, in the next place, the circumſtances of the 2d and 3d Obſervations; that is, how the rings of the colours may, by turning the priſms about their common axis the contrary way to that expreſſed in thoſe Obſervations, be converted into White and Black rings, and afterwards into rings of colours again, the colours of each ring lying now in an inverted order; it muſt be remembered, that thoſe rings of colours are dilated by the obliquation of the rays to the air which intercedes the glaſſes; and that according to the Table in the 7th Obſervation, their dilatation, or increaſe of their diameter, is moſt manifeſt and ſpeedy when they are obliqueſt. Now the rays of Yellow being more refracted by the firſt ſuperficies of the ſaid air than thoſe of Red, are thereby made more oblique to the ſecond ſuperficies, at which they

they are reflected to produce the coloured rings; and conſequently the Yellow circle in each ring will be more dilated than the Red; and the exceſs of its dilatation will be ſo much the greater, by how much the greater is the obliquity of the rays, until at laſt it become of equal extent with the Red of the ſame ring. And for the ſame reaſon the Green, Blue and Violet, will be alſo ſo much dilated by the ſtill greater obliquity of their rays, as to become all very nearly of equal extent with the Red, that is, equally diſtant from the center of the rings. And then all the colours of the ſame ring muſt be coincident, and by their mixture exhibit a White ring. And theſe White rings muſt have Black and Dark rings between them, becauſe they do not ſpread and interfere with one another as before. And for that reaſon alſo they muſt become diſtincter and viſible to far greater numbers. But yet the Violet, being obliqueſt, will be ſomething more dilated in proportion to its extent than the other colours, and ſo very apt to appear at the exterior verges of the White.

by motion of the Priſms.

Afterwards, by a greater obliquity of the rays, the Violet and Blue become more ſenſibly dilated than the Red and Yellow; and ſo being farther removed from the center of the rings, the colours muſt emerge out of the White in an order contrary to that which they had before; the Violet and Blue at the exterior limbs of each ring, and the Red and Yellow at the interior. And the Violet, by reaſon of the greateſt obliquity of its rays, being in proportion moſt of all expanded, will ſooneſt appear at the exterior limb of each White ring, and become more conſpicuous than the reſt. And the ſeveral ſeries of colours belonging to the ſeveral rings, will, by their unfolding and ſpreading, begin again to interfere, and thereby render the rings leſs diſtinct, and not viſible to ſo great numbers.

If inſtead of the priſms the object-glaſſes be made uſe of, the rings, which they exhibit, become not white and diſtinct by the Obliquity of the eye; by reaſon that the rays in their paſſage through that air, which intercedes the glaſſes, are very nearly parallel to thoſe lines in which they were firſt incident on the glaſſes; and conſequently the rays endued with ſeveral colours are not in-

clined one more than another to that air, as it happens in the priſms.

Of the confuſion of the Black and White rings, and the Colour on their edges produced by approaching the Eye.

There is yet another circumſtance of theſe experiments to be conſidered, and that is why the Black and White rings, which when viewed at a diſtance appear diſtinct, ſhould not only become confuſed by viewing them near at hand, but alſo yield a Violet colour at both the edges of every White ring. And the reaſon is, that the rays, which enter the eye at ſeveral parts of the pupil, have ſeveral obliquities to the glaſſes; and thoſe which are moſt oblique, if conſidered apart, would repreſent the rings bigger than thoſe which are the leaſt oblique. Whence the breadth of the perimeter of every White ring is expanded outwards by the obliqueſt rays, and inwards by the leaſt oblique. And this expanſion is ſo much the greater by how much the greater is the difference of the obliquity; that is, by how much the pupil is wider, or the eye nearer to the glaſſes. And the breadth of the Violet muſt be moſt expanded, becauſe the rays apt to excite a ſenſation of that colour are moſt oblique to a ſecond or farther ſuperficies of the thinned air, at which they are reflected, and have alſo the greateſt variation of obliquity; which makes that colour ſooneſt emerge out of the edges of the White. And as the breadth of every ring is thus augmented, the dark intervals muſt be diminiſhed, until the neighbouring rings become contiguous, and are blended; the exterior firſt, and then thoſe nearer the center; ſo that they can no longer be diſtinguiſhed apart, but ſeem to conſtitute an even and uniform Whiteneſs.

The effects of the Priſm in the 24th Obſ. explained.

Among all the obſervations there is none accompanied with ſo odd circumſtances as the twenty-fourth. Of thoſe the principal are, that in thin plates, which to the naked eye ſeem of an even and uniform tranſparent Whiteneſs, without any terminations of ſhadows, the refraction of a priſm ſhould make rings of colours appear, whereas it uſually makes objects appear coloured only there, where they are terminated with ſhadows, or have parts unequally luminous; and that it ſhould make thoſe rings exceedingly diſtinct and White, although it uſually renders objects confuſed and coloured. The cauſe of theſe things you will underſtand by conſidering, that all the rings of colours are really in the

the plate, when viewed with the naked eye; although, by reaſon of the great breadth of their circumferences, they ſo much interfere and are blended together, that they ſeem to conſtitute an uniform Whiteneſs. But when the rays paſs through the priſm to the eye, the orbits of the ſeveral colours in every ring are refracted, ſome more than others, according to their degrees of refrangibility: by which means the colours on one ſide of the ring, that is in the circumference on one ſide of its center, become more unfolded and dilated, and thoſe on the other ſide more complicated and contracted. And where by a due refraction they are ſo much contracted, that the ſeveral rings become narrower than to interfere with one another, they muſt appear diſtinct, and alſo white, if the conſtituent colours be ſo much contracted as to be wholly coincident. But, on the other ſide, where the orbit of every ring is made broader by the farther unfolding of its colours, it muſt interfere more with other rings than before, and ſo become leſs diſtinct.

To explain this a little farther, ſuppoſe the concentric circles AV and BX [in *fig.* 7.] repreſent the Red and Violet of any order, which, together with the intermediate colours, conſtitute any one of theſe rings. Now theſe being viewed through a priſm, the Violet circle BX will, by a greater refraction, be farther tranſlated from its place than the Red AV, and ſo approach nearer to it on that ſide of the circles, towards which the refractions are made. For inſtance, if the Red be tranſlated to *av*, the Violet may be tranſlated to *bx*, ſo as to approach nearer to it at *x* than before; and if the Red be farther tranſlated to av, the Violet may be ſo much farther tranſlated, to bx, as to convene with it at x; and if the Red be yet farther tranſlated to $\alpha\Upsilon$, the Violet may be ſtill ſo much farther tranſlated, to $\beta\xi$, as to paſs beyond it at ξ, and convene with it at *e* and *f*. And this being underſtood not only of the Red and Violet, but of all the other intermediate colours, and alſo of every revolution of thoſe colours, you will eaſily perceive how thoſe of the ſame revolution or order, by their nearneſs at *xv* and $\Upsilon\xi$, and their coincidence at xv, *e* and *f*, ought to conſtitute pretty diſtinct arcs of circles, eſpecially at xv, or at *e* and *f*: and that they will appear ſeverally at *xv*, and at xv exhibit

Effects of the Priſm in Obſ. 24. explained.

hibit Whiteneſs by their coincidence; and again appear ſeverally at ϒξ, but yet in a contrary order to that which they had before, and ſtill retain beyond *e* and *f*. But, on the other ſide, at *ab*, ab, or *αβ*, theſe colours muſt become much more confuſed, by being dilated and ſpread, ſo as to interfere with thoſe of other orders. And the ſame confuſion will happen at ϒξ, between *e* and *f*, if the refraction be very great, or the priſm very diſtant from the object-glaſſes: in which caſe no parts of the rings will be ſeen, ſave only two little arcs at *e* and *f*, whoſe diſtance from one another will be augmented by removing the priſm ſtill farther from the object-glaſſes: and theſe little arcs muſt be diſtincteſt and whiteſt at their middle; and at their ends, where they begin to grow confuſed they muſt be coloured. And the colours at one end of every arc muſt be in a contrary order to thoſe at the other end, by reaſon that they croſs in the intermediate White; namely, their ends, which verge towards ϒξ, will be Red and Yellow on that ſide next the center, and Blue and Violet on the other ſide. But their other ends, which verge from ϒξ, will on the contrary be Blue and Violet on that ſide towards the center, and on the other ſide Red and Yellow.

Now as all theſe things follow from the properties of light by a mathematical way of reaſoning, ſo the truth of them may be manifeſted by Experiments. For in a dark room, by viewing theſe rings through a priſm, by reflexion of the ſeveral Priſmatick Colours, which an aſſiſtant cauſes to move to and fro upon a wall or paper, from whence they are reflected, whilſt the ſpectator's eye, the priſm and the object-glaſſes (as in the 13th Obſervation) are placed ſteady: the poſition of the circles, made ſucceſſively by the ſeveral colours, will be found ſuch, in reſpect of one another, as I have deſcribed in the figures *abxv*, or abxv, or *αβξϒ*. And by the ſame method the truth of the explications of other Obſervations may be examined.

Cauſe of the Colours ſeen in ſmall Fragments

By what hath been ſaid, the like phænomena of water, and thin plates of glaſs may be underſtood. But in ſmall fragments of thoſe plates, there is this farther obſervable; that where they lie flat upon a table, and are turned about their centers whilſt they are viewed through a priſm, they will in ſome poſtures exhibit

exhibit waves of various colours, and ſome of them exhibit theſe waves in one or two poſitions only, but the moſt of them do in all poſitions exhibit them, and make them for the moſt part appear almoſt all over the plates. The reaſon is, that the ſuperficies of ſuch plates are not even, but have many cavities and ſwellings, which how ſhallow ſoever do a little vary the thickneſs of the plate. For at the ſeveral ſides of thoſe cavities, for the reaſons newly deſcribed, there ought to be produced waves in ſeveral poſtures of the priſm. Now though it be but ſome very ſmall and narrower parts of the glaſs, by which theſe waves for the moſt part are cauſed, yet they may ſeem to extend themſelves over the whole glaſs: becauſe from the narroweſt of thoſe parts, there are colours of ſeveral orders, that is of ſeveral rings, confuſedly reflected; which, by refraction of the priſm, are unfolded, ſeparated, and according to their degrees of refraction, diſperſed to ſeveral places, ſo as to conſtitute ſo many ſeveral waves, as there were divers orders of colours promiſcuouſly reflected from that part of the glaſs.

of Glaſs-plates viewed through a Priſm.

Theſe are the principal phænomena of thin plates or bubbles, whoſe explications depend on the properties of light, which I have heretofore delivered. And theſe you ſee do neceſſarily follow from them, and agree with them, even to their very leaſt circumſtances; and not only ſo, but do very much tend to their proof. Thus by the 24th Obſervation it appears, that the rays of ſeveral colours, made as well by thin plates or bubbles, as by refractions of a priſm, have ſeveral degrees of refrangibility; whereby thoſe of each order, which at the reflexion from the plate or bubble are intermixed with thoſe of other orders, are ſeparated from them by refraction, and aſſociated together ſo as to become viſible by themſelves like arcs of circles. For if the rays were all alike refrangible, it is impoſſible that the Whiteneſs, which to the naked ſenſe appears uniform, ſhould, by refraction, have its parts tranſpoſed and ranged into thoſe Black and White arcs.

It appears alſo that the unequal refractions of difform rays proceed not from any contingent irregularities; ſuch as are veins, an uneven poliſh, or fortuitous poſition of the pores of glaſs; unequal

The Colorific diſpoſition of the rays innate and immutable.

The Colorific Diſpoſition of

unequal and caſual motions in the air or æther; the ſpreading, breaking, or dividing the ſame ray into many diverging parts, or the like. For, admitting any ſuch irregularities, it would be impoſſible for refractions to render thoſe rings ſo very diſtinct, and well defined, as they do in the 24th Obſervation. It is neceſſary therefore that every ray have its proper and conſtant degree of refrangibility connate with it, according to which its refraction is ever juſtly and regularly performed, and that ſeveral rays have ſeveral of thoſe degrees.

And what is ſaid of their refrangibility may be alſo underſtood of their reflexibility, that is of their diſpoſitions to be reflected ſome at a greater, and others at a leſs thickneſs, of thin plates or bubbles; namely, that thoſe diſpoſitions are alſo connate with the rays, and immutable; as may appear by the 13th, 14th and 15th Obſervations compared with the 4th and 18th.

By the precedent Obſervations it appears alſo, that Whiteneſs is a diſſimilar mixture of all colours, and that light is a mixture of rays endued with all thoſe colours. For conſidering the multitude of the rings of colours, in the 3d, 12th and 24th Obſervations, it is manifeſt, that although in the 4th and 18th Obſervations there appear no more than eight or nine of thoſe rings, yet there are really a far greater number, which ſo much interfere and mingle with one another, as after thoſe eight or nine revolutions to dilute one another wholly, and conſtitute an even and ſenſibly uniform Whiteneſs. And conſequently that Whiteneſs muſt be allowed a mixture of all colours, and the light, which conveys it to the eye, muſt be a mixture of rays endued with all thoſe colours.

But farther, by the 24th Obſervation, it appears that there is a conſtant relation between colours and refrangibility; the moſt refrangible rays being Violet, the leaſt refrangible Red, and thoſe of intermediate colours having proportionably intermediate degrees of refrangibility. And by the 13th, 14th and 15th Obſervations, compared with the 4th or 18th, there appears to be the ſame conſtant relation between colour and reflexibility, the Violet being in like circumſtances reflected at leaſt thickneſſes of any thin plate or bubble, the Red at greateſt thickneſſes, and the in-

termediate colours at intermediate thickneſſes. Whence it follows, that the colorifick diſpoſitions of rays are alſo connate with them and immutable; and by conſequence that all the productions and appearances of colours in the world are derived not from any Phyſical change cauſed in light by refraction or reflexion, but only from the various mixtures or ſeparations of rays, by virtue of their different refrangibility or reflexibility. And in this reſpect the ſcience of colours becomes a ſpeculation as truly mathematical, as any other part of Opticks. I mean ſo far as they depend on the nature of light, and are not produced or altered by the power of imagination, or by ſtriking or preſſing the eye.

the Rays of Light innate and immutable.

THE

SECOND BOOK

OF

OPTICKS.

PART III.

Of the Permanent Colours of Natural Bodies, and the analogy between them and the colours of thin tranſparent plates.

I AM now come to another part of this deſign; which is to conſider how the phænomena of thin tranſparent plates ſtand related to thoſe of all other Natural Bodies. Of theſe bodies I have already told you, that they appear of divers colours, accordingly as they are diſpoſed to reflect moſt copiouſly the rays originally endued with thoſe colours. But their conſtitutions, whereby they reflect ſome rays more copiouſly than others, remain to be diſcovered, and theſe I ſhall endeavour to manifeſt in the following Propoſitions.

PROP. I.

Thoſe ſuperficies of tranſparent bodies Reflect the greateſt quantity of light, which have the greateſt Refracting Power; that is, which intercede mediums that differ moſt in their Refractive Denſities. And in the confines of equally refracting mediums there is no reflexion.

Analogy between

The analogy between reflexion and refraction will appear by conſidering, that when light paſſeth obliquely out of one medium

Reflexion and Refraction.

um into another, which refracts from the perpendicular, the greater is the difference of their Refractive Denſity, the leſs obliquity of incidence is requiſite to cauſe a total reflexion. For as the ſines are which meaſure the refraction, ſo is the ſine of incidence at which the total reflexion begins, to the radius of the circle; and conſequently that angle of incidence is leaſt, where there is the greateſt difference of the ſines. Thus in the paſſing of light out of water into air, where the refraction is meaſured by the ratio of the ſines 3 to 4, the total reflexion begins when the angle of incidence is about 48 degrees 35 minutes. In paſſing out of glaſs into air, where the refraction is meaſured by the ratio of the ſines 20 to 31, the total reflexion begins when the angle of incidence is 40 degrees 10 minutes; and ſo in paſſing out of cryſtal, or more ſtrongly refracting mediums into air, there is ſtill a leſs obliquity requiſite to cauſe a total reflexion. Superficies therefore which Refract moſt do ſooneſt Reflect all the light which is incident on them, and ſo muſt be allowed moſt ſtrongly reflexive.

But the truth of this Propoſition will farther appear by obſerving, that in the ſuperficies interceding two tranſparent mediums (ſuch as are air, water, oil, common glaſs, cryſtal, metalline glaſſes, iſland glaſſes, white tranſparent arſenick, diamonds, &c.) the reflexion is ſtronger or weaker accordingly, as the ſuperficies hath a greater or leſs Refracting power. For in the confine of air and ſal-gem it is ſtronger than in the confine of air and water; and ſtill ſtronger in the confine of air and common glaſs or cryſtal; and ſtronger in the confine of air and a diamond. If any of theſe, and ſuch like tranſparent ſolids, be immerged in water, its reflexion becomes much weaker than before; and ſtill weaker, if they be immerged in the more ſtrongly Refracting liquors of well rectified oil of vitriol or ſpirit of turpentine. If water be diſtinguiſhed into two parts, by any imaginary ſurface, the reflexion in the confine of thoſe two parts, is none at all. In the confine of water and ice it is very little; in that of water and oil it is ſomething greater; in that of water and ſal-gem ſtill greater; and in that of water and glaſs, or cryſtal, or other denſer ſubſtances ſtill greater, accordingly as thoſe mediums differ more or leſs in their Refracting powers. Hence in the confine of com-

mon glaſs and cryſtal, there ought to be a weak reflexion; and a ſtronger reflexion in the confine of common and metalline glaſs, though I have not yet tried this. But, in the confine of two glaſſes of equal denſity, there is not any ſenſible reflexion, as was ſhewn in the firſt Obſervation. And the ſame may be underſtood of the ſuperficies interceding two cryſtals, or two liquors, or any other ſubſtances in which no refraction is cauſed. So then the reaſon why uniform pellucid mediums, ſuch as water, glaſs or cryſtal, have no ſenſible reflexion but in their external ſuperficies, where they are adjacent to other mediums of a different denſity, is becauſe all their contiguous parts have one and the ſame degree of denſity.

PROP. II.

The leaſt parts of almoſt all Natural Bodies are in ſome meaſure tranſparent: and the Opacity of thoſe bodies ariſeth from the multitude of reflexions cauſed in their internal parts.

Cauſe of Opacity

That this is ſo has been obſerved by others, and will eaſily be granted by them that have been converſant with microſcopes. And it may be alſo tried by applying any ſubſtance to a hole, through which ſome light is immitted into a dark room. For how Opake ſoever that ſubſtance may ſeem in the open air, it will by that means appear very manifeſtly tranſparent, if it be of a ſufficient thinneſs. Only White Metalline bodies muſt be excepted, which, by reaſon of their exceſſive denſity, ſeem to reflect almoſt all the light incident on their firſt ſuperficies, unleſs by ſolution in menſtruums they be reduced into very ſmall particles, and then they become tranſparent.

PROP.

PROP. III.

Between the parts of Opake and Coloured bodies are many ſpaces, either empty, or repleniſhed with mediums of other denſities; as water between the tinging corpuſcles wherewith any liquor is impregnated; air between the aqueous globules that conſtitute clouds or miſts; and for the moſt part ſpaces void of both air and water, but yet perhaps not wholly void of all ſubſtance, between the parts of Hard Bodies.

The truth of this is evinced by the two precedent Propoſitions: for by the ſecond Propoſition there are many reflexions made by the internal parts of bodies, which, by the firſt Propoſition, would not happen if the parts of thoſe bodies were continued without any ſuch interſtices between them, becauſe reflexions are cauſed only in ſuperficies, which intercede mediums of a differing denſity by Prop. I.

Porouſneſs a principal cauſe of Opacity.

But farther, that this diſcontinuity of parts is the principal cauſe of the Opacity of bodies, will appear by conſidering, that Opake ſubſtances become Tranſparent, by filling their pores with any ſubſtance of equal, or almoſt equal, denſity with their parts. Thus paper dipped in water or oil, the *Oculus Mundi* ſtone ſteeped in water, linen cloth oiled or varniſhed, and many other ſubſtances ſoaked in ſuch liquors as will intimately pervade their little pores, become by that means more tranſparent than otherwiſe; ſo, on the contrary, the moſt Tranſparent ſubſtances may, by evacuating their pores, or ſeparating their parts, be rendered ſufficiently Opake; as ſalts or wet paper, or the *Oculus Mundi* ſtone, by being dried; horn, by being ſcraped; glaſs, by being reduced to powder, or otherwiſe flawed; turpentine, by being ſtirred about with water till they mix imperfectly; and water, by being formed into many ſmall bubbles, either alone in the form of froth, or by ſhaking it together with oil of turpentine, or oil olive, or with ſome other convenient liquor, with which it will not perfectly incorporate. And to the increaſe of the Opacity of theſe bodies it conduces ſomething, that, by the 23d Obſervation, the reflexions

of

of very thin tranſparent ſubſtances are conſiderably ſtronger than thoſe made by the ſame ſubſtances of a greater thickneſs.

PROP. IV.

The Parts of bodies and their interſtices muſt not be leſs than of ſome definite bigneſs, to render them Opake and Coloured.

A definite ſize of Particle and Pore requiſite to Opacity and Colour.

For the Opakeſt bodies, if their parts be ſubtilly divided (as metals by being diſſolved in acid menſtruums, &c.) become perfectly Tranſparent. And you may alſo remember, that in the eighth Obſervation there was no ſenſible reflexion at the ſuperficies of the object-glaſſes, where they were very near one another, though they did not abſolutely touch. And in the 17th Obſervation the reflexion of the water-bubble where it became thinneſt was almoſt inſenſible, ſo as to cauſe very Black ſpots to appear on the top of the bubble by the want of reflected light.

On theſe grounds I perceive it is, that water, ſalt, glaſs, ſtones, and ſuch like ſubſtances, are tranſparent. For, upon divers conſiderations, they ſeem to be as full of pores, or interſtices between their parts, as other bodies are; but yet their parts and interſtices to be too ſmall to cauſe reflexions in their common ſurfaces.

PROP. V.

The Tranſparent Parts of bodies according to their ſeveral ſizes reflect rays of one colour, and tranſmit thoſe of another, on the ſame grounds that thin plates or bubbles do reflect or tranſmit thoſe rays. And this I take to be the ground of all their colours.

The tranſparent Parts of bodies reflect or tranſmit

For if a thinned or plated body, which being of an even thickneſs, appears all over of one uniform colour, ſhould be ſlit into threads, or broken into fragments, of the ſame thickneſs with the plate; I ſee no reaſon why every thread or fragment ſhould not keep its colour, and by conſequence why a heap of thoſe threads or fragments ſhould not conſtitute a maſs or powder of the ſame colour, which the plate exhibited before it was broken. And the parts of all Natural Bodies being like ſo many

fragments

fragments of a plate, muſt on the ſame grounds exhibit the ſame colours.

the ſeveral Colorific rays according to their ſizes.

Now that they do ſo, will appear by the affinity of their properties. The finely coloured feathers of ſome birds, and particularly thoſe of peacocks tails, do in the very ſame part of the feather appear of ſeveral colours in ſeveral poſitions of the eye, after the very ſame manner that thin plates were found to do in the 7th and 19th Obſervations, and therefore their colours ariſe from the thinneſs of the tranſparent parts of the feathers; that is, from the ſlenderneſs of the very fine hairs, or *Capillamenta*, which grow out of the ſides of the groſſer lateral branches or fibres of thoſe feathers. And to the ſame purpoſe it is, that the webs of ſome ſpiders, by being ſpun very fine, have appeared coloured, as ſome have obſerved, and that the coloured fibres of ſome ſilks, by varying the poſition of the eye, do vary their colour. Alſo the colours of ſilks, cloths, and other ſubſtances, which water or oil can intimately penetrate, become more faint and obſcure by being immerged in thoſe liquors, and recover their vigour again by being dried; much after the manner declared of thin bodies in the 10th and 21ſt Obſervations. Leaf-gold, ſome ſorts of painted glaſs, the infuſion of *Lignum Nephriticum*, and ſome other ſubſtances reflect one colour, and tranſmit another, like thin bodies in the 9th and 20th Obſervations. And ſome of thoſe coloured powders which painters uſe, may have their colours a little changed, by being very elaborately and finely ground. Where I ſee not what can be juſtly pretended for thoſe changes, beſides the breaking of their parts into leſs parts, by that contrition, after the ſame manner that the colour of a thin plate is changed, by varying its thickneſs. For which reaſon alſo it is that the coloured flowers of plants and vegetables by being bruiſed uſually become more tranſparent than before; or at leaſt in ſome degree or other change their colours. Nor is it much leſs to my purpoſe, that by mixing divers liquors very odd and remarkable productions and changes of colours may be effected; of which no cauſe can be more obvious and rational, than that the ſaline corpuſcles of one liquor do variouſly act upon, or unite with, the tinging corpuſcles of another, ſo as to make them ſwell or ſhrink;

whereby

whereby not only their bulk but their denſity alſo may be changed; or to divide them into ſmaller corpuſcles, whereby a coloured liquor may become tranſparent; or to make many of them aſſociate into one cluſter, whereby two tranſparent liquors may compoſe a coloured one. For we ſee how apt thoſe ſaline menſtruums are to penetrate and diſſolve ſubſtances to which they are applied, and ſome of them to precipitate what others diſſolve. In like manner, if we conſider the various phænomena of the atmoſphere, we may obſerve, that when varpours are firſt raiſed, they hinder not the tranſparency of the air, being divided into parts too ſmall to cauſe any reflexion in their ſuperficies. But when in order to compoſe drops of rain they begin to coaleſce and conſtitute globules of all intermediate ſizes, thoſe globules, when they become of a convenient ſize to reflect ſome colours and tranſmit others, may conſtitute clouds of various colours according to their ſizes. And I ſee not what can be rationally conceived in ſo tranſparent a ſubſtance as water for the production of theſe colours, beſides the various ſizes of its fluid and globular parcels.

PROP. VI.

The Parts of bodies on which their colours depend, are denſer than the medium, which pervades their interſtices.

Colorific parts of Bodies denſer than the Medium which pervades their Pores.

This will appear by conſidering, that the colour of a body depends not only on the rays which are incident perpendicularly on its parts, but on thoſe alſo which are incident at all other angles. And that according to the 7th Obſervation, a very little variation of Obliquity will change the reflected colour, where the thin body, or ſmall particle is rarer than the ambient medium; inſomuch that ſuch a ſmall particle is, at diverſly oblique incidences, reflect of all ſorts of colours, in ſo great a variety that the colour reſulting from them all, confuſedly reflected from a heap of ſuch particles, muſt rather be a White or Grey than any other colour, or at beſt it muſt be but a very inperfect and dirty colour. Whereas if the thin body, or ſmall particle, be much denſer than the ambient medium, the colours according to the 19th Obſervation are ſo little changed by the variation of Obliquity, that the

rays, which are reflected leaſt obliquely, may predominate over the reſt, ſo much as to cauſe a heap of ſuch particles to appear very intenſely of their colour.

It conduces ſomething alſo to the confirmation of this Propoſition, that, according to the 22d Obſervation, the colours exhibited by the denſer thin body, within the rarer, are more briſk than thoſe exhibited by the rarer within the denſer.

PROP. VII.

The Bigneſs of the component Parts of Natural bodies may be conjectured by their colours.

The Colours of Bodies indicate the ſizes of their component particles.

For ſince the Parts of theſe bodies by Prop. v. do moſt probably exhibit the ſame colours with a plate of equal thickneſs, provided they have the ſame Refractive Denſity; and ſince their Parts ſeem for the moſt part to have much the ſame denſity with water or glaſs, as by many circumſtances is obvious to collect; to determine the ſizes of thoſe Parts, you need only have recourſe to the precedent Tables, in which the thickneſs of water or glaſs exhibiting any colour is expreſſed. Thus if it be deſired to know the diameter of a corpuſcle, which being of equal denſity with glaſs, ſhall reflect Green of the Third order; the number $16\frac{1}{4}$ ſhews it to be $\frac{16\frac{1}{4}}{1000000}$ parts of an inch.

The greateſt difficulty is here to know of what Order the colour of any body is. And for this end we muſt have recourſe to the 4th and 18th Obſervations, from whence may be collected theſe particulars.

Scarlets, and other *Reds*, *Oranges* and *Yellows*, if they be pure and intenſe, are moſt probably of the Second order. Thoſe of the Firſt and Third order alſo may be pretty good, only the Yellow of the firſt order is faint, and the Orange and Red of the Third order have a great mixture of Violet and Blue.

There may be good *Greens* of the Fourth order, but the pureſt are of the Third. And of this order the Green of all vegetables ſeem to be, partly by reaſon of the intenſeneſs of their colours, and partly becauſe when they wither, ſome of them turn to a greeniſh Yellow, and others to a more perfect Yellow or Orange,

The Colours of Bodies indicate

or perhaps to Red, paſſing firſt through all the aforeſaid intermediate colours. Which changes ſeem to be effected by the exhaling of the moiſture; which may leave the tinging corpuſcles more denſe, and ſomething augmented by the accretion of the oily and earthy part of that moiſture. Now the Green without doubt is of the ſame order, with thoſe colours into which it changeth; becauſe the changes are gradual, and thoſe colours, though uſually not very full, yet are often too full and lively to be of the Fourth order.

Blues and *Purples* may be either of the Second or Third order, but the beſt are of the Third. Thus the colour of Violets ſeems to be of that order, becauſe their ſyrup by Acid liquors turns Red, and by Urinous and Alcalizate turns Green. For ſince it is of the nature of Acids to diſſolve or attenuate, and of alcalies to precipitate or incraſſate; if the Purple colour of the ſyrup was of the Second order, an Acid liquor, by attenuating its tinging corpuſcles, would change it to a Red of the Firſt order, and an alcali by incraſſating them would change it to a Green of the ſecond order; which Red and Green, eſpecially the Green, ſeem too imperfect to be the colours produced by theſe changes. But if the ſaid Purple be ſuppoſed of the Third order, its change to Red of the Second, and Green of the Third, may without any inconvenience be allowed.

If there be found any body of a deeper and leſs reddiſh Purple than that of the Violets, its colour moſt probably is if the Second order. But yet there being no body, commonly known, whoſe colour is conſtantly more deep than theirs; I have made uſe of their name to denote the deepeſt and leaſt reddiſh Purples, ſuch as manifeſtly tranſcend their colour in purity.

The *Blue* of the Firſt order, though very faint and little, may poſſibly be the colour of ſome ſubſtances; and particularly the Azure colour of the ſkies ſeems to be of this order. For all vapours, when they begin to condenſe and coaleſce into ſmall parcels, become firſt of that bigneſs, whereby ſuch an Azure muſt be reflected, before they can conſtitute clouds of other colours. And ſo this being the firſt colour which vapours begin to reflect, it ought to be the colour of the fineſt and moſt tranſparent ſkies,

in

in which vapours are not arrived to that groſſneſs requiſite to reflect other colours, as we find it is by experience. the ſizes of their component particles.

Whiteneſs, if moſt intenſe and luminous, is that of the Firſt order, if leſs ſtrong and luminous a mixture of the colours of ſeveral orders. Of this laſt kind is the whiteneſs of froth, paper, linen, and moſt white ſubſtances; of the former I reckon that of white metals to be. For whilſt the denſeſt of metals, gold, if foliated, is tranſparent, and all metals become tranſparent, if diſſolved in menſtruums or vitrified; the opacity of white metals ariſeth not from their denſity alone. They being leſs denſe than gold would be more tranſparent than it, did not ſome other cauſe concur with their denſity to make them Opake. And this cauſe I take to be ſuch a bigneſs of their particles, as fits them to reflect the White of the Firſt order. For if they be of other thickneſſes, they may reflect other colours; as is manifeſt by the colours which appear upon hot ſteel in tempering it, and ſometimes upon the ſurface of melted metals, in the ſkin or ſcoria which ariſes upon them in their cooling. And as the White of the Firſt order is the ſtrongeſt which can be made by plates of tranſparent ſubſtances, ſo it ought to be ſtronger in the denſer ſubſtances of metals, than in the rarer of air, water, and glaſs. Nor do I ſee but that Metallic ſubſtances, of ſuch a thickneſs as may fit them to reflect the White of the Firſt order, may, by reaſon of their great denſity, according to the tenour of the firſt of theſe Propoſitions, reflect all the light incident upon them, and ſo be as opake and ſplendent as it is poſſible for any body to be. Gold, or Copper mixed with leſs than half their weight of ſilver, or tin, or regulus of antimony, in fuſion, or amalgamed with a very little mercury, become white; whieh ſhews both that the particles of white metals have much more ſuperficies, and ſo are ſmaller, than thoſe of gold and copper, and alſo that they are ſo opake, as not to ſuffer the particles of gold or copper to ſhine through them. Now it is ſcarce to be doubted, but that the colours of gold and copper are of the Second and Third order; and therefore the particles of white metals cannot be much bigger, than is requiſite to make them reflect the White of the Firſt order. The volatility of Mercury argues, that they are not much

The Colours of Bodies indicate

bigger; nor may they be much leſs, leſt they loſe their Opacity; and become either tranſparent, as they do when attenuated by vitrification, or by ſolution in Menſtruums; or Black, as they do when ground ſmaller, by rubbing ſilver, or tin, or lead, upon other ſubſtances to draw Black lines. The firſt and only colour which white metals take by grinding their particles ſmaller, is Black; and therefore their White ought to be that which borders upon the Black ſpot in the center of the rings of colours, that is, the White of the Firſt order. But if you would hence gather the bigneſs of Metallic particles, you muſt allow for their denſity. For were Mercury tranſparent, its denſity is ſuch that the ſine of incidence upon it (by my computation) would be to the ſine of its refraction, as 71 to 20, or 7 to 2. And therefore the thickneſs of its particles, that they may exhibit the ſame colours with thoſe of bubbles of water, ought to be leſs than the thickneſs of the ſkin of thoſe bubbles, in the proportion of 2 to 7. Whence it is poſſible that the particles of Mercury may be as little as the particles of ſome tranſparent and volatile fluids, and yet reflect the White of the Firſt order.

Laſtly, for the production of *Black*, the corpuſcles muſt be leſs than any of thoſe which exhibit colours. For at all greater ſizes there is too much light reflected to conſtitute this colour. But if they be ſuppoſed a little leſs than is requiſite to reflect the White and very faint Blue of the Firſt order, they will, according to the 4th, 8th, 17th and 18th Obſervations, reflect ſo very little light as to appear intenſely Black; and yet may perhaps variouſly refract it to and fro within themſelves ſo long, until it happen to be ſtifled and loſt; by which means they will appear Black, in all poſitions of the eye, without any tranſparency. And from hence may be underſtood why fire, and the more ſubtle diſſolver putrefaction, by dividing the particles of ſubſtances, turn them to Black; why ſmall quantities of Black ſubſtances impart their colour very freely and intenſely to other ſubſtances, to which they are applied; the minute particles of theſe, by reaſon of their very great number, eaſily overſpreading the groſs particles of others; why glaſs ground very elaborately with ſand on a copper plate, till it be well poliſhed, makes the ſand, toge-

ther with what is worn off from the glaſs and copper, become very Black: why Black ſubſtances do ſooneſt of all others become hot in the ſun's light and burn (which effect may proceed partly from the multitude of refractions in a little room, and partly from the eaſy commotion of ſo very ſmall corpuſcles); and why Blacks are uſually a little inclined to a blueiſh colour: for that they are ſo may be ſeen, by illuminating white paper by light reflected from Black ſubſtances. For the paper will uſually appear of a blueiſh White; and the reaſon is, that Black borders on the obſcure Blue of the Firſt order deſcribed in the 18th Obſervation, and therefore reflects more rays of that colour, than of any other.

the fizes of their component particles.

In theſe deſcriptions I have been the more particular, becauſe it is not impoſſible but that Microſcopes may at length be improved to the diſcovery of the particles of bodies on which their colours depend, if they are not already in ſome meaſure arrived to that degree of perfection. For if thoſe inſtruments are or can be ſo far improved, as with ſufficient diſtinctneſs to repreſent objects five or ſix hundred times bigger, than at a foot diſtance they appear to our naked eyes; I ſhould hope that we might be able to diſcover ſome of the greateſt of thoſe corpuſcles. And by one that would magnify three or four thouſand times, perhaps they might all be diſcovered, but thoſe which produce Blackneſs. In the mean while I ſee nothing material in this diſcourſe that may rationally be doubted of, excepting this poſition: That tranſparent corpuſcles of the ſame thickneſs and denſity with a plate, do exhibit the ſame colour. And this I would have underſtood not without ſome latitude; as well becauſe thoſe corpuſcles may be of irregular figures, and many rays muſt be obliquely incident on them, and ſo have a ſhorter way through them than the length of their diameters; as becauſe the ſtraitneſs of the medium, pent in on all ſides within ſuch corpuſcles, may a little alter its motions, or other qualities, on which the reflexion depends. But yet I cannot much ſuſpect the laſt; becauſe I have obſerved of ſome ſmall plates of Muſcovy-glaſs, which were of an even thickneſs, that through a microſcope they have appeared of the ſame colour at their edges and corners, where the included medium

Utmoſt probable improvement of Microſcopes.

dium was terminated, which they appeared of in other places. However it will add much to our ſatisfaction, if thoſe corpuſcles can be diſcovered with microſcopes; which, if we ſhall at length attain to, I fear it will be the utmoſt improvement of this ſenſe. For it ſeems impoſſible to ſee the more ſecret and noble works of Nature within the corpuſcles, by reaſon of their tranſparency.

PROP. VIII.

The cauſe of reflexion is not the impinging of light on the ſolid or impervious Parts of bodies, as is commonly believed.

The impact of light againſt the ſolid particles

This will appear by the following conſiderations. Firſt, That in the paſſage of light out of glaſs into air there is a reflexion as ſtrong, as in its paſſage out of air into glaſs, or rather a little ſtronger; and by many degrees ſtronger, than in its paſſage out of glaſs into water. And it ſeems not probable, that air ſhould have more ſtrongly reflecting parts than water or glaſs. But if that ſhould poſſibly be ſuppoſed, yet it will avail nothing; for the reflexion is as ſtrong or ſtronger when the air is drawn away from the glaſs, ſuppoſe in the air-pump invented by *Otto Gueric* and improved and made uſeful by Mr. *Boyle*, as when it is adjacent to it. Secondly, If light in its paſſage out of glaſs into air be incident more obliquely, than at an angle of 40 or 41 degrees, it is wholly reflected; if leſs obliquely, it is in great meaſure tranſmitted. Now it is not to be imagined, that light, at one degree of obliquity, ſhould meet with pores enough in the air to tranſmit the greater part of it; and at another degree of Obliquity, ſhould meet with nothing but parts to reflect it wholly; eſpecially conſidering that in its paſſage out of air into glaſs, how oblique ſoever be its incidence, it finds pores enough in the glaſs to tranſmit a great part of it. If any man ſuppoſe that it is not reflected by the air, but by the outmoſt ſuperficial parts of the glaſs, there is ſtill the ſame difficulty: beſides, that ſuch a ſuppoſition is unintelligible, and will alſo appear to be falſe by applying water behind ſome part of the glaſs inſtead of air. For ſo in a convenient obliquity of the rays, ſuppoſe of 45 or 46 degrees, at which they are all reflected where the air is adjacent

to

to the glaſs; they ſhall be in great meaſure tranſmitted, where the water is adjacent to it: which argues, that their reflexion or tranſmiſſion depends on the conſtitution of the air and water behind the glaſs, and not on the ſtriking of the rays upon the parts of the glaſs. Thirdly, If the colours made by a priſm, placed at the entrance of a beam of light into a darkened room, be ſucceſſively caſt on a ſecond priſm placed at a greater diſtance from the former, in ſuch manner that they are all alike incident upon it; the ſecond priſm may be ſo inclined to the incident rays, that thoſe which are of a Blue colour ſhall be all reflected by it, and yet thoſe of a Red colour pretty copiouſly tranſmitted. Now if the reflexion be cauſed by the parts of air or glaſs, I would aſk, why, at the ſame obliquity of incidence, the Blue ſhould wholly impinge on thoſe parts ſo as to be all reflected, and yet the Red find pores enough to be in a great meaſure tranſmitted. Fourthly, Where two glaſſes touch one another, there is no ſenſible reflexion, as was declared in the firſt Obſervation; and yet I ſee no reaſon, why the rays ſhould not impinge on the parts of glaſs as much when contiguous to other glaſs, as when contiguous to air. Fifthly, When the top of a water-bubble, in the 17th Obſervation, by the continual ſubſiding and exhaling of the water grew very thin, there was ſuch a little and almoſt inſenſible quantity of light reflected from it, that it appeared intenſely Black; whereas round about that Black ſpot, where the water was thicker, the reflexion was ſo ſtrong as to make the water ſeem very White. Nor is it only at the leaſt thickneſs of thin plates or bubbles, that there is no manifeſt reflexion, but at many other thickneſſes continually greater and greater. For in the 15th Obſervation the rays of the ſame colour were by turns tranſmitted at one thickneſs, and reflected at another thickneſs, for an indeterminate number of ſucceſſions. And yet in the ſuperficies of the thinned body, where it is of one thickneſs, there are as many parts for the rays to impinge on, as where it is of any other thickneſs. Sixthly, If reflexion were cauſed by the parts of reflecting bodies, it would be impoſſible for thin plates or bubbles at one and the ſame place to reflect the rays of one colour, and tranſmit thoſe of another; as they do, according to the 13th and 15th Obſervations.

of bodies not the cauſe of reflexion.

The impact of Light againſt the ſolid particles

vations. For it is not to be imagined, that at one place the rays, which for inſtance exhibit a Blue colour, ſhould have the fortune to daſh upon the Parts, and thoſe which exhibit a Red to hit upon the Pores of the body; and then at another place, where the body is either a little thicker, or a little thinner, that on the contrary the Blue ſhould hit upon its Pores, and the Red upon its Parts. Laſtly, were the rays of light reflected by impinging on the ſolid parts of bodies, their reflexions from poliſhed bodies could not be ſo regular as they are. For in poliſhing glaſs with ſand, putty or tripoly, it is not to be imagined, that thoſe ſubſtances can, by grating and fretting the glaſs, bring all its leaſt particles to an accurate poliſh; ſo that all their ſurfaces ſhall be truly plain or truly ſpherical, and look all the ſame way, ſo as together to compoſe one even ſurface. The ſmaller the particles of thoſe ſubſtances are, the ſmaller will be the ſcratches, by which they continually fret and wear away the glaſs until it be poliſhed; but, be they never ſo ſmall, they can wear away the glaſs no otherwiſe, than by grating and ſcratching it, and breaking the protuberances; and therefore poliſh it no otherwiſe, than by bringing its roughneſs to a very fine grain, ſo that the ſcratches and frettings of the ſurface become too ſmall to be viſible. And therefore if light were reflected by impinging upon the ſolid Parts of the glaſs, it would be ſcattered as much by the moſt poliſhed glaſs as by the rougheſt. So then it remains a Problem, how glaſs, poliſhed by fretting ſubſtances, can reflect light ſo regularly as it does. And this Problem is ſcarce otherwiſe to be ſolved, than by ſaying, that the reflexion of a ray is effected, not by a ſingle point of the reflecting body, but by ſome Power of the body which is evenly diffuſed all over its ſurface, and by which it acts upon the ray without immediate contact. For that the Parts of bodies do act upon light at a diſtance ſhall be ſhewn hereafter.

Now if light be reflected not by impinging on the ſolid Parts of bodies, but by ſome other principle; it is probable that as many of its rays as impinge on the ſolid Parts of bodies are not reflected, but ſtifled and loſt in the bodies. For otherwiſe we muſt allow two ſorts of reflexions. Should all the rays be re-

flected

flected which impinge on the internal parts of clear water or cryſtal, thoſe ſubſtances would rather have a cloudy colour than a clear tranſparency. To make bodies look Black, it is neceſſary that many rays be ſtopped, retained and loſt in them; and it ſeems not probable, that any rays can be ſtopped and ſtifled in them, which do not impinge on their Parts.

of bodies not the cauſe of Reflexion.

And hence we may underſtand that bodies are much more rare and porous, than is commonly believed. Water is nineteen times lighter, and by conſequence nineteen times rarer, than gold; and gold is ſo rare as very readily, and without the leaſt oppoſition, to tranſmit the Magnetic effluvia, and eaſily to admit quick-ſilver into its pores, and to let water paſs through it. For a concave ſphere of gold filled with water, and ſodered up, has, upon preſſing the ſphere with great force, let the water ſqueeze through it, and ſtand all over its outſide in multitudes of ſmall drops, like dew, without burſting or cracking the body of the gold; as I have been informed by an eye-witneſs. From all which we may conclude, that gold has more pores than ſolid parts, and by conſequence that water has above forty times more Pores than Parts. And he that ſhall find out an hypotheſis, by which water may be ſo rare, and yet not be capable of compreſſion by force; may doubtleſs, by the ſame hypotheſis, make gold and water, and all other bodies, as much rarer as he pleaſes, ſo that light may find a ready paſſage through tranſparent ſubſtances.

Poroſireſs of Bodies.

The magnet acts upon iron through all denſe bodies not magnetic nor red hot, without any diminution of its virtue; as for inſtance through gold, ſilver, lead, glaſs, water. The Gravitating power of the ſun is tranſmitted through the vaſt bodies of the planets without any diminution; ſo as to act upon all their parts to their very centers, with the ſame force, and according to the ſame laws, as if the part, upon which it acts, were not ſurrounded with the body of the planet. The rays of light, whether they be very ſmall bodies projected, or only motion or force propagated, are moved in right lines; and whenever a ray of light is, by any obſtacle, turned out of its rectilinear way, it will never return into the ſame rectilinear way, unleſs perhaps by very great accident. And yet light is tranſmitted through pellucid ſolid bo-

Porouſneſs of Bodies.

dies in right lines to very great diſtances. How bodies can have a ſufficient quantity of pores for producing theſe effects, is very difficult to conceive; but perhaps not altogether impoſſible. For the colours of bodies ariſe from the magnitudes of the particles which reflect them, as was explained above. Now if we conceive theſe particles of bodies to be ſo diſpoſed amongſt themſelves, that the intervals, or empty ſpaces between them, may be equal in magnitude to them all; and that theſe particles may be compoſed of other particles much ſmaller, which have as much empty ſpace between them as equals all the magnitudes of theſe ſmaller particles; and that in like manner theſe ſmaller particles are again compoſed of others much ſmaller, all which together are equal to all the pores, or empty ſpaces, between them; and ſo on perpetually till you come to ſolid particles, ſuch as have no pores, or empty ſpaces within them: and if in any groſs body there be, for inſtance, three ſuch degrees of particles, the leaſt of which are ſolid; this body will have ſeven times more pores than ſolid parts. But if there be four ſuch degrees of particles, the leaſt of which are ſolid, the body will have fifteen times more pores than ſolid parts. If there be five degrees, the body will have one and thirty times more pores than ſolid parts. If ſix degrees, the body will have ſixty and three times more pores than ſolid parts. And ſo on perpetually. And there are other ways of conceiving how bodies may be exceeding porous. But what is really their inward frame is not yet known to us.

PROP. IX.

Bodies Reflect and Refract light by one and the ſame Power variouſly exerciſed in various circumſtances.

Reflexion and Refraction

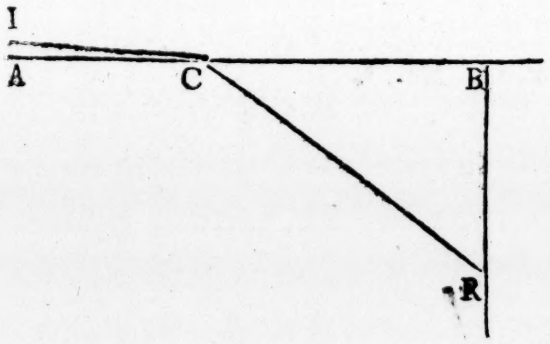
This appears by ſeveral conſiderations. Firſt, Becauſe when light goes out of glaſs into air, as obliquely as it can poſſibly do: if its incidence be made ſtill more oblique, it becomes totally Reflected. For the power of the glaſs, after it has refracted the light as obliquely as is poſſible, if the incidence be ſtill made more oblique, becomes too ſtrong to let any of its rays go through, and by conſequence cauſes total reflexions. Secondly, Becauſe

Becauſe light is alternately reflected and tranſmitted by thin plates of glaſs for many ſucceſſions, accordingly as the thickneſs of the plate increaſes in an arithmetical progreſſion. For here the thickneſs of the glaſs determines whether that power, by which glaſs acts upon light, ſhall cauſe it to be reflected, or ſuffer it to be tranſmitted. And, Thirdly, Becauſe thoſe ſurfaces of tranſparent bodies, which have the greateſt Refracting power, reflect the greateſt quantity of light, as was ſhewed in the firſt Propoſition. various effects of one Power.

PROP. X.

If light be ſwifter in bodies than in vacuo, *in the proportion of the ſines which meaſure the refraction of the bodies; the forces of the bodies to reflect and refract light, are very nearly proportional to the Denſities of the ſame bodies, excepting that Unctuous and Sulphureous bodies refract more than others of this ſame denſity.*

Let AB repreſent the refracting plane ſurface of any body, and IC a ray incident very obliquely upon the body in C, ſo that the angle ACI may be infinitely little; and let CR be the refracted ray. From a given point, B, perpendicular to the refracting ſurface erect BR, meeting with the refracted ray, CR, in R; and if CR repreſent the motion of the refracted ray, and this motion be diſtinguiſhed into two motions, CB and BR; whereof CB is parallel to the refracting plane, and BR perpendicular to it: CB ſhall repreſent the motion of the incident ray, and BR the motion generated by the refraction, as Opticians have of late explained. Refractive and Reflective force proportional to Denſity.



Now if any body or thing, in moving through any ſpace of a given breadth terminated on both ſides by two parallel planes, be urged forward, in all parts of that ſpace, by forces tending directly forwards towards the laſt plane, and before its incidence on the firſt plane, had no motion towards it, or but an infinitely little one; and if the forces, in all parts of that ſpace, between

Refractive and Reflective force

the planes, be at equal diſtances from the planes equal to one another; but at ſeveral diſtances be bigger or leſs in any given proportion: the motion generated by the forces, in the whole paſſage of the body or thing through that ſpace, ſhall be in a ſubduplicate proportion of the forces, as Mathematicians will eaſily underſtand (q). And therefore if the ſpace of Activity of the refracting ſuperficies of the body be conſidered as ſuch a ſpace, the motion of the ray generated by the Refracting force of the body, during its paſſage through that ſpace, that is the motion BR, muſt be in a ſubduplicate proportion of that Refracting force. I ſay, therefore, that the ſquare of the line BR, and by conſequence the Refracting force of the body, is very nearly as the Denſity of the ſame body. For this will appear by the following Table, wherein the proportion of the ſines which meaſure the refractions of ſeveral bodies, the ſquare of BR, ſuppoſing CB an unit, the denſities of the bodies eſtimated by their ſpecific Gravities, and their Refractive power in reſpect of their Denſities, are ſet down in ſeveral columns.

(q) See above, Book I. Part I. Ex. 15. Not. 5.

The

The refracting bodies.	The proportion of the ſines of incidence and refraction of yellow light.	The ſquare of BR, to which the refracting force of the body is proportionate.	The denſity and ſpecific gravity of the body.	The refractive power of the body in reſpect of its denſity.
A Pſeudo-Topazius, being a natural, pellucid, brittle, hairy ſtone, of a Yellow colour.	23 to 14	1'699	4'27	3979.
Air.	3201 to 3200	0'000625	0'0012	5208
Glaſs of Antimony.	17 to 9	2'568	5'28	4864
A Selenitis.	61 to 41	1'213	2'252	5386
Glaſs vulgar.	31 to 20	1'4025	2'58	5436
Cryſtal of the Rock.	25 to 16	1'445	2'65	5450
Iſland Cryſtal.	5 to 3	1'778	2'72	6536
Sal Gemmæ.	17 to 11	1'388	2'143	6477
Alume.	55 to 24	1'1267	1'714	6570
Borax.	22 to 15	1'1511	1'714	6716
Niter.	32 to 21	1'345	1'9	7079
Dantzick Vitriol.	303 to 200	1'295	1'715	7551
Oil of Vitriol.	10 to 7	1'041	1'7	6124
Rain Water.	529 to 396	0'7845	1'	7845
Gum Arabick.	31 to 21	1'179	1'375	8574
Spirit of Wine well rectified.	100 to 73	0'8765	0'866	10121
Camphire.	3 to 2	1'25	0'996	12551
Oil Olive.	22 to 15	1'1511	0'913	12607
Linſeed Oil.	40 to 27	1'1948	0'932	12819
Spirit of Turpentine.	25 to 17	1'1626	0'874	13222
Ambar.	14 to 9	1'42	1'04	13654
A Diamond.	100 to 41	4'949	3'4	14556

proportional to Denſity.

The refraction of the air, in this Table, is determined by that of the atmoſphere obſerved by Aſtronomers. For if light paſs through many refracting ſubſtances or mediums, gradually denſer and denſer, and terminated with parallel ſurfaces, the ſum of all the refractions will be equal to the ſingle refraction, which it would have ſuffered in paſſing immediately out of the firſt medium into the laſt. And this holds true, though the number of the refracting ſubſtances be increaſed to infinity, and the diſtances from one another as much decreaſed; ſo that the light may be refracted in every point of its paſſage, and, by continual refractions, bent into a curve line. And therefore the whole refraction of light, in paſſing through the atmoſphere from the higheſt and rareſt part thereof down to the loweſt and denſeſt part, muſt be equal to the refraction which it would ſuffer in paſſing, at like obliquity, out of a vacuum immediately into air, of equal denſity with that in the loweſt part of the atmoſphere.

Refractive force of

Now although a Pſeudo-topaz, a Selenitis, Rock Cryſtal, Iſland Cryſtal, Vulgar Glaſs (that is, ſand melted together) and glaſs of Antimony, which are terreſtrial ſtony alcalizate Concretes, and air, which probably ariſes from ſuch ſubſtances by fermentation, be ſubſtances very differing from one another in Denſity; yet by this Table, they have their Refractive powers almoſt in the ſame proportion to one another as their Denſities are, excepting that the refraction of that ſtrange ſubſtance Iſland Cryſtal is a little bigger than the reſt. And particularly air, which is 3500 times rarer than the Pſeudo-Topaz, and 4400 times rarer than Glaſs of Antimony, and 2000 times rarer than the Selenitis, Glaſs Vulgar, or Cryſtal of the Rock, has, notwithſtanding its rarity, the ſame Refractive power in reſpect of its Denſity, which thoſe very denſe ſubſtances have in reſpect of theirs, excepting ſo far as thoſe differ from one another.

Again, the refraction of Camphire, Oil Olive, Linſeed Oil, Spirit of Turpentine and Ambar, which are fat ſulphureous unctuous bodies, and a Diamond, which probably is an unctuous ſubſtance coagulated, have their Refractive Powers in proportion to one another as their Denſities, without any conſiderable variation. But the Refractive Powers of theſe unctuous ſubſtances are two or three times greater, in reſpect of their Denſities, than the Refractive Powers of the former ſubſtances in reſpect of theirs.

Water has a Refractive Power in a middle degree between thoſe two ſorts of ſubſtances, and probably is of a middle nature. For out of it grow all Vegetable and Animal ſubſtances, which conſiſt as well of ſulphureous fat and inflammable parts, as of earthly lean and alcalizate ones.

Salts and Vitriols have Refractive Powers in a middle degree between thoſe of Earthly ſubſtances and water; and accordingly are compoſed of thoſe two ſorts of ſubſtances. For by diſtillation and rectification of their ſpirits a great part of them goes into water, and a great part remains behind in the form of a dry fixed earth, capable of vitrification.

Spirit of Wine has a Refractive Power in a middle degree between thoſe of Water and Oily ſubſtances; and accordingly ſeems

to

to be compoſed of both, united by fermentation; the water, by means of ſome ſaline ſpirits with which it is impregnated, diſſolving the oil, and volatizing it by the action. For Spirit of Wine is inflammable by means of its oily parts; and being diſtilled often from Salt of Tartar, grows by every diſtillation more and more Aqueous and Phlegmatick. And Chemiſts obſerve, that Vegetables (as Lavender, Rue, Marjoram, &c.) diſtilled *per ſe*, before fermentation yield oils without any burning ſpirits; but after fermentation, yield ardent ſpirits without oils: which ſhews, that their oil is by fermentation converted into ſpirit. They find alſo, that if oils be poured in ſmall quantity upon fermentating vegetables, they diſtil over after fermentation in the form of ſpirits. various ſubſtances.

So then, by the foregoing Table, all bodies ſeem to have their Refractive Powers proportional to their Denſities, or very nearly; excepting ſo far as they partake more or leſs of ſulphureous oily particles, and thereby have their Refractive power made greater or leſs. Whence it ſeems rational to attribute the Refractive power of all bodies chiefly, if not wholly, to the ſulphureous parts with which they abound. For it is probable that all bodies abound more or leſs with ſulphurs. And as light, congregated by a burning-glaſs, acts moſt upon ſulphureous bodies, to turn them into fire and flame; ſo, ſince all action is mutual, ſulphurs ought to act moſt upon light. For that the action between light and bodies is mutual, may appear from this conſideration; that the denſeſt bodies, which refract and reflect light moſt ſtrongly, grow hotteſt in the ſummer-ſun, by the action of the refracted or reflected light.

I have hitherto explained the power of bodies to reflect and refract; and ſhewed, that thin tranſparent plates, fibres and particles do, according to their ſeveral thickneſſes and denſities, reflect ſeveral ſorts of rays, and thereby appear of ſeveral colours; and, by conſequence, that nothing more is requiſite for producing all the colours of Natural Bodies, than the ſeveral ſizes and denſities of their tranſparent particles. But whence it is that theſe plates, fibres and particles do, according to their ſeveral thickneſſes and denſities, reflect ſeveral ſorts of rays, I have not yet explained.

explained. To give ſome inſight into this matter, and make way for underſtanding the next part of this Book, I ſhall conclude this Part with a few more Propoſitions. Thoſe, which preceded, reſpect the nature of bodies: theſe, the nature of light: for both muſt be underſtood, before the reaſon of their actions upon one another can be known. And becauſe the laſt Propoſition depended upon the velocity of light, I will begin with a Propoſition of that kind.

PROP. XI.

Light is propagated from luminous Bodies in time; and ſpends about ſeven or eight minutes of an hour in paſſing from the Sun to the Earth.

Light propagated in time.

This was obſerved firſt by *Roemer*, and then by others, by means of the eclipſes of the ſatellites of *Jupiter*. For theſe eclipſes, when the earth is between the Sun and *Jupiter*, happen about ſeven or eight minutes ſooner, than they ought to do by the Tables; and when the Earth is beyond the Sun, they happen about ſeven or eight minutes later than they ought to do; the reaſon being, that the light of the ſatellites has farther to go in the latter caſe, than in the former, by the diameter of the Earth's orbit. Some inequalities of time may ariſe from the excentricities of the orbs of the ſatellites; but thoſe cannot anſwer in all the ſatellites, and at all times, to the poſition and diſtance of the Earth from the Sun. The mean motions of *Jupiter*'s ſatellites is alſo ſwifter in his deſcent from his aphelium to his perihelium, than in his aſcent in the other half of his orb: but this inequality has no reſpect to the poſition of the Earth, and in the three interior ſatellites is inſenſible; as I find by computation from the theory of their gravity.

PROP.

PROP. XII.

Every ray of Light, in its paſſage through any refracting ſurface, is put into a certain tranſient conſtitution or ſtate; which, in the progreſs of the ray, returns at equal intervals; and diſpoſes the ray, at every return, to be eaſily tranſmitted through the next refracting ſurface, and, between the returns, to be eaſily reflected by it.

This is manifeſt by the 5th, 9th, 12th and 15th Obſervations. For by thoſe Obſervations it appears, that one and the ſame ſort of rays, at equal angles of incidence on any thin tranſparent plate, is alternately reflected and tranſmitted for many ſucceſſions, accordingly as the Thickneſs of the plate increaſes in arithmetical progreſſion of the numbers, 0, 1, 2, 3, 4, 5, 6, 7, 8, &c. ſo that if the Firſt reflexion (that which makes the firſt or innermoſt of the rings of colours there deſcribed) be made at the thickneſs 1, the rays ſhall be tranſmitted at the thickneſſes 0, 2, 4, 6, 8, 10, 12, &c. and thereby make the central ſpot and rings of light, which appear by tranſmiſſion; and be reflected at the thickneſs 1, 3, 5, 7, 9, 11, &c. and thereby make the rings which appear by reflexion. And this alternate reflexion and tranſmiſſion, as I gather by the 24th Obſervation, continues for above an hundred viciſſitudes, and by the Obſervations in the next part of this Book, for many thouſands; being propagated from one ſurface of a glaſs-plate to the other, though the thickneſs of the plate be a quarter of an inch, or above; ſo that this alternation ſeems to be propagated from every refracting ſurface, to all diſtances, without end or limitation. Fits of Tranſmiſſion and Reflexion.

This alternate reflexion and refraction depends on both the ſurfaces of every thin plate, becauſe it depends on their diſtance. By the 21ſt Obſervation, if either ſurface of a thin plate of Muſcovy-glaſs be wetted, the colours cauſed by the alternate reflexion and refraction grow faint, and therefore it depends on them both.

It is therefore performed at the ſecond ſurface; for if it were performed at the firſt, before the rays arrive at the ſecond, it would not depend on the ſecond.

Fits of Tranſmiſſion

It is alſo influenced by ſome action or diſpoſition, propagated from the firſt to the ſecond; becauſe otherwiſe at the ſecond it would not depend on the firſt. And this action or diſpoſition, in its propagation, intermits and returns by equal intervals; becauſe in all its progreſs it inclines the ray, at one diſtance from the firſt ſurface, to be reflected by the ſecond; at another, to be tranſmitted by it, and that by equal intervals for innumerable viciſſitudes. And becauſe the ray is diſpoſed to reflexion at the diſtances 1, 3, 5, 7, 9, &c. and to tranſmiſſion at the diſtances 0, 2, 4, 6, 8, 10, &c. (for its tranſmiſſion through the firſt ſurface is at the diſtance 0, and it is tranſmitted through both together, if their diſtance be infinitely little or much leſs than 1) the diſpoſition to be tranſmitted at the diſtances 2, 4, 6, 8, 10, &c. is to be accounted a return of the ſame diſpoſition which the ray firſt had at the diſtance 0; that is, at its tranſmiſſion through the firſt refracting ſurface. All which is the thing I would prove.

What kind of action or diſpoſition this is; whether it conſiſts in a circulating or a vibrating motion of the ray, or of the medium, or ſomething elſe, I do not here enquire. Thoſe that are averſe from aſſenting to any new diſcoveries, but ſuch as they can explain by an hypotheſis, may for the preſent ſuppoſe, that as ſtones, by falling upon water, put the water into an undulating motion, and all bodies by percuſſion excite vibrations in the air; ſo the rays of light, by impinging on any refracting or reflecting ſurface, excite vibrations in the refracting or reflecting medium or ſurſtance, and by exciting them agitate the ſolid parts of the refracting or reflecting body, and by agitating them cauſe the body to grow warm or hot; that the vibrations, thus excited, are propagated in the refracting or reflecting medium or ſubſtance, much after the manner that vibrations are propagated in the air for cauſing ſound, and move faſter than the rays, ſo as to overtake them; and that when any ray is in that part of the vibration which conſpires with its motion, it eaſily breaks through a refracting ſurface; but when it is in the contrary part of the vibration, which impedes its motion, it is eaſily reflected; and, by conſequence, that every ray is ſucceſſively diſpoſed to be eaſily reflected, or eaſily tranſmitted, by every vibration which overtakes

takes it. But whether this hypotheſis be true or falſe, I do not here conſider. I content myſelf with the bare diſcovery, that the rays of light are, by ſome cauſe or other, alternately diſpoſed to be reflected or refracted for many viciſſitudes. and Reflexion.

DEFINITION.

The returns of the diſpoſition of any Ray to be reflected I will call its Fits of eaſy Reflexion; *and thoſe of its diſpoſition to be tranſmitted, its* Fits of eaſy Tranſmiſſion; *and the ſpace it paſſes between every return and the next return, the* Interval of its Fits.

PROP. XIII.

The reaſon why the ſurfaces of all thick tranſparent bodies Reflect part of the light incident on them, and Refract the reſt, is, that ſome rays, at their incidence, are in Fits of eaſy Reflexion, and others in Fits of eaſy Tranſmiſſion.

This may be gathered from the 24th Obſervation, where the light reflected by thin plates of air and glaſs, which to the naked eye appeared evenly white all over the plate, did through a priſm appear waved with many ſucceſſions of light and darkneſs, made by alternate fits of eaſy Reflexion and eaſy Tranſmiſſion, the priſm ſevering and diſtinguiſhing the waves of which the White reflected light was compoſed, as was explained above.

And hence light is in fits of eaſy Reflexion and eaſy Tranſmiſſion, before its incidence on tranſparent bodies. And probably it is put into ſuch fits at its firſt emiſſion from luminous bodies, and continues in them during all its progreſs. For theſe fits are of a laſting nature, as will appear by the next part of this Book.

In this Propoſition I ſuppoſe the tranſparent bodies to be thick; becauſe if the thickneſs of the body be much leſs than the interval of the fits of eaſy Reflexion and Tranſmiſſion of the rays, the body loſeth its reflecting power. For if the rays, which at their entering into the body are put into fits of eaſy Tranſmiſſion, arrive at the fartheſt ſurface of the body before they be out of thoſe fits, they muſt be tranſmitted. And this is the rea-

Fits of Tranſmiſſion

ſon why bubbles of water loſe their reflecting power when they grow very thin, and why all Opake bodies, when reduced into very ſmall parts, become tranſparent.

PROP. XIV.

The ſurfaces of tranſparent bodies, which, if the ray be in a fit of Refraction, do refract it moſt ſtrongly; if the ray be in a fit of Reflexion, do reflect it moſt eaſily.

For we ſhewed above in Prop. VIII. that the cauſe of Reflexion is not the impinging of light on the ſolid impervious Parts of bodies, but ſome other power, by which thoſe ſolid Parts act on light at a diſtance. We ſhewed alſo in Prop. IX. that bodies reflect and refract light by one and the ſame power variouſly exerciſed in various circumſtances; and in Prop. I. that the moſt ſtrongly refracting ſurfaces reflect the moſt light: all which compared together evince and ratify both this and the laſt Propoſition.

PROP. XV.

In any one and the ſame ſort of rays, emerging in any angle out of any refracting ſurface into one and the ſame medium, the Intervals of the following fits of eaſy Reflexion and Tranſmiſſion are, either accurately or very nearly, as the rectangle of the ſecant of the angle of refraction, and of the ſecant of another angle, whoſe ſine is the firſt of 106 arithmetical mean proportionals between the ſines of incidence and refraction, counted from the ſine of refraction.

This is manifeſt by the 7th and 19th Obſervations.

PROP.

PROP. XVI.

and Reflexion.

In ſeveral ſorts of rays, emerging in equal angles out of any refracting ſurface into the ſame medium, the intervals of the following fits of eaſy Reflexion and eaſy Tranſmiſſion are, either accurately or very nearly, as the cube-roots of the ſquares of the lengths of a chord, which ſound the notes of an eight, ſol, la, fa, ſol, la, mi, fa, ſol, *with all their intermediate degrees anſwering to the colours of thoſe rays, according to the analogy deſcribed in the ſeventh Experiment of the ſecond Part of the firſt Book.*

This is manifeſt by the 13th and 14th Obſervations.

PROP. XVII.

If rays of any ſort paſs perpendicularly into ſeveral mediums, the intervals of the fits of eaſy Refraction and Tranſmiſſion in any one medium, are to thoſe intervals in any other, as the ſine of incidence to the ſine of refraction, when the rays paſs out of the firſt of thoſe two mediums into the ſecond.

This is manifeſt by the 10th Obſervation.

PROP. XVIII.

If the rays, which paint the colour in the confine of Yellow and Orange, paſs perpendicularly out of any medium into air, the intervals of their fits of eaſy Reflexion are the $\frac{1}{89000}$th part of an inch. And of the ſame length are the intervals of their fits of eaſy Tranſmiſſion.

This is manifeſt by the 6th Obſervation.

From theſe Propoſitions it is eaſy to collect the intervals of the fits of eaſy Reflexion and Tranſmiſſion of any ſort of rays, refracted in any angle into any medium; and thence to know, whether the rays ſhall be reflected or tranſmitted, at their ſubſequent incidence upon any other pellucid medium. Which thing, being uſeful for underſtanding the next part of this Book, was here to be ſet down. And for the ſame reaſon I add the two following Propoſitions.

PROP.

Fits of Tranſmiſſion

PROP. XIX.

If any ſort of rays, falling on the polite ſurface of any pellucid medium, be reflected back; the fits of eaſy Reflexion, which they have at the point of reflexion, ſhall ſtill continue to return; and the returns ſhall be at diſtances from the point of reflexion in the arithmetical progreſſion of the numbers 2, 4, 6, 8, 10, 12, *&c. and between theſe fits the rays ſhall be in fits of eaſy Tranſmiſſion.*

For ſince the fits of eaſy Reflexion and eaſy Tranſmiſſion are of a returning nature, there is no reaſon why theſe fits, which continued till the ray arrived at the reflecting medium, and there inclined the ray to reflexion, ſhould there ceaſe. And if the ray, at the point of reflexion, was in a fit of eaſy reflexion, the progreſſion of the diſtances of theſe fits from that point muſt begin from 0, and ſo be of the numbers 0, 2, 4, 6, 8, &c. And therefore the progreſſion of the diſtances of the intermediate fits of eaſy Tranſmiſſion, reckoned from the ſame point, muſt be in the progreſſion of the odd numbers 1, 3, 5, 7, 9, &c. contrary to what happens, when the fits are propagated from points of refraction.

PROP. XX.

The intervals of the fits of eaſy Reflexion and eaſy Tranſmiſſion, propagated from points of reflexion into any medium, are equal to the intervals of the like fits, which the ſame rays would have, if refracted into the ſame medium in angles of refraction equal to their angles of reflexion.

For when light is reflected by the ſecond ſurface of thin plates, it goes out afterwards freely at the firſt ſurface, to make the rings of colours which appear by reflexion; and by the freedom of its egreſs, makes the colours of theſe rings more vivid and ſtrong, than thoſe which appear on the other ſide of the plates, by the tranſmitted light. The reflected rays are therefore in fits of eaſy Tranſmiſſion at their egreſs; which would not always happen, if the intervals of the fits within the plate, after reflexion, were not

not equal both in length and number to their intervals before it. And this confirms alſo the proportions ſet down in the former Propoſition. For if the rays, both in going in and out at the firſt ſurface, be in fits of eaſy Tranſmiſſion, and the intervals and numbers of thoſe fits between the firſt and ſecond ſurface, before and after reflexion, be equal; the diſtances of the fits of eaſy Tranſmiſſion from either ſurface muſt be in the ſame progreſſion after reflexion as before; that is, from the Firſt ſurface which tranſmitted them, in the progreſſion of the Even numbers 0, 2, 4, 6, 8, &c: and from the Second which reflected them, in that of the Odd numbers 1, 3, 5, 7, &c. But theſe two Propoſitions will become much more evident by the Obſervations in the following part of this Book. and Reflexion.

THE

THE SECOND BOOK OF OPTICKS.

PART IV.

Obſervations concerning the Reflexions and Colours of thick tranſparent poliſhed Plates.

Reflexion and Colours

THERE is no glaſs or ſpeculum, how well ſoever poliſhed, but, beſides the light which it refracts or reflects regularly, ſcatters every way irregularly a faint light; by means of which the poliſhed ſurface, when illuminated in a dark room by a beam of the ſun's light, may be eaſily ſeen in all poſitions of the eye. There are certain phænomena of this ſcattered light, which when I firſt obſerved them, ſeemed very ſtrange and ſurpriſing to me. My Obſervations were as follows.

Obſ. 1. The ſun ſhining into my darkened chamber, through a hole one-third of an inch wide, I let the intromitted beam of light fall perpendicularly upon a glaſs-ſpeculum, ground concave on one ſide and convex on the other, to a ſphere of five feet and eleven inches radius, and quick-ſilvered over on the convex ſide. And holding a white opake chart or a quire of paper, at the center of the ſpheres to which the ſpeculum was ground, that is, at the diſtance of about five feet and eleven inches from the ſpeculum, in ſuch manner, that the beam of light might paſs

through a little hole made in the middle of the chart to the ſpeculum, and thence be reflected back to the ſame hole: I obſerved upon the chart four or five concentric iriſes, or rings of colours, like rainbows, encompaſſing the hole; much after the manner that thoſe, which in the fourth and following Obſervations of the firſt Part of the third (r) Book appeared between the object-glaſſes, encompaſſed the Black ſpot, but yet larger and fainter than thoſe. Theſe rings, as they grew larger and larger, became diluter and fainter, ſo that the fifth was ſcarce viſible. Yet ſometimes, when the ſun ſhone very clear, there appeared faint lineaments of a ſixth and ſeventh. If the diſtance of the chart from the ſpeculum was much greater or much leſs than that of ſix feet, the rings became dilute and vaniſhed. And if the diſtance of the ſpeculum from the window was much greater than that of ſix feet, the reflected beam of light would be ſo broad at the diſtance of ſix feet from the ſpeculum, where the rings appeared, as to obſcure one or two of the innermoſt rings. And therefore I uſually placed the ſpeculum at about ſix feet from the window; ſo that its focus might there fall in with the center of its concavity, at the rings upon the chart. And this poſture is always to be underſtood in the following Obſervations where no other is expreſſed. of thick tranſparent Plates.

Obſ. 2. The colours of theſe rainbows ſucceeded one another from the center outwards, in the ſame form and order, with thoſe which were made in the ninth Obſervation of the firſt Part of this Book by light not reflected, but tranſmitted through the two object-glaſſes. For, firſt, there was in their common center a White round ſpot of faint light, ſomething broader than the reflected beam of light; which beam ſometimes fell upon the middle of the ſpot, and ſometimes, by a little inclination of the ſpeculum, receded from the middle, and left the ſpot white to the center.

This White ſpot was immediately encompaſſed with a dark Grey or Ruſſet, and that dark Grey with the colours of the firſt iris; which colours, on the inſide next the dark Grey, were a little Violet and Indigo, and next to that a Blue, which on the

(r) —— *third.*] Read *ſecond.*

Reflexion and Colours

outside grew pale; and then succeeded a little greenish Yellow; and after that a brighter Yellow; and then, on the outward edge of the iris, a Red, which on the outside inclined to Purple.

This iris was immediately encompassed with a second; whose colours were in order from the inside outwards, Purple, Blue, Green, Yellow, light Red, a Red mixed with Purple.

Then immediately followed the colours of the third iris; which were in order outwards a Green inclining to Purple, a good Green, and a Red more bright than that of the former iris.

The fourth and fifth iris seemed of a blueish Green within, and Red without; but so faintly that it was difficult to discern the colours.

Obs. 3. Measuring the diameters of these rings upon the chart, as accurately as I could, I found them also in the same proportion to one another with the rings made by light transmitted through the two object-glass. For the diameters of the four first of the bright rings, measured between the brightest parts of their orbits, at the distance of six feet from the speculum, were $1\frac{11}{16}$, $2\frac{3}{8}$, $2\frac{11}{12}$, $3\frac{3}{8}$ inches; whose squares are in arithmetical progression of the numbers 1, 2, 3, 4. If the White circular spot in the middle be reckoned amongst the rings, and its central light, where it seems to be most luminous, be put equipollent to an infinitely little ring; the squares of the diameters of the rings will be in the progression 0, 1, 2, 3, 4, &c. I measured also the diameters of the dark circles between these luminous ones; and found their squares in the progression of the numbers $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c. the diameters of the first four, at the distance of six feet from the speculum, being $1\frac{3}{16}$, $2\frac{1}{16}$, $2\frac{2}{3}$, $4\frac{3}{20}$ (s) inches. If the distance of the chart from the speculum was increased or diminished, the diameters of the circles were increased or diminished proportionally.

Obs. 4. By the analogy between these rings and those described in the Observations of the first Part of this Book, I suspected that there were many more of them which spread into one another, and by interfering mixed their colours, and diluted one another so that they could not be seen apart. I viewed them therefore

(s) $4\frac{3}{10}$. So the third edition. All the rest $3\frac{3}{10}$.

through

through a prism, as I did those in the 24th Observation of the first Part of this Book. And when the prism was so placed as, by refracting the light of their mixed colours, to separate them, and distinguish the rings from one another, as it did those in that Observation; I could then see them distincter than before, and easily number eight or nine of them, and sometimes twelve or thirteen. And had not their light been so very faint, I question not but that I might have seen many more. of thick transparent Plates.

Obs. 5. Placing a prism at the window to refract the intromitted beam of light, and cast the oblong spectrum of colours on the speculum: I covered the speculum with a black paper, which had in the middle of it a hole to let any one of the colours pass through to the speculum, whilst the rest were intercepted by the paper. And now I found rings of that colour only, which fell upon the speculum. If the speculum was illuminated with Red, the rings were totally Red with dark intervals; if with Blue, they were totally Blue, and so of the other colours. And when they were illuminated with any one colour, the squares of their diameters, measured between their most luminous parts, were in the arithmetical progression of the numbers 0, 1, 2, 3, 4; and the squares of the diameters of their dark intervals, in the progression of the intermediate numbers $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$. But if the colour was varied, they varied their magnitude. In the Red, they were largest; in the Indigo and Violet, least; and in the intermediate colours, Yellow, Green and Blue, they were of several intermediate bignesses answering to the colour; that is, greater in Yellow than in Green, and greater in Green than in Blue. And hence I knew, that when the speculum was illuminated with White light, the Red and Yellow, on the outside of the rings, were produced by the least refrangible rays, and the Blue and Violet by the most refrangible; and that the colours of each ring spread into the colours of the neighbouring rings on either side, after the manner explained in the first and second Part of this Book; and, by mixing, diluted one another so that they could not be distinguished; unless near the center, where they were least mixed. For in this Observation I could see the rings more distinctly, and to a greater number than before; being able in the Yellow light to

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number eight or nine of them, beſides a faint ſhadow of a tenth. To ſatisfy myſelf how much the colours of the ſeveral rings ſpread into one another, I meaſured the diameters of the ſecond and third rings; and found them, when made by the confine of the Red and Orange, to be to the ſame diameters, when made by the confine of Blue and Indigo, as 9 to 8, or thereabouts. For it was hard to determine this proportion accurately. Alſo the circles made ſucceſſively by the Red, Yellow and Green, differed more from one another than thoſe made ſucceſſively by the Green, Blue and Indigo. For the circle made by the Violet was too dark to be ſeen. To carry on the computation, let us therefore ſuppoſe, that the differences of the diameters of the circles made by the utmoſt Red, the confine of Red and Orange, the confine of Orange and Yellow, the confine of Yellow and Green, the confine of Green and Blue, the confine of Blue and Indigo, the confine of Indigo and Violet, and outmoſt Violet, are in proportion as the differences of the lengths of a monochord which ſound the tones in an eight; *ſol*, *la*, *fa*, *ſol*, *la*, *mi*, *fa*, *ſol*, that is, as the numbers $\frac{1}{9}$, $\frac{1}{18}$, $\frac{1}{12}$, $\frac{1}{12}$, $\frac{2}{27}$, $\frac{1}{27}$, $\frac{1}{18}$. And if the diameter of the circle made by the confine of Red and Orange be 9A, and that of the circle made by the confine of Blue and Indigo be 8A, as above; their difference, 9A − 8A, will be to the difference of the diameters of the circle made by the outmoſt Red, and by the confine of Red and Orange as $\frac{1}{18} + \frac{1}{12} + \frac{1}{12} + \frac{2}{27}$ to $\frac{1}{9}$, that is as $\frac{8}{27}$ to $\frac{1}{9}$, or 8 to 3; and to the difference of the circles made by the outmoſt Violet, and by the confine of Blue and Indigo as $\frac{1}{18} + \frac{1}{12} + \frac{1}{12} + \frac{2}{27}$ to $\frac{1}{27} + \frac{1}{18}$, that is, as $\frac{8}{27}$ to $\frac{5}{54}$, or as 16 to 5. And therefore theſe differences will be $\frac{3}{8}$A and $\frac{5}{16}$A. Add the firſt to 9A, and ſubduct the laſt from 8A, and you will have the diameters of the circles made by the leaſt and moſt refrangible rays, $\frac{75}{8}$A and $\frac{61\frac{1}{2}}{8}$A. Theſe diameters are therefore to one another as 75 to $61\frac{1}{2}$, or 50 to 41; and their ſquares as 2500 to 1681, that is, as 3 to 2 very nearly. Which proportion differs not much from the proportion of the diameters of the circles made by the outmoſt Red and outmoſt Violet, in the 13th Obſervation of the firſt Part of this Book.

Obſ. 6. Placing my eye where theſe rings appeared plaineſt, I ſaw the ſpeculum tinged all over with waves of colours (Red, Yellow,

low, Green, Blue); like thoſe which, in the Obſervations of the firſt Part of this Book, appeared between the object-glaſſes and upon bubbles of water, but much larger. And after the manner of thoſe, they were of various magnitudes in various poſitions of the eye; ſwelling and ſhrinking as I moved my eye this way and that way. They were formed like arcs of concentrick circles, as thoſe were; and when my eye was over againſt the center of the concavity of the ſpeculum (that is, 5 feet and 10 inches diſtant from the ſpeculum) their common center was in a right line with that center of concavity, and with the hole in the window. But in other poſtures of my eye their center had other poſitions. They appeared by the light of the clouds, propagated to the ſpeculum through the hole in the window; and when the ſun ſhone through that hole upon the ſpeculum, his light upon it was of the colour of the ring whereon it fell; but, by its ſplendor, obſcured the rings made by the light of the clouds, unleſs when the ſpeculum was removed to a great diſtance from the window, ſo that his light upon it might be broad and faint. By varying the poſition of my eye, and moving it nearer to or farther from the direct beam of the ſun's light, the colour of the ſun's reflected light conſtantly varied upon the ſpeculum, as it did upon my eye; the ſame colour always appearing to a by-ſtander upon my eye, which to me appeared upon the ſpeculum. And thence I knew, that the rings of colours upon the chart were made by theſe reflected colours, propagated thither from the ſpeculum in ſeveral angles; and that their production depended not upon the termination of light and ſhadow.

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Obſ. 7. By the analogy of all theſe phænomena with thoſe of the like rings of colours deſcribed in the firſt Part of this Book; it ſeemed to me, that theſe colours were produced by this thick plate of glaſs, much after the manner that thoſe were produced by very thin plates. For, upon trial, I found that if the quick-ſilver were rubbed off from the backſide of the ſpeculum, the glaſs alone would cauſe the ſame rings of colours; but much more faint than before: and therefore the phænomenon depends not upon the quick-ſilver, unleſs ſo far as the quick-ſilver, by increaſing the reflexion of the backſide of the glaſs, increaſes the

light

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light of the rings of colours. I found alſo that a ſpeculum of metal without glaſs, made ſome years ſince for optical uſes, and very well wrought, produced none of thoſe rings; and thence I underſtood that theſe rings ariſe not from one ſpecular ſurface alone, but depend upon the two ſurfaces of the plate of glaſs whereof the ſpeculum was made, and upon the thickneſs of the glaſs between them. For, as in the 7th and 19th Obſervations of the firſt Part of this Book, a thin plate of air, water or glaſs, of an even thickneſs, appeared of one colour, when the rays were perpendicular to it; of another, when they were a little oblique; of another, when more oblique; of another, when ſtill more oblique, and ſo on: ſo here, in the ſixth Obſervation, the light, which emerged out of the glaſs in ſeveral obliquities, made the glaſs appear of ſeveral colours; and being propagated in thoſe obliquities to the chart, there painted rings of thoſe colours. And as the reaſon why a thin plate appeared of ſeveral colours in ſeveral obliquities of the rays, was, that the rays of one and the ſame ſort are reflected by the thin plate at one obliquity, and tranſmitted at another; and thoſe of other ſorts tranſmitted where theſe are reflected, and reflected where theſe are tranſmitted: ſo the reaſon why the thick plate of glaſs, whereof the ſpeculum was made, did appear of various colours in various obliquities, and in thoſe obliquities propagated thoſe colours to the chart, was, that the rays of one and the ſame ſort did, at one obliquity, emerge out of the glaſs; at another, did not emerge, but were reflected back towards the quick-ſilver by the hither ſurface of the glaſs: and, accordingly as the obliquity became greater and greater, emerged and were reflected alternately for many ſucceſſions; and that in one and the ſame obliquity the rays of one ſort were reflected, and thoſe of another tranſmitted. This is manifeſt by the fifth Obſervation of this Part of this Book. For in that Obſervation, when the ſpeculum was illuminated by any one of the Priſmatick Colours, that light made many rings of the ſame colour upon the chart with dark intervals; and therefore, at its emergence out of the ſpeculum, was alternately tranſmitted and not tranſmitted from the ſpeculum to the chart for many ſucceſſions, according to the various obliquities of its emergence.

ence. And when the colour caſt on the ſpeculum by the priſm was varied, the rings became of the colour caſt on it, and varied their bigneſs with their colour; and therefore the light was now alternately tranſmitted and not tranſmitted, from the ſpeculum to the chart, at other obliquities than before. It ſeemed to me therefore, that theſe rings were of one and the ſame original with thoſe of thin plates; but yet with this difference, that thoſe of thin plates are made by the alternate reflexions and tranſmiſſions of the rays at the ſecond ſurface of the plate, after one paſſage through it; but here, the rays go twice through the plate, before they are alternately reflected and tranſmitted. Firſt, they go through it from the firſt ſurface to the quick-ſilver; and then return through it from the quick-ſilver to the firſt ſurface; and there are either tranſmitted to the chart, or reflected back to the quick-ſilver, accordingly as they are in their fits of eaſy Reflexion or Tranſmiſſion, when they arrive at that ſurface. For the intervals of the fits of the rays, which fall perpendicularly on the ſpeculum and are reflected back in the ſame perpendicular lines, by reaſon of the equality of theſe angles and lines, are of the ſame length and number within the glaſs after reflexion, as before; by the 19th Propoſition of the third Part of this Book. And therefore, ſince all the rays that enter through the firſt ſurface are in their fits of eaſy Traſmiſſion at their entrance; and as many of theſe as are reflected by the ſecond, are in their fits of eaſy Reflexion there; all theſe muſt be again in their fits of eaſy Tranſmiſſion at their return to the firſt; and by conſequence there go out of the glaſs to the chart, and form upon it the White ſpot of light in the center of the rings. For the reaſon holds good in all ſorts of rays, and therefore all ſorts muſt go out promiſcuouſly to that ſpot, and by their mixture cauſe it to be White. But the intervals of the fits of thoſe rays, which are reflected more obliquely than they enter, muſt be greater after reflexion than before, by the 15th and 20th Propoſitions. And hence it may happen that the rays, at their return to the firſt ſurface, may, in certain obliquities, be in fits of eaſy Reflexion, and return back to the quick-ſilver; and, in other intermediate obliquities, be again in fits of eaſy Traſmiſſion, and ſo go out to the chart, and paint on it

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it the rings of colours about the White ſpot. And becauſe the intervals of the fits, at equal obliquities, are greater and fewer in the leſs refrangible rays, and leſs and more numerous in the more refrangible; therefore the leſs refrangible, at equal obliquities, ſhall make fewer rings than the more refrangible; and the rings made by thoſe ſhall be larger than the like number of rings made by theſe; that is, the Red rings ſhall be larger than the Yellow, the Yellow than the Green, the Green than the Blue, and the Blue than the Violet; as they were really found to be in the fifth Obſervation. And therefore the firſt ring, of all colours encompaſſing the White ſpot of light, ſhall be Red without any Violet within, and Yellow and Green and Blue in the middle, as it was found in the ſecond Obſervation; and theſe colours in the ſecond ring, and thoſe that follow, ſhall be more expanded, till they ſpread into one another, and blend one another by interfering.

Theſe ſeem to be the reaſons of theſe rings in general; and this put me upon obſerving the Thickneſs of the glaſs, and conſidering whether the dimenſions and proportions of the rings may be truly derived from it by computation.

Obſ. 8. I meaſured therefore the thickneſs of this concavo-convex plate of glaſs, and found it every where $\frac{1}{4}$ of an inch preciſely. Now, by the ſixth Obſervation of the firſt Part of this Book, a thin plate of air tranſmits the brighteſt light of the Firſt ring, that is the bright Yellow, when its thickneſs is the $\frac{1}{89000}$th part of an inch; and by the tenth Obſervation of the ſame Part, a thin plate of glaſs tranſmits the ſame light of the ſame ring, when its thickneſs is leſs in proportion of the ſine of refraction to the ſine of incidence; that is, when its thickneſs is the $\frac{11}{1513000}$th or $\frac{1}{137545}$th part of an inch, ſuppoſing the ſines are as 11 to 17. And if this thickneſs be doubled, it tranſmits the ſame bright light of the Second ring; if tripled, it tranſmits that of the Third, and ſo on; the bright Yellow light in all theſe caſes being in its fits of Tranſmiſſion. And therefore if its thickneſs be multiplied 34386 times, ſo as to become $\frac{1}{4}$ of an inch, it tranſmits the ſame bright light of the 34386th ring. Suppoſe this be the bright Yellow light, tranſmitted perpendicularly from the reflecting convex ſide of the glaſs through the concave ſide, to the

the White ſpot in the center of the rings of colours on the chart: and by a rule in the 7th and 19th Obſervations in the firſt Part of this Book, and by the 15th and 20th Propoſitions of the third Part of this Book, if the rays be made oblique to the glaſs, the thickneſs of the glaſs requiſite to tranſmit the ſame bright light of the ſame ring, in any obliquity, is to this thickneſs of $\frac{1}{4}$ of an inch, as the ſecant of a certain angle to the radius; the ſine of which angle is the firſt of an hundred and ſix arithmetical means between the ſines of incidence and refraction, counted from the ſine of incidence, when the refraction is made out of any plated body into any medium encompaſſing it, that is, in this caſe, out of glaſs into air. Now if the thickneſs of the glaſs be increaſed by degrees, ſo as to bear to its firſt thickneſs (viz. that of a quarter of an inch) the proportions which 34386 (the number of fits of the Perpendicular rays in going through the glaſs towards the White ſpot in the center of the rings) hath to 34385, 34384, 34383 and 34382 (the numbers of the fits of the Oblique rays in going through the glaſs towards the firſt, ſecond, third and fourth rings of colours). And if the firſt thickneſs be divided into 100000000 equal parts, the increaſed thickneſſes will be 100002908, 100005816, 100008725 and 100011633; and the angles, of which theſe thickneſſes are ſecants, will be 26′ 13″, 37′ 5″, 45′ 6″ and 52′ 26″, the radius being 100000000; and the ſines of theſe angles are 762, 1079, 1321 and 1525; and the proportional ſines of refraction 1172, 1659, 2031 and 2345, the radius being 100000. For ſince the ſines of incidence, out of glaſs into air, are to the ſines of refraction as 11 to 17; and to the above-mentioned ſecants, as 11 to the firſt of 106 arithmetical means between 11 and 17, that is, as 11 to $11\frac{6}{106}$; thoſe ſecants will be to the ſines of refraction, as $11\frac{6}{106}$ to 17, and by this analogy will give theſe ſines. So then if the obliquities of the rays to the concave ſurface of the glaſs be ſuch, that the ſines of their refraction, in paſſing out of the glaſs through that ſurface into the air, be 1172, 1659, 2031, 2345, the bright light of the 34386th ring ſhall emerge at the thickneſſes of the glaſs which are to $\frac{1}{4}$ of an inch, as 34386 to 34385, 34384, 34383, 34382, reſpectively. And therefore if the

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thickneſs in all theſe caſes be $\frac{1}{4}$ of an inch (as it is in the glaſs of which the ſpeculum was made) the bright light of the 34385th ring ſhall emerge where the ſine of refraction is 1172; and that of the 34384th, 34383d and 34382d ring, where the ſine is 1659, 2031 and 2345 reſpectively. And in theſe angles of refraction the light of theſe rings ſhall be propagated from the ſpeculum to the chart; and there paint rings about the white central round ſpot of light, which we ſaid was the light of the 34386th ring. And the ſemi-diameters of theſe rings ſhall ſubtend the angles of refraction made at the concave ſurface of the ſpeculum; and by conſequence their diameters ſhall be to the diſtance of the chart from the ſpeculum, as thoſe ſines of refraction doubled are to the radius; that is, as 1172, 1659, 2031, and 2345, doubled, are to 100000. And therefore if the diſtance of the chart from the concave ſurface of the ſpeculum be ſix feet (as it was in the third of theſe Obſervations) the diameters of the rings of this bright Yellow light upon the chart ſhall be 1'688, 2'389, 2'925, 3'375 inches. For theſe diameters are to ſix feet, as the above-mentioned ſines doubles are to the radius. Now theſe diameters of the bright Yellow rings, thus found by computation, are the very ſame with thoſe found in the third of theſe Obſervations by meaſuring them, viz. with $1\frac{11}{16}$, $2\frac{3}{8}$, $2\frac{11}{12}$, and $3\frac{3}{8}$ inches; and therefore the theory of deriving theſe rings from the Thickneſs of the plate of glaſs, of which the ſpeculum was made, and from the obliquity of the emerging rays, agrees with the Obſervation. In this computation I have equalled the diameters of the bright rings made by light of all colours, to the diameters of the rings made by the bright Yellow. For this Yellow makes the brighteſt part of the rings of all colours. If you deſire the diameters of the rings made by the light of any other unmixed colour, you may find them readily, by putting them to the diameters of the bright Yellow ones in a ſubduplicate proportion of the intervals of the fits of the rays of thoſe colours, when equally inclined to the refracting or reflecting ſurface, which cauſed thoſe fits; that is, by putting the diameters of the rings made by the rays in the extremities and limits of the ſeven colours, Red, Orange, Yellow, Green, Blue, Indigo, Violet, proportional to the cube-

roots of the numbers 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, $\frac{1}{2}$, which expreſs the lengths of a monochord ſounding the notes in an eighth: for by this means the diameters of the rings of theſe colours will be found pretty nearly in the ſame proportion to one another, which they ought to have by the fifth of theſe Obſervations. of thick tranſparent Plates.

And thus I ſatisfied myſelf that theſe rings were of the ſame kind and original with thoſe of thin plates; and by conſequence that the fits, or alternate diſpoſitions of the rays to be reflected and tranſmitted, are propagated to great diſtances from every reflecting and refracting ſurface. But yet to put the matter out of doubt, I added the following Obſervation.

Obſ. 9. If theſe rings thus depend on the Thickneſs of the plate of glaſs, their diameters at equal diſtances from ſeveral ſpeculums, made of ſuch concavo-convex plates of glaſs as are ground on the ſame ſphere, ought to be reciprocally in a ſubduplicate proportion of the Thickneſſes of the plates of glaſs. And if this proportion be found true by experience, it will amount to a demonſtration, that theſe rings, like thoſe formed in thin plates, do depend on the Thickneſs of the glaſs. I procured therefore another concavo-convex plate of glaſs, ground on both ſides to the ſame ſphere with the former plate. Its thickneſs was $\frac{5}{62}$ parts of an inch; and the diameters of the three firſt bright rings, meaſured between the brighteſt parts of their orbits at the diſtance of ſix feet from the glaſs, were 3. $4\frac{1}{6}$. $5\frac{1}{8}$. inches. Now the thickneſs of the other glaſs, being $\frac{1}{4}$ of an inch, was to the thickneſs of this glaſs as $\frac{1}{4}$ to $\frac{5}{62}$; that is, as 31 to 10, or 310000000 to 100000000; and the roots of theſe numbers are 17607 and 10000; and in the proportion of the firſt of theſe roots to the ſecond, are the diameters of the bright rings made in this Obſervation by the thinner glaſs, 3. $4\frac{1}{6}$. $5\frac{1}{8}$, to the diameters of the ſame rings made in the third of theſe Obſervations by the thicker glaſs, $1\frac{11}{16}$. $2\frac{3}{8}$. $2\frac{11}{12}$; that is, the diameters of the rings are reciprocally in a ſubduplicate proportion of the Thickneſſes of the plates of glaſs.

So then in plates of glaſs which are alike concave on one ſide, and alike convex on the other ſide, and alike quick-ſilvered on the convex ſides, and differ in nothing but their Thickneſs; the

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diameters of the rings are reciprocally in a ſubduplicate proportion of the Thickneſſes of the plates. And this ſhews ſufficiently, that the rings depend on both the ſurfaces of the glaſs. They depend on the Convex ſurface, becauſe they are more luminous when that ſurface is quick-ſilvered over, than when it is without quick-ſilver. They depend alſo upon the Concave ſurface, becauſe without that ſurface a ſpeculum makes them not. They depend on both ſurfaces, and on the diſtances between them, becauſe their bigneſs is varied by varying only that diſtance. And this dependence is of the ſame kind with that, which the colours of thin plates have on the diſtance of the ſurfaces of thoſe plates, becauſe the bigneſs of the rings, and their proportion to one another, and the variation of their bigneſs ariſing from the variation of the Thickneſs of the glaſs, and the Orders of their colours, is ſuch as ought to reſult from the Propoſitions in the end of the third Part of this Book, derived from the phænomena of the colours of thin plates ſet down in the firſt Part.

There are yet other phænomena of theſe rings of colours, but ſuch as follow from the ſame Propoſitions, and therefore confirm both the truth of thoſe Propoſitions, and the analogy between theſe rings and the rings of colours made by very thin plates. I ſhall ſubjoin ſome of them.

Obſ. 10. When the beam of the ſun's light was reflected back from the ſpeculum not directly to the hole in the window, but to a place a little diſtant from it, the common center of that ſpot, and of all the rings of colours fell in the middle way between the beam of the Incident light, and the beam of the Reflected light; and by conſequence in the center of the ſpherical concavity of the ſpeculum; whenever the chart, on which the rings of colours fell, was placed at that center. And as the beam of Reflected light, by inclining the ſpeculum, receded more and more from the beam of Incident light, and from the common center of the coloured rings between them; thoſe rings grew bigger and bigger, and ſo alſo did the White round ſpot; and new rings of colours emerged ſucceſſively out of their common center, and the white ſpot became a white ring encompaſſing them; and the Incident and Reflected beams of light always fell upon the oppo-

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site parts of this white ring, illuminating its perimeter like two mock suns in the opposite parts of an iris. So then the diameter of this ring, measured from the middle of its light on one side to the middle of its light on the other side, was always equal to the distance between the middle of the Incident beam of light, and the middle of the Reflected beam, measured at the chart on which the rings appeared: and the rays, which formed this ring, were reflected by the speculum in angles equal to their angles of incidence, and by consequence to their angles of refraction at their entrance into the glass; but yet their angles of reflexion were not in the same planes with their angles of incidence. of thick transparent Plates.

Obs. 11. The colours of the new rings were in a contrary order to those of the former, and arose after this manner. The white round spot of light in the middle of the rings continued white to the center, till the distance of the incident and reflected beams at the chart was about $\frac{7}{8}$ parts of an inch, and then it began to grow dark in the middle. And when that distance was about $1\frac{3}{16}$ of an inch, the white spot was become a ring, encompassing a dark round spot, which in the middle inclined to Violet and Indigo. And the luminous rings encompassing it were grown equal to those dark ones, which in the four first Observations encompassed them; that is to say, the white spot was grown a white ring equal to the first of those dark rings; and the first of those luminous rings was now grown equal to the second of those dark ones, and the second of those luminous ones to the third of those dark ones, and so on. For the diameters of the luminous rings were now $1\frac{3}{16}$, $2\frac{1}{16}$, $2\frac{2}{3}$, $3\frac{3}{20}$, &c. inches.

When the distance between the incident and reflected beams of light became a little bigger, there emerged out of the middle of the dark spot, after the Indigo, a Blue; and then out of that Blue, a pale Green; and soon after a Yellow and Red. And when the colour at the center was brightest, being between Yellow and Red, the bright rings were grown equal to those rings which in the four first Observations next encompassed them; that is to say, the white spot in the middle of those rings was now become a white ring equal to the first of those bright rings; and the first of those bright ones was now come equal to the second of those,

and

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and ſo on. For the diameters of the white ring, and of the other luminous rings encompaſſing it, were now $1\frac{11}{16}$, $2\frac{3}{8}$, $2\frac{11}{12}$, $3\frac{3}{8}$, &c. or thereabouts.

When the diſtance of the two beams of light at the chart was a little more increaſed, there emerged out of the middle in order after the Red, a Purple, a Blue, a Green, a Yellow, and a Red inclining much to Purple; and when the colour was brighteſt, being between Yellow and Red, the former Indigo, Blue, Green, Yellow and Red, were become an iris, or ring of colours, equal to the firſt of thoſe luminous rings which appeared in the four firſt Obſervations; and the White ring, which was now become the ſecond of the luminous rings, was grown equal to the ſecond of thoſe; and the firſt of thoſe, which was now become the third ring, was become equal to the third of thoſe; and ſo on. For their diameters were $1\frac{11}{16}$, $2\frac{3}{8}$, $2\frac{11}{12}$, $3\frac{3}{8}$ inches, the diſtance of the two beams of light, and the diameter of the white ring being $2\frac{3}{8}$ inches.

When theſe two beams became more diſtant, there emerged out of the middle of the purpliſh Red, firſt a darker round ſpot; and then, out of the middle of that ſpot, a brighter. And now the former colours (Purple, Blue, Green, Yellow, and purpliſh Red) were become a ring equal to the firſt of the bright rings mentioned in the four firſt Obſervations; and the rings about This ring were grown equal to the rings about That reſpectively; the diſtance between the two beams of light and the diameter of the white ring, which was now become the third ring, being about 3 inches.

The colours of the rings in the middle began now to grow very dilute, and if the diſtance between the two beams was increaſed half an inch or an inch more, they vaniſhed; whilſt the White ring, with one or two of the rings next it on either ſide, continued ſtill viſible. But if the diſtance of the two beams of light was ſtill more increaſed, theſe alſo vaniſhed: for the light which, coming from ſeveral parts of the hole in the window, fell upon the ſpeculum in ſeveral angles of incidence, made rings of ſeveral bigneſſes, which diluted and blotted out one another, as I knew by intercepting ſome part of that light. For if I intercepted

cepted that part which was neareſt to the axis of the ſpeculum, the rings would be leſs; if the other part, which was remoteſt from it, they would be bigger. of thick tranſparent Plates.

Obſ. 12. When the colours of the priſm were caſt ſucceſſively on the ſpeculum, that ring, which in the two laſt Obſervations was White, was of the ſame bigneſs in all the colours; but the rings without it were greater in the Green than in the Blue, and ſtill greater in the Yellow, and greateſt in the Red. And, on the contrary, the rings within that White circle were leſs in the Green than in the Blue, and ſtill leſs in the Yellow, and leaſt in the Red. For the angles of reflexion of thoſe rays which made this ring, being equal to their angles of incidence, the fits of every reflected ray, within the glaſs after reflexion, are equal in length and number to their fits of the ſame ray, within the glaſs before its incidence on the reflecting ſurface. And therefore ſince all the rays of all ſorts, at their entrance into the glaſs, were in a fit of Tranſmiſſion, they were alſo in a fit of Tranſmiſſion at their returning to the ſame ſurface after reflexion; and by conſequence were tranſmitted, and went out to the White ring on the chart. This is the reaſon why that ring was of the ſame bigneſs in all the colours, and why in a mixture of all it appears White. But in rays which are reflected in other angles, the interval of the fits of the leaſt refrangible, being greateſt, make the rings of their colour, in their progreſs from this White ring either outwards or inwards, increaſe or decreaſe by the greateſt ſteps; ſo that the rings of this colour without are greateſt, and within leaſt. And this is the reaſon why, in the laſt Obſervation, when the ſpeculum was illuminated with White light, the Exterior rings, made by all colours, appeared Red without and Blue within; and the interior, Blue without and Red within.

Theſe are the phænomena of thick convexo-concave plates of glaſs, which are every where of the ſame thickneſs. There are yet other phænomena, when theſe plates are a little thicker on one ſide than on the other; and others when the plates are more or leſs concave than convex, or plano-convex, or double-convex. For in all theſe caſes the plates make rings of colours, but after various manners; all which, ſo far as I have yet obſerved, follow

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low from the Propoſitions in the end of the third Part of this Book, and ſo conſpire to confirm the truth of thoſe Propoſitions. But the phænomena are too various, and the calculations, whereby they follow from thoſe Propoſitions, too intricate to be here proſecuted. I content myſelf with having proſecuted this kind of phænomena ſo far as to diſcover their cauſe, and by diſcovering it, to ratify the Propoſitions in the third Part of this Book.

Obſ. 13. As light reflected by a lens, quick-ſilvered on the backſide, makes the rings of colours above deſcribed, ſo it ought to make the like rings of colours in paſſing through a drop of water. At the firſt reflexion of the rays within the drop, ſome colours ought to be tranſmitted, as in the caſe of a lens, and others to be reflected back to the eye. For inſtance, if the diameter of a ſmall drop or globule of water be about the 500th part of an inch, ſo that a Red-making ray, in paſſing through the middle of this globule, has 250 fits of eaſy Tranſmiſſion within the globule, and that all the Red-making rays, which are at a certain diſtance from this middle ray round about it, have 249 fits within the globule, and all the like rays, at a certain farther diſtance round about it, have 248 fits, and all thoſe at a certain farther diſtance 247 fits, and ſo on; theſe concentrick circles of rays, after their tranſmiſſion, falling on a white paper, will make concentrick rings of Red upon the paper, ſuppoſing the light which paſſes through one ſingle globule, ſtrong enough to be ſenſible. And, in like manner, the rays of other colours will make rings of other colours. Suppoſe now that in a fair day the ſun ſhines through a thin cloud of ſuch globules of water or hail, and that the globules are all of the ſame bigneſs; and the ſun, ſeen through this cloud, ſhall appear encompaſſed with the like concentrick rings of colours; and the diameter of the firſt ring of Red ſhall be $7\frac{1}{4}$ degrees; that of the ſecond, $10\frac{1}{4}$ degrees; that of the third, 12 degrees 33 minutes. And accordingly as the globules of water are bigger or leſs, the rings ſhall be leſs or bigger. This is the Theory, and experience anſwers it. For in *June* 1692 I ſaw by reflexion, in a veſſel of ſtagnating water, three halos, crowns, or rings of colours about the ſun, like three little rain-bows, concentrick to his body. The colours

Fig. 1.

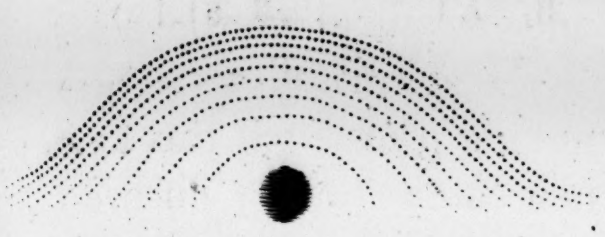


Fig. 2.

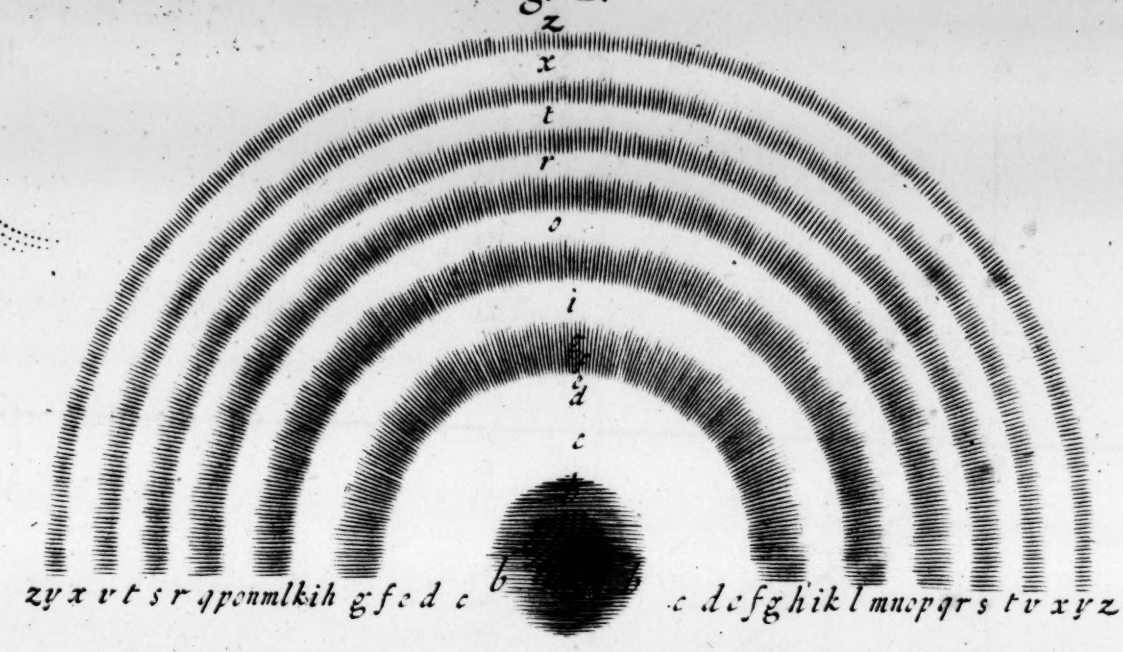


Fig. 3.

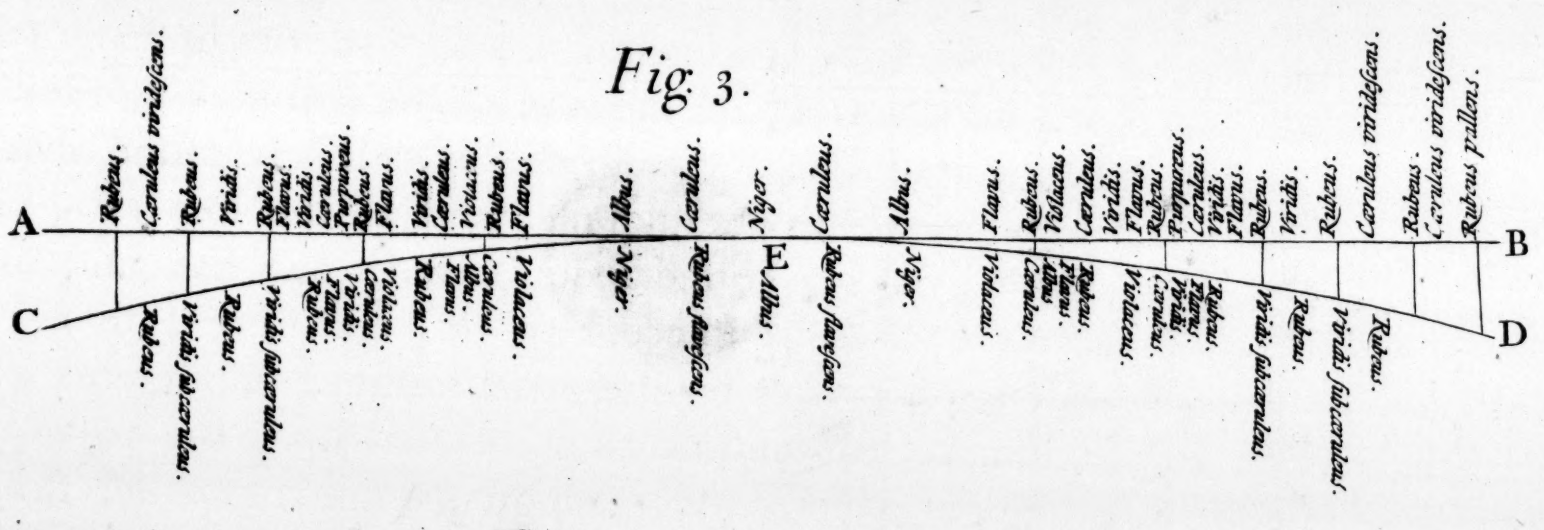
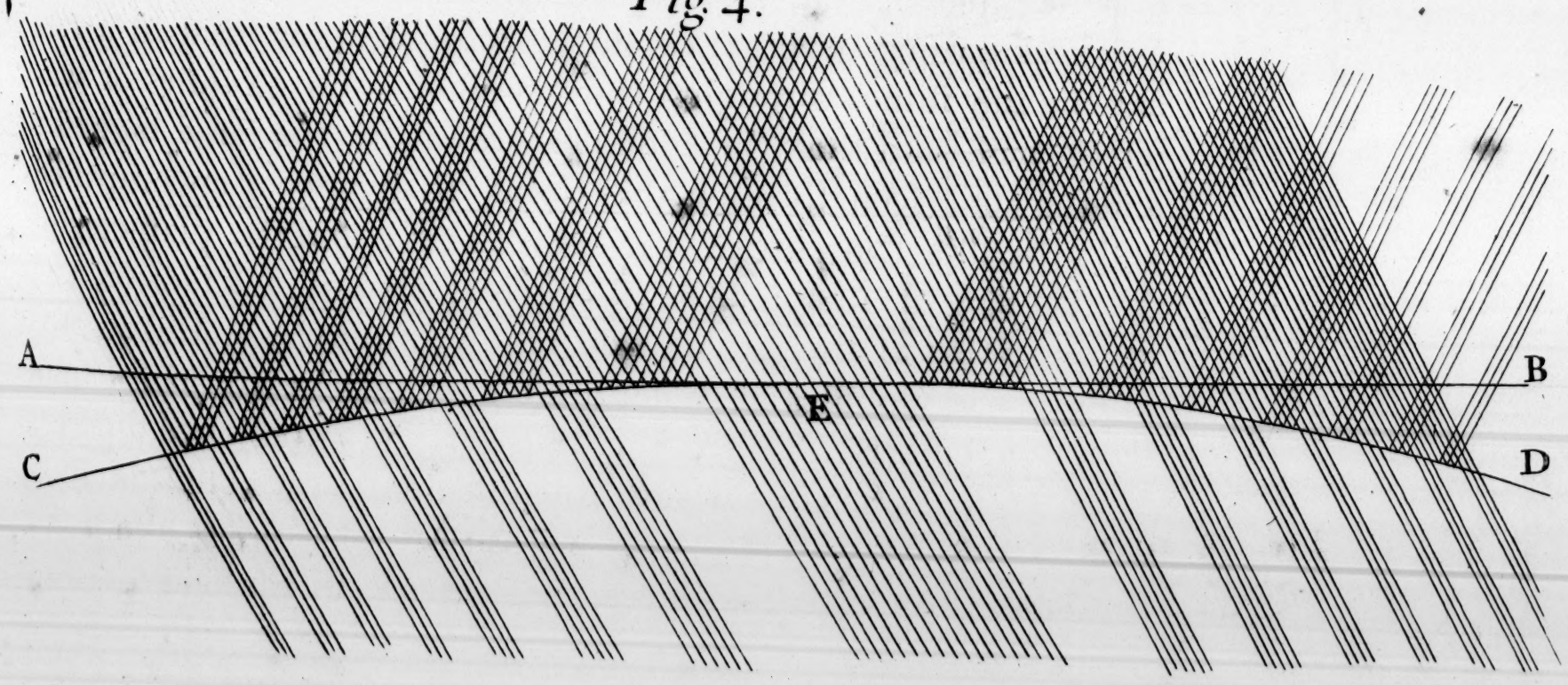


Fig. 4.



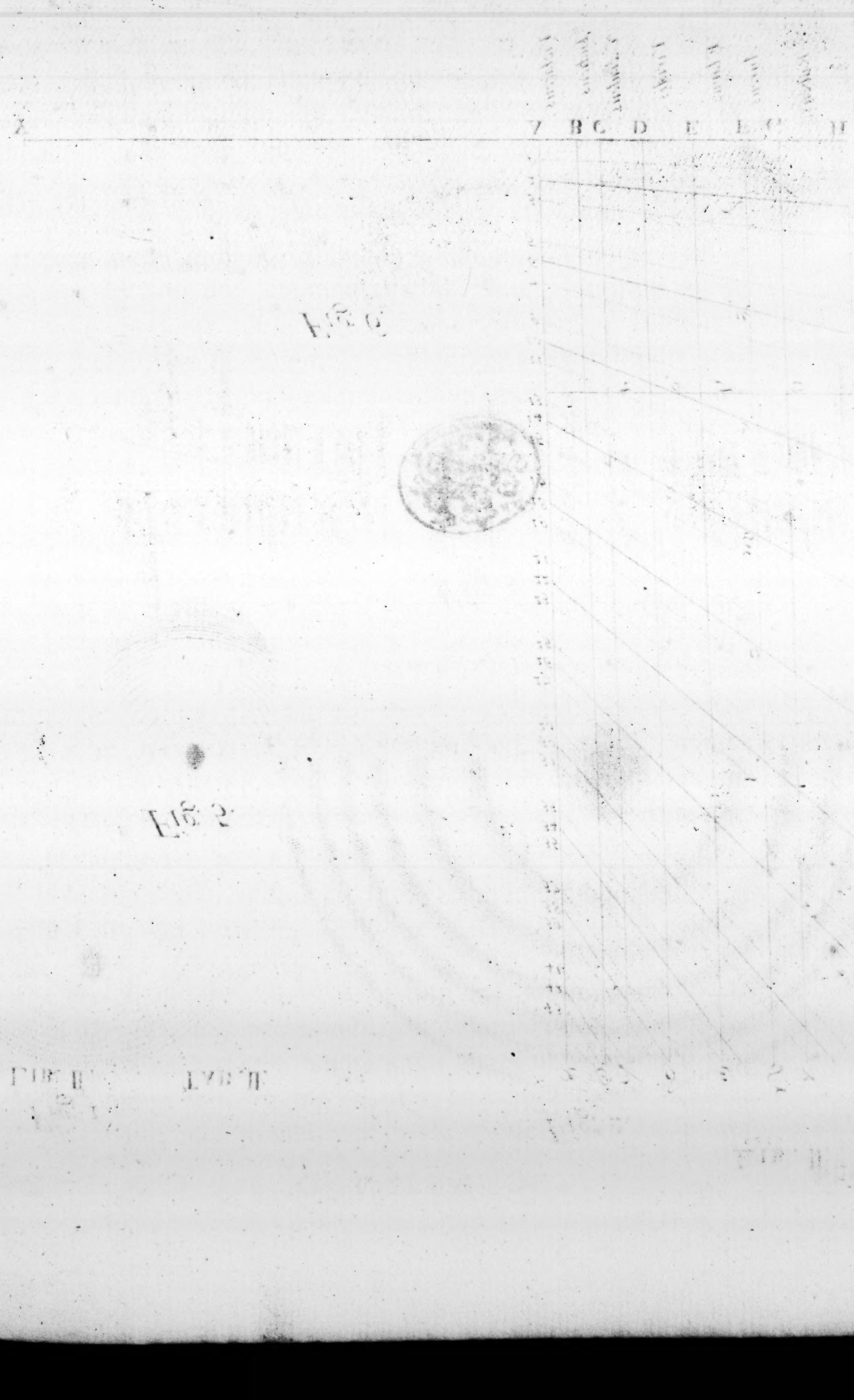


Fig. 5.

Fig. 6.

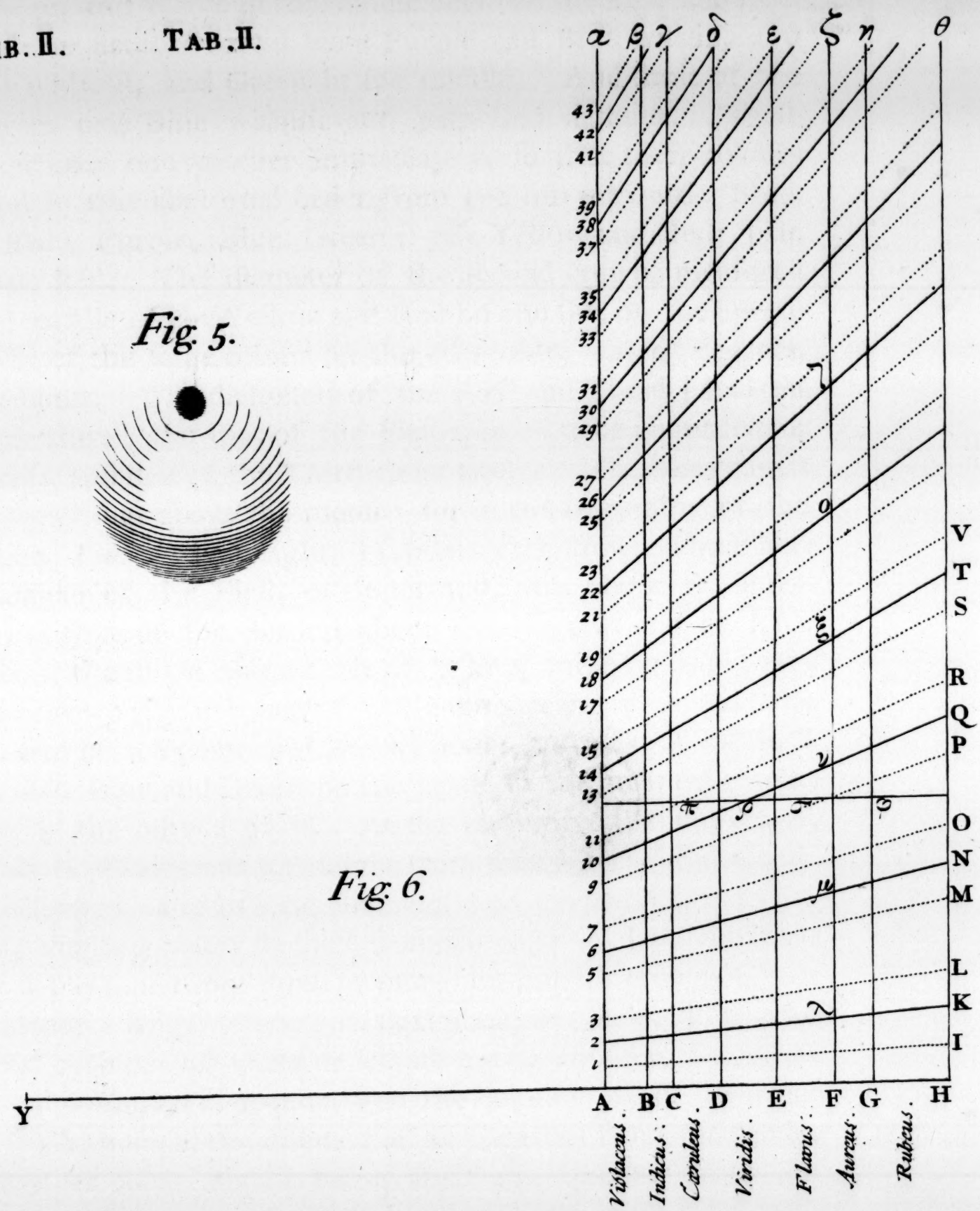
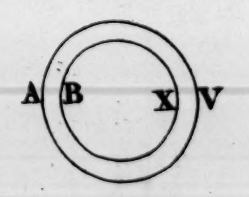
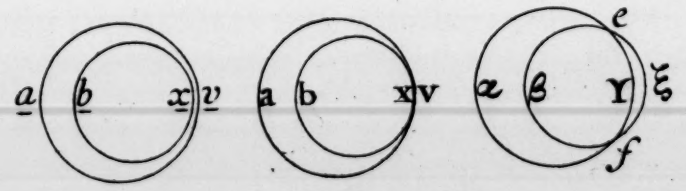


Fig. 7.





colours of the Firſt or innermoſt crown were Blue next the Sun, Red without, and White in the middle between the Blue and Red. Thoſe of the Second crown were Purple and Blue within, and pale Red without, and Green in the middle. And thoſe of the Third were pale Blue within, and pale Red without. Theſe crowns encloſed one another immediately, ſo that their colours proceeded in this continual order from the ſun outward: Blue, White, Red; Purple, Blue, Green; pale Yellow and Red; pale Blue, pale Red. The diameter of the Second crown, meaſured from the middle of the Yellow and Red on one ſide of the Sun to the middle of the ſame colour on the other ſide, was $9\frac{1}{3}$ degrees, or thereabouts. The diameters of the Firſt and Third I had not time to meaſure; but that of the Firſt ſeemed to be about five or ſix degrees, and that of the Third about twelve. The like crowns appear ſometimes about the moon: for in the beginning of the year 1664, *Feb.* 19th at night, I ſaw two ſuch crowns about her. The diameter of the Firſt, or innermoſt, was about three degrees, and that of the Second about five degrees and an half. Next about the moon was a circle of White, and next about that the inner crown; which was of a Blueiſh Green within next the White, and of a Yellow and Red without: and next about theſe colours were Blue and Green on the inſide of the outward crown, and Red on the outſide of it. At the ſame time there appeared a halo about 22 degrees 35′ diſtant from the center of the moon. It was Elliptical, and its long diameter was perpendicular to the horizon, verging below fartheſt from the moon. I am told that the moon has ſometimes three or more concentrick crowns of colours, encompaſſing one another, next about her body. The more equal the globules of water or ice are to one another, the more crowns of colours will appear, and the colours will be the more lively. The halo, at the diſtance of $22\frac{1}{2}$ degrees from the moon, is of another ſort. By its being Oval and remoter from the moon below than above, I conclude, that it was made by refraction, in ſome ſort of hail or ſnow floating in the air in an horizontal poſture; the Refracting angle being about 58 or 60 degrees.

Halos encircling the Sun and Moon.

THE

THIRD BOOK

OF

OPTICKS.

Obſervations concerning the Inflexions of the Rays of Light, and the Colours made thereby.

Inflexion of the

GRIMALDO has informed us, that if a beam of the ſun's light be let into a dark room through a very ſmall hole, the ſhadows of things, in this light, will be larger than they ought to be, if the rays went on by the bodies in ſtrait lines; and that theſe ſhadows have three parallel fringes, bands or ranks, of coloured light adjacent to them. But if the hole be enlarged, the fringes grow broad, and run into one another, ſo that they cannot be diſtinguiſhed. Theſe broad ſhadows and fringes have been reckoned by ſome to proceed from the ordinary refraction of the air, but without due examination of the matter. For the circumſtances of the phænomenon, ſo far as I have obſerved them, are as follows.

Obſ. 1. I made in a piece of lead a ſmall hole with a pin, whoſe breadth was the 42d part of an inch: for 21 of thoſe pins laid together took up the breadth of half an inch. Through this hole,

hole, I let into my darkened chamber a beam of the ſun's light; and found that the ſhadows of hairs, threads, pins, ſtraws, and ſuch like ſlender ſubſtances, placed in this beam of light, were conſiderably broader than they ought to be, if the rays of light paſſed on by theſe bodies in right lines. And particularly a hair of a man's head, whoſe breadth was but the 280th part of an inch, being held in this light, at the diſtance of about twelve feet from the hole, did caſt a ſhadow, which, at the diſtance of four inches from the hair, was the ſixtieth part of an inch broad; that is, above four times broader than the hair: and at the diſtance of two feet from the hair, was about the eight and twentieth part of an inch broad; that is, ten times broader than the hair: and at the diſtance of ten feet, was an eighth part of an inch broad; that is, 35 times broader. Rays of Light.

Nor is it material whether the hair be encompaſſed with air, or with any other pellucid ſubſtance. For I wetted a poliſhed plate of glaſs, and laid the hair in the water upon the glaſs; and then laying another poliſhed plate of glaſs upon it, ſo that the water might fill up the ſpace between the glaſſes, I held them in the aforeſaid beam of light, ſo that the light might paſs through them perpendicularly, and the ſhadow of the hair was, at the ſame diſtances, as big as before. The ſhadows of ſcratches, made in poliſhed plates of glaſs, were alſo much broader than they ought to be; and the veins in poliſhed plates of glaſs, did alſo caſt the like broad ſhadows. And therefore the great breadth of theſe ſhadows proceeds from ſome other cauſe, than the refraction of the air.

Let the circle X [in *fig.* I.] repreſent the middle of the hair; ADG, BEH, CFI, three rays, paſſing by one ſide of the hair at ſeveral diſtances; KNQ, LOR, MPS, three other rays, paſſing by the other ſide of the hair at the like diſtances; D, E, F, and N, O, P, the places where the rays are bent in their paſſage by the hair; G, H, I and Q, R, S, the places where the rays fall on a paper GQ; IS the breadth of the ſhadow of the hair caſt on the paper; and TI, VS, two rays paſſing to the points I and S without bending, when the hair is taken away. And it is manifeſt that all the light between theſe two rays, TI and VS, is bent in

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paſſing by the hair, and turned aſide from the ſhadow is; becauſe if any part of this light were not bent, it would fall on the paper within the ſhadow, and there illuminate the paper, contrary to experience. And becauſe when the paper is at a great diſtance from the hair, the ſhadow is broad, and therefore the rays, TI and VS, are at a great diſtance from one another, it follows that the hair acts upon the rays of light at a good diſtance in their paſſing by it. But the action is ſtrongeſt on the rays, which paſs by at leaſt diſtances; and grows weaker and weaker, accordingly as the rays paſs by at diſtances greater and greater, as is repreſented in the ſcheme: for thence it comes to paſs, that the ſhadow of the hair is much broader, in proportion to the diſtance of the paper from the hair, when the paper is nearer the hair, than when it is at a great diſtance from it.

Obſ. 2. The ſhadows of all bodies (metals, ſtones, glaſs, wood, horn, ice, &c.) in this light were bordered with three parallel fringes or bands of coloured light; whereof that which was contiguous to the ſhadow was broadeſt and moſt luminous; and that which was remoteſt from it was narroweſt; and ſo faint, as not eaſily to be viſible. It was difficult to diſtinguiſh the colours, unleſs when the light fell very obliquely upon a ſmooth paper, or ſome other ſmooth white body; ſo as to make them appear much broader, than they would otherwiſe do. And then the colours were plainly viſible in this order: the Firſt or innermoſt fringe was Violet and deep Blue next the ſhadow; and then light Blue, Green and Yellow in the middle, and Red without. The Second fringe was almoſt contiguous to the firſt, and the Third to the ſecond; and both were Blue within the Yellow, and Red without; but their colours were very faint, eſpecially thoſe of the third. The colours therefore proceeded in this order from the ſhadow; Violet, Indigo, pale Blue, Green, Yellow, Red; Blue, Yellow, Red; pale Blue, pale Yellow and Red. The ſhadows, made by ſcratches and bubbles in poliſhed plates of glaſs, were bordered with the like fringes of coloured light. And if plates of looking-glaſs, ſloped off near the edges with a diamond-cut, be held in the ſame beam of light; the light, which paſſes through the parallel planes of the glaſs, will be bordered with the like

fringes of colours, where thoſe planes meet with the diamond-cut; and by this means there will ſometimes appear four or five fringes of colours. Let AB, CD [in *fig.* 2.] repreſent the parallel planes of a looking-glaſs, and BD the plane of the diamond-cut, making at B a very obtuſe angle with the plane AB. And let all the light, between the rays ENI and FBM, paſs directly through the parallel planes of the glaſs, and fall upon the paper between I and M; and all the light between the rays GO and HD be refracted by the oblique plane of the diamond-cut BD, and fall upon the paper between K and L; and the light which paſſes directly through the parallel planes of the glaſs, and falls upon the paper between I and M, will be bordered with three or more fringes at M. Rays of Light.

So by looking on the ſun through a feather or black ribband held cloſe to the eye, ſeveral rain-bows will appear; the ſhadows, which the fibres or threads caſt on the *Tunica Retina*, being bordered with the like fringes of colours.

Obſ. 3. When the hair was twelve feet diſtant from this hole, and its ſhadow fell obliquely upon a flat white ſcale of inches and parts of an inch, placed half a foot beyond it, and alſo when the ſhadow fell perpendicularly upon the ſame ſcale, placed nine feet beyond it; I meaſured the breadth of the ſhadow and fringes as accurately as I could, and found them in parts of an inch as follows.

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At the diſtance of	half a foot	nine feet
The breadth of the ſhadow	$\frac{1}{34}$	$\frac{1}{9}$
The breadth between the middles of the brighteſt light of the innermoſt fringes on either ſide the ſhadow	$\frac{1}{38}$ or $\frac{1}{39}$	$\frac{7}{30}$
The breadth between the middles of the brighteſt light of the middlemoſt fringes on either ſide the ſhadow	$\frac{1}{23\frac{1}{2}}$	$\frac{4}{17}$
The breadth between the middles of the brighteſt light of the outmoſt fringes on either ſide the ſhadow	$\frac{1}{18}$ or $\frac{1}{18\frac{1}{2}}$	$\frac{3}{10}$
The diſtance between the middles of the brighteſt light of the firſt and ſecond fringes	$\frac{1}{120}$	$\frac{1}{21}$
The diſtance between the middles of the brighteſt light of the ſecond and third fringes	$\frac{1}{170}$	$\frac{1}{31}$
The breadth of the luminous part (Green, White, Yellow and Red) of the firſt fringe	$\frac{1}{170}$	$\frac{1}{32}$
The breadth of the darker ſpace between the firſt and ſecond fringes	$\frac{1}{240}$	$\frac{1}{43}$
The breadth of the luminous part of the ſecond fringe	$\frac{1}{290}$	$\frac{1}{53}$
The breadth of the darker ſpace between th ſecond and third fringes	$\frac{1}{340}$	$\frac{1}{63}$

Theſe meaſures I took, by letting the ſhadow of the hair, at half a foot diſtance, fall ſo obliquely on the ſcale, as to appear twelve times broader, than when it fell perpendicularly on it at the ſame diſtance, and ſetting down in this Table the twelfth part of the meaſures I then took.

Obſ. 4. When the ſhadow and fringes were caſt obliquely upon a ſmooth white body, and that body was removed farther and farther from the hair, the Firſt fringe began to appear, and look brighter than the reſt of the light, at the diſtance of leſs than a quarter of an inch from the hair; and the dark line, or ſhadow, between that and the Second fringe, began to appear at a leſs diſtance from the hair than that of the third part of an inch. The Second fringe began to appear, at a diſtance from the hair of leſs than half an inch; and the ſhadow between that and the third fringe, at a diſtance leſs than an inch; and the Third fringe, at a diſtance leſs than three inches. At greater diſtances they became much more ſenſible, but kept very nearly the ſame proportion of their breadths and intervals, which they had at their firſt appearing. For the diſtance between the middle of the Firſt and the

the middle of the Second fringe, was to the diſtance between the middle of the Second and middle of the Third fringe, as three to two, or ten to ſeven. And the laſt of theſe two diſtances was equal to the breadth of the bright light or luminous part of the Firſt fringe. And this breadth was to the breadth of the bright light of the Second fringe as ſeven to four; and to the dark interval of the Firſt and Second fringe as three to two; and to the like dark interval between the Second and Third, as two to one. For the breadths of the fringes ſeemed to be in the progreſſion of the numbers 1, $\sqrt{\frac{1}{3}}$, $\sqrt{\frac{1}{5}}$, and their intervals to be in the ſame progreſſion with them; that is, the fringes and their intervals together to be in the continual progreſſion of the numbers 1, $\sqrt{\frac{1}{2}}$, $\sqrt{\frac{1}{3}}$, $\sqrt{\frac{1}{4}}$, $\sqrt{\frac{1}{5}}$, or thereabouts. And theſe proportions held the ſame, very nearly, at all diſtances from the hair; the dark intervals of the fringes being as broad, in proportion to the breadth of the fringes, at their firſt appearance, as afterwards at great diſtances from the hair, though not ſo dark and diſtinct. Rays of Light.

Obſ. 5. The ſun ſhining into my darkened chamber, through a hole a quarter of an inch broad; I placed at the diſtance of two or three feet from the hole a ſheet of paſte-board, which was blackened all over on both ſides, and in the middle of it had a hole, about three quarters of an inch ſquare, for the light to paſs through. And behind the hole I faſtened to the paſte-board with pitch the blade of a ſharp knife, to intercept ſome part of the light, which paſſed through the hole. The planes of the paſte-board and blade of the knife were parallel to one another, and perpendicular to the rays. And when they were ſo placed, that none of the ſun's light fell on the paſteboard, but all of it paſſed through the hole to the knife, and there part of it fell upon the blade of the knife, and part of it paſſed by its edge: I let this part of the light, which paſſed by, fall on a white paper two or three feet beyond the knife; and there ſaw two ſtreams of faint light ſhoot out both ways from the beam of light into the ſhadow, like the tails of comets. But becauſe the ſun's direct light, by its brightneſs upon the paper, obſcured theſe faint ſtreams, ſo that I could ſcarce ſee them; I made a little hole in the midſt of the paper, for that light to paſs through, and fall on a black cloth behind

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behind it; and then I ſaw the two ſtreams plainly. They were like one another, and pretty nearly equal in length and breadth, and quantity of light. Their light, at that end next the ſun's direct light, was pretty ſtrong for the ſpace of about a quarter of an inch, or half an inch; and in all its progreſs from that direct light decreaſed gradually, till it became inſenſible. The whole length of either of theſe ſtreams, meaſured upon the paper at the diſtance of three feet from the knife, was about ſix or eight inches; ſo that it ſubtended an angle, at the edge of the knife, of about 10 or 12, or at moſt 14 degrees. Yet ſometimes I thought I ſaw it ſhoot three or four degrees farther; but with a light ſo very faint that I could ſcarce perceive it, and ſuſpected it might (in ſome meaſure at leaſt) ariſe from ſome other cauſe, than the two ſtreams did. For placing my eye in that light, beyond the end of that ſtream which was behind the knife, and looking towards the knife, I could ſee a line of light upon its edge; and that not only when my eye was in the line of the ſtreams, but alſo when it was without that line, either towards the point of the knife, or towards the handle. This line of light appeared contiguous to the edge of the knife, and was narrower than the light of the innermoſt fringe, and narroweſt when my eye was fartheſt from the direct light; and therefore ſeemed to paſs between the light of that fringe and the edge of the knife; and that which paſſed neareſt the edge, to be moſt bent, though not all of it.

Obſ. 6 I placed another knife by this, ſo that their edges might be parallel, and look towards one another; and that the beam of light might fall upon both the knives, and ſome part of it paſs between their edges. And when the diſtance of their edges was about the 400th part of an inch, the ſtream parted in the middle, and left a ſhadow between the two parts. This ſhadow was ſo black and dark, that all the light which paſſed between the knives ſeemed to be bent, and turned aſide to the one hand or to the other. And as the knives ſtill approached one another, the ſhadow grew broader, and the ſtreams ſhorter at their inward ends which were next the ſhadow, until, upon the contact

tact of the knives, the whole light vaniſhed, leaving its place to the ſhadow. Rays of Light.

And hence I gather, that the light which is leaſt bent, and goes to the inward ends of the ſtreams, paſſes by the edges of the knives at the greateſt diſtance; and this diſtance, when the ſhadow begins to appear between the ſtreams, is about the 800th part of an inch. And the light, which paſſes by the edges of the knives at diſtances ſtill leſs and leſs, is more and more bent, and goes to thoſe parts of the ſtreams which are farther and farther from the direct light; becauſe when the knives approach one another till they touch, thoſe parts of the ſtreams vaniſh laſt, which are fartheſt from the direct light.

Obſ. 7. In the fifth Obſervation the fringes did not appear; but, by reaſon of the breadth of the hole in the window, became ſo broad as to run into one another, and, by joining, to make one continued light in the beginning of the ſtreams. But in the ſixth, as the knives approached one another, a little before the ſhadow appeared between the two ſtreams, the fringes began to appear on the inner ends of the ſtreams on either ſide of the direct light; three on one ſide, made by the edge of one knife; and three on the other ſide, made by the edge of the other knife. They were diſtincteſt, when the knives were placed at the greateſt diſtance from the hole in the window; and ſtill became more diſtinct, by making the hole leſs; inſomuch that I could ſometimes ſee a faint lineament of a fourth fringe, beyond the three above-mentioned. And as the knives continually approached one another, the fringes grew diſtincter and larger, until they vaniſhed. The outmoſt fringe vaniſhed firſt, and the middlemoſt next, and the innermoſt laſt. And after they were all vaniſhed, and the line of light which was in the middle between them was grown very broad, enlarging itſelf on both ſides into the ſtreams of light deſcribed in the fifth Obſervation; the above-mentioned ſhadow began to appear in the middle of this line, and divide it along the middle into two lines of light, and increaſed until the whole light vaniſhed. This enlargement of the fringes was ſo great, that the rays, which go to the innermoſt fringe, ſeemed to

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be bent above twenty times more when this fringe was ready to vaniſh, than when one of the knives was taken away.

And from this and the former Obſervation compared, I gather, that the light of the Firſt fringe paſſed by the edge of the knife, at a diſtance greater than the 800th part of an inch; and the light of the Second fringe paſſed by the edge of the knife, at a greater diſtance than the light of the firſt fringe did; and that of the Third, at a greater diſtance than that of the ſecond; and that of the ſtreams of light, deſcribed in the fifth and ſixth Obſervations, paſſed by the edges of the knives at leſs diſtances than that of any of the fringes.

Obſ. 8. I cauſed the edges of two knives to be ground truly ſtrait; and pricking their points into a board, ſo that their edges might look towards one another, and, meeting near their points, contain a rectilinear angle; I faſtened their handles together with pitch, to make this angle invariable. The diſtance of the edges of the knives from one another, at the diſtance of four inches from the angular point, where the edges of the knives met, was the eighth part of an inch; and therefore the angle, contained by the edges, was about 1 degree 54′. The knives, thus fixed together, I placed in a beam of the ſun's light, let into my darkened chamber through a hole the 42d part of an inch wide, at the diſtance of 10 or 15 feet from the hole; and let the light, which paſſed between their edges, fall very obliquely upon a ſmooth white ruler at the diſtance of half an inch, or an inch, from the knives; and there ſaw the fringes, made by the two edges of the knives, run along the edges of the ſhadows of the knives, in lines parallel to thoſe edges, without growing ſenſibly broader; till they met in angles equal to the angle contained by the edges of the knives; and where they met and joined, they ended without croſſing one another. But if the ruler was held at a much greater diſtance from the knives, the fringes, where they were farther from the place of their meeting, were a little narrower, and became ſomething broader and broader, as they approached nearer and nearer to one another; and after they met they croſſed one another, and then became much broader than before.

Whence

Whence I gather, that the diſtances, at which the fringes paſs by the knives, are not increaſed nor altered by the approach of the knives; but the angles, in which the rays are there bent, are much increaſed by that approach; and that the knife, which is neareſt any ray, determines which way the ray ſhall be bent, and the other knife increaſes the bent. Rays of Light.

Obſ. 9. When the rays fell very obliquely upon the ruler, at the diſtance of the third part of an inch from the knives; the dark line between the firſt and ſecond fringe of the ſhadow of one knife, and the dark line between the firſt and ſecond fringe of the ſhadow of the other knife, met with one another at the diſtance of the fifth part of an inch from the end of the light, which paſſed between the knives at the concourſe of their edges. And therefore the diſtance of the edges of the knives, at the meeting of theſe dark lines, was the 160th part of an inch. For as four inches to the eighth part of an inch, ſo is any length of the edges of the knives, meaſured from the point of their concourſe, to the diſtance of the edges of the knives at the end of that length; and ſo is the fifth part of an inch to the 160th part. So then the dark lines above-mentioned meet in the middle of the light, which paſſes between the knives, where they are diſtant the 160th part of an inch; and the one half of that light paſſes by the edge of one knife, at a diſtance not greater than the 320th part of an inch, and, falling upon the paper, makes the fringes of the ſhadow of that knife; and the other half paſſes by the edge of the other knife, at a diſtance not greater than the 320th part of an inch, and, falling upon the paper, makes the fringes of the ſhadow of the other knife. But if the paper be held at a diſtance from the knives greater than the third part of an inch, the dark lines above-mentioned meet at a greater diſtance than the fifth part of an inch from the end of the light, which paſſed between the knives at the concourſe of their edges; and therefore the light, which falls upon the paper where thoſe dark lines meet, paſſes between the knives, where their edges are diſtant above the 160th part of an inch.

For at another time, when the two knives were diſtant eight feet and five inches from the little hole in the window, made with a

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ſmall pin as above; the light which fell upon the paper, where the aforeſaid dark lines met, paſſed between the knives, where the diſtance between their edges was as in the following Table, when the diſtance of the paper from the knives was alſo as follows.

Diſtances of the Paper from the Knives in Inches.	Diſtance between the edges of the Knives in milleſimal parts of an Inch.
$1\frac{1}{2}$.	0'012
$3\frac{1}{3}$.	0'020
$8\frac{2}{5}$.	0'034
32.	0'057
96.	0'081
131.	0'087

And hence I gather that the light, which makes the fringes upon the paper, is not the ſame light at all diſtances of the paper from the knives; but when the paper is held near the knives, the fringes are made by light, which paſſes by the edges of the knives at a leſs diſtance, and is more bent, than when the paper is held at a greater diſtance from the knives.

Obſ. 10. When the fringes of the ſhadows of the knives fell perpendicularly upon a paper, at a great diſtance from the knives, they were in the form of hyperbolas, and their dimenſions were as follows. Let CA, CB [in *fig.* 3.] repreſent lines drawn upon the paper parallel to the edges of the knives, and between which all the light would fall, if it paſſed between the edges of the knives without reflexion; DE, a right line drawn through C, making the angles ACD, BCE, equal to one another, and terminating all the light, which falls upon the paper from the point where the edges of the knives meet; *eis*, *fkt*, and *glv*, three hyperbolical lines, repreſenting the *terminus* of the ſhadow of one of the knives, the dark line between the firſt and ſecond fringes of that ſhadow, and the dark line between the ſecond and third fringes of the ſame ſhadow; *xip*, *ykq* and *zlr*, three other hyperbolical lines, repreſenting the *terminus* of the ſhadow of the other knife, the dark line between the firſt and ſecond fringes of that ſhadow, and the dark line between the ſecond and third fringes of the ſame ſhadow. And conceive that theſe three hyperbolas are like and equal to the former three; and croſs them in the points

points *i*, *k* and *l*; and that the ſhadows of the knives are terminated, and diſtinguiſhed from the firſt luminous fringes, by the lines *eis* and *xip*, until the meeting and croſſing of the fringes; and then thoſe lines croſs the fringes in the form of dark lines, terminating the firſt luminous fringes within ſide, and diſtinguiſhing them from another light, which begins to appear at *i*, and illuminates all the triangular ſpace, *ip*DE*s*, comprehended by theſe dark lines, and the right line DE. Of theſe hyperbolas one aſymptote is the line DE, and the other aſymptotes are parallel to the lines CA and CB. Let *rv* repreſent a line, drawn any where upon the paper, parallel to the aſymptote DE; and let this line croſs the right lines AC in *m*, and BC in *n*, and the ſix dark hyperbolical lines in *p*, *q*, *r*; *s*, *t*, *v*; and by meaſuring the diſtances *ps*, *qt*, *rv*, and thence collecting the lengths of the ordinates *np*, *nq*, *nr*, or *ms*, *mt*, *mv*; and doing this at ſeveral diſtances of the line *rv* from the aſymptote DE; you may find as many points of theſe hyperbolas as you pleaſe, and thereby know that theſe curve lines are hyperbolas differing little from the Conical hyperbola. And by meaſuring the lines c*i*, c*k*, c*l*, you may find other points of theſe curves. Rays of Light.

For inſtance, when the knives were diſtant from the hole in the window ten feet, and the paper from the knives nine feet, and the angle contained by the edges of the knives, to which the angle ACB is equal, was ſubtended by a chord which was to the radius as 1 to 32, and the diſtance of the line *rv* from the aſymptote DE was half an inch: I meaſured the lines *ps*, *qt*, *rv*, and found them 0'35, 0'65, 0'98 inches reſpectively; and by adding to their halfs the line $\frac{1}{2}mn$ (which here was the 128th part of an inch, or 0'0078 inches) the ſums, *np*, *nq*, *nr*, were 0'1828, 0'3328, 0'4978 inches. I meaſured alſo the diſtances of the brighteſt parts of the fringes, which run between *pq* and *st*, *qr* and *tv*, and next beyond *r* and *v*, and found them 0'5, 0'8, and 1'17 inches.

Obſ. 11. The ſun ſhining into my darkened room, through a ſmall round hole made in a plate of lead with a ſlender pin as above; I placed at the hole a priſm to refract the light, and form on the oppoſite wall the ſpectrum of colours, deſcribed in the third

Inflexion of the

third Experiment of the firſt Book. And then I found that the ſhadows of all bodies, held in the coloured light between the priſm and the wall, were bordered with fringes of the colour of that light, in which they were held. In the full Red light they were totally Red, without any ſenſible Blue or Violet; and in the deep Blue light they were totally Blue, without any ſenſible Red or Yellow; and ſo in the Green light they were totally Green, excepting a little Yellow and Blue, which were mixed in the Green light of the priſm. And comparing the fringes made in the ſeveral coloured lights, I found that thoſe made in the Red light were largeſt, thoſe made in the Violet were leaſt, and thoſe made in the Green were of a middle bigneſs. For the fringes with which the ſhadow of a man's hair were bordered, being meaſured croſs the ſhadow, at the diſtance of ſix inches from the hair; the diſtance, between the middle and moſt luminous part of the Firſt or innermoſt fringe on one ſide of the ſhadow, and that of the like fringe on the other ſide of the ſhadow, was, in the full Red light, $\frac{1}{37\frac{1}{2}}$ of an inch; and, in the full Violet, $\frac{1}{46}$. And the like diſtance, between the middle and moſt luminous parts of the Second fringes on either ſide the ſhadow, was, in the full Red light, $\frac{1}{22}$; and, in the Violet, $\frac{1}{27}$ of an inch. And theſe diſtances of the fringes held the ſame proportion, at all diſtances from the hair, without any ſenſible variation.

So then the rays, which made theſe fringes in the Red light, paſſed by the hair at a greater diſtance, than thoſe did which made the like fringes in the Violet; and therefore the hair, in cauſing theſe fringes, acted alike upon the Red light, or leaſt refrangible rays, at a greater diſtance, and upon the Violet, or moſt refrangible rays, at a leſs diſtance; and by thoſe actions diſpoſed the Red light into larger fringes, and the Violet into ſmaller, and the lights of intermediate colours into fringes of intermediate bigneſſes, without changing the colour of any ſort of light.

When therefore the hair, in the firſt and ſecond of theſe Obſervations, was held in the white beam of the Sun's light, and caſt a ſhadow, which was bordered with three fringes of coloured light; thoſe colours aroſe not from any new Modifications impreſſed upon the rays of light by the hair; but only from the various

rious Inflexions, whereby the ſeveral ſorts of rays were ſeparated from one another, which before ſeparation, by the mixture of all their colours, compoſed the white beam of the ſun's light; but whenever ſeparated compoſe lights of the ſeveral colours, which they are originally diſpoſed to exhibit. In this 11th Obſervation, where the colours are ſeparated before the light paſſes by the hair, the leaſt refrangible rays, which when ſeparated from the reſt make Red, were inflected at a greater diſtance from the hair; ſo as to make three Red fringes at a greater diſtance from the middle of the ſhadow of the hair: and the moſt refrangible rays, which when ſeparated make Violet, were inflected at a leſs diſtance from the hair; ſo as to make three Violet fringes, at a leſs diſtance from the middle of the ſhadow of the hair: and other rays, of intermediate degrees of refrangibility, were inflected at intermediate diſtances from the hair; ſo as to make fringes of intermediate colours, at intermediate diſtances from the middle of the ſhadow of the hair. And in the ſecond Obſervation, where all the colours are mixed in the white light which paſſes by the hair, theſe colours are ſeparated by the various Inflexions of the rays; and the fringes, which they make, appear all together; and the Innermoſt fringes, being contiguous, make one broad fringe compoſed of all the colours in due order; the Violet lying on the inſide of the fringe next the ſhadow; the Red, on the outſide fartheſt from the ſhadow; and the Blue, Green and Yellow, in the middle. And, in like manner, the middlemoſt fringes of all the colours lying in order, and being contiguous, make another broad fringe compoſed of all the colours; and the outmoſt fringes of all the colours lying in order, and being contiguous, make a third broad fringe compoſed of all the colours. Theſe are the three fringes of coloured light, with which the ſhadows of all bodies are bordered in the ſecond Obſervation. Rays of Light.

When I made the foregoing Obſervations, I deſigned to repeat moſt of them with more care and exactneſs; and to make ſome new ones, for determining the manner how the rays of light are bent in their paſſage by bodies, for making the fringes of colours with the dark lines between them. But I was then interrupted, and cannot now think of taking theſe things into farther conſi-

deration. And ſince I have not finiſhed this part of my deſign, I ſhall conclude with propoſing only ſome Queries, in order to a farther ſearch to be made by others.

Queries.

Query 1. Do not bodies act upon light at a diſtance; and by their action bend its rays; and is not this action (*cæteris paribus*) ſtrongeſt at the leaſt diſtance?

Qu. 2. Do not the rays which differ in refrangibility differ alſo in flexibility; and are they not by their different inflexions ſeparated from one another, ſo as after ſeparation to make the colours in the three fringes above deſcribed? And after what manner are they inflected to make thoſe fringes?

Qu. 3. Are not the rays of light, in paſſing by the edges and ſides of bodies, bent ſeveral times backwards and forwards, with a motion like that of an eel? And do not the three fringes of coloured light above-mentioned, ariſe from three ſuch bendings?

Qu. 4. Do not the rays of light, which fall upon bodies and are reflected or refracted, begin to bend before they arrive at the bodies; and are they not Reflected, Refracted and Inflected by one and the ſame principle, acting variouſly in various circumſtances?

Qu. 5. Do not bodies and light act mutually upon one another; that is to ſay, bodies upon light in emitting, reflecting, refracting and inflecting it; and light upon bodies, for heating them, and putting their parts into a vibrating motion, wherein heat conſiſts?

Qu. 6. Do not Black bodies conceive heat more eaſily from light than thoſe of other colours do, by reaſon that the light falling on them is not reflected outwards; but enters the bodies, and is often reflected and refracted within them, until it be ſtifled and loſt?

Qu. 7. Is not the ſtrength and vigour of the action between light and Sulphureous bodies obſerved above, one reaſon why Sulphureous bodies take fire more readily, and burn more vehemently, than other bodies do?

Qu.

(a) —— *their Parts*] Nothing of what follows of this eighth Query was in the firſt Edition. From the words *And do not all bodies*, to the words *Oil of anniſeeds*, was added (with ſome ſmall variety noted below) in the firſt Latin edition of Dr. *Clarke*. And the remaining part firſt appeared

Qu. 8. Do not all Fixed bodies, when heated beyond a certain degree, emit light and ſhine; and is not this emiſſion performed by the vibrating motions of their Parts (a). And do not all bodies, which abound with Terreſtrial parts, and eſpecially with Sulphureous ones, emit light, as often as thoſe Parts are ſufficiently agitated; whether that agitation be made by heat, or by friction, or percuſſion, or putrefaction, or by any vital motion, or any other cauſe? As for inſtance; ſea water in a raging ſtorm; quick-ſilver agitated *in Vacuo*; the back of a cat, or neck of a horſe obliquely ſtruck or rubbed in a dark place; wood, fleſh and fiſh, while they putrefy; vapours, ariſing from putrefied waters, uſually called *Ignes Fatui*; ſtacks of moiſt hay, or corn, growing hot by fermentation; glow-worms, and the eyes of ſome animals, by vital motions (b); the vulgar *Phoſphorus* (c) agitated by the attrition of any body, or by the Acid particles of the air; ambar (d), and ſome diamonds by ſtriking, preſſing or rubbing them; ſcrapings of ſteel, ſtruck off with a flint; iron, hammered very nimbly till it become ſo hot as to kindle ſulphur thrown upon it; the axle-trees of chariots taking fire by the rapid rotation of the wheels; and ſome liquors mixed with one another, whoſe particles come together with an impetus, as oil of vitriol diſtilled from its weight of nitre, and then mixed with twice its weight of oil of anniſeeds. So alſo a globe of glaſs about 8 or 10 inches in diameter, being put into a frame where it may be ſwiftly turned round its axis, will in turning ſhine, where it rubs againſt the palm of one's hand applied to it: and if at the ſame time a piece of white paper, or a white cloth, or the end of one's finger, be held at the diſtance of about a quarter of an inch, or half an inch, from that part of the glaſs where it is moſt in motion, the Electrick vapour, which is excited by the friction of the glaſs againſt the hand, will, by daſhing againſt the white paper, cloth or finger, be put into ſuch an agitation as to emit light; and make the white paper, cloth or finger, appear lucid like a glow-

Query VIII. Of the emiſſion of Light by heated bodies.

peared in the ſecond *Engliſh* edition.

(b) — *Glow-worms—by vital motions.*] Not in Dr. *Clarke's* firſt *Latin.*

(c) — *Phoſphorus.*] Phoſphorus Bononienſis, radiis luminis agitatus; Phoſphorus vulgaris, &c. Dr. *Clarke's* firſt *Latin.*

(d) *Ambar.*] Not in Dr. *Clarke's* firſt *Latin.*

Queries ix, x.

worm; and in ruſhing out of the glaſs, will ſometimes puſh againſt the finger ſo as to be felt. And the ſame things have been found by rubbing a long and large cylinder of glaſs or ambar with a paper held in one's hand, and continuing the Friction till the glaſs grew warm.

Of the form of Fire.

Qu. 9. Is not Fire a body heated ſo hot, as to emit light copiouſly? For what elſe is a red hot iron than fire? And what elſe is a burning coal than red hot wood?

—of Flame.

Qu. 10. Is not Flame a vapour, fume or exhalation heated red hot, that is, ſo hot as to ſhine? For bodies do not flame without emitting a copious fume, and this fume burns in the flame. The *Ignis Fatuus* is a vapour ſhining without heat; and is there not the ſame difference between this vapour and flame, as between rotted wood ſhining without heat and burning coals of fire? In diſtilling hot ſpirits, if the head of the ſtill be taken off, the vapour, which aſcends out of the ſtill, will take fire at the flame of a candle, and turn into flame, and the flame will run along the vapour from the candle to the ſtill. Some bodies heated by motion or fermentation, if the heat grow intenſe, fume copiouſly; and if the heat be great enough, the fumes will ſhine, and become flame. Metals in fuſion do not flame for want of a copious fume, except ſpelter, which fumes copiouſly, and thereby flames. All flaming bodies, as oil, tallow, wax, wood, foſſil coals, pitch, ſulphur, by flaming waſte and vaniſh into burning ſmoke; which ſmoke, if the flame be put out, is very thick and viſible, and ſometimes ſmells ſtrongly, but in the flame loſes its ſmell by burning; and, according to the nature of the ſmoke, the flame is of ſeveral colours; as that of ſulphur, blue; that of copper opened with ſublimate, green; that of tallow, yellow; that of camphire, white ([e]). Smoke paſſing through flame cannot but grow red hot; and red hot ſmoke can have no other appearance than that of flame ([f]). When gun-powder takes fire, it goes away into flaming ſmoke. For the charcoal and ſulphur eaſily take fire,

([e]) — *that of Camphire white.*] Not in the firſt edition.

([f]) —*flame.*] What follows of this 10th Query was not in the firſt edition, but appeared in the firſt *Latin* of Dr. *Clarke.*

([g]) — *which ariſe from them.*] What follows of this 11th Query was not in the firſt edition, but

fire, and ſet fire to the nitre; and the ſpirit of the nitre being thereby rarified into vapour, ruſhes out with exploſion much after the manner that the vapour of water ruſhes out of an æolipile; the ſulphur alſo, being volatile, is converted into vapour, and augments the exploſion. And the Acid vapour of the ſulphur (namely, that which diſtills under a bell into oil of ſulphur) entering violently into the Fixed body of the nitre, ſets looſe the ſpirit of the nitre, and excites a greater fermentation; whereby the heat is farther augmented, and the fixed body of the nitre is alſo rarified into fume, and the exploſion is thereby made more vehement and quick. For if ſalt of Tartar be mixed with gun-powder, and that mixture be warmed till it takes fire; the exploſion will be more violent and quick than that of gun-powder alone: which cannot proceed from any other cauſe than the action of the vapour of the gun-powder upon the ſalt of tartar, whereby that ſalt is rarified. The exploſion of gun-powder ariſes therefore from the violent action, whereby all the mixture, being quickly and vehemently heated, is rarified and converted into fume and vapour: which vapour, by the violence of that action, becoming ſo hot as to ſhine, appears in the form of flame. Queries X, XI.

Qu. 11. Do not Great bodies conſerve their heat the longeſt, their parts heating one another; and may not Great denſe and Fixed bodies, when heated beyond a certain degree, emit light ſo copiouſly, as by the emiſſion and re-action of its light, and the reflexions and refractions of its rays within its pores, to grow ſtill hotter, till it comes to a certain period of heat, ſuch as is that of the ſun? And are not the ſun and fixed ſtars great earths vehemently hot; whoſe heat is conſerved by the greatneſs of the bodies, and the mutual action and re-action between them, and the light which they emit; and whoſe parts are kept from fuming away, not only by their Fixity, but alſo by the vaſt weight and denſity of the atmoſpheres incumbent upon them, and very ſtrongly compreſſing them, and condenſing the vapours and exhalations which ariſe from them (g)? For if water be made Formal cauſe of the Sun.

but appeared in the firſt *Latin* of Dr. *Clarke*, except that we find nothing there anſwering to the final words, *and a very ſmall quantity of vapours and exhalations*, which were firſt inſerted in the ſecond *Engliſh*.

Queries xi, xii.

warm in any pellucid veſſel emptied of air, that water in the *vacuum* will bubble and boil as vehemently, as it would in the open air in a veſſel ſet upon the fire till it conceives a much greater heat. For the weight of the incumbent atmoſphere keeps down the vapours, and hinders the water from boiling, until it grow much hotter than is requiſite to make it boil *in Vacuo*. Alſo a mixture of tin and lead, being put upon a red hot iron *in Vacuo*, emits a fume and flame; but the ſame mixture in the open air, by reaſon of the incumbent atmoſphere, does not ſo much as emit any fume, which can be perceived by ſight. In like manner the great weight of the atmoſphere, which lies upon the globe of the ſun, may hinder bodies there from riſing up and going away from the ſun, in the form of vapours and fumes; unleſs by means of a far greater heat, than that which, on the ſurface of the earth, would very eaſily turn them into vapours and fumes. And the ſame great weight may condenſe thoſe vapours and exhalations, as ſoon as they ſhall at any time begin to aſcend from the ſun, and make them preſently fall back again into him; and by that action increaſe his heat, much after the manner that in our earth the air increaſes the heat of a culinary fire. And the ſame weight may hinder the globe of the ſun from being diminiſhed, unleſs by the emiſſion of light, and a very ſmall quantity of vapours and exhalations.

Of the ſenſe of Sight.

Qu. 12. Do not the rays of light, in falling upon the bottom of the eye, excite vibrations in the *Tunica Retina*? Which vibrations, being propagated along the ſolid fibres of the Optick nerves into the brain, cauſe the ſenſe of ſeeing. For becauſe denſe bodies conſerve their heat a long time, and the denſeſt bodies conſerve their heat the longeſt; the vibrations of their parts are of a laſting nature, and therefore may be propagated along ſolid fibres of uniform denſe matter to a great diſtance, for conveying into the brain the impreſſions made upon all the organs of ſenſe. For that motion, which can continue long in one and the ſame part of a body, can be propagated a long way from one part to another, ſuppoſing the body homogeneal, ſo that the motion may not be reflected, refracted, interrupted or diſordered by any unevenneſs of the body.

Qu.

Qu. 13. Do not ſeveral ſorts of rays make vibrations of ſeveral bigneſſes; which according to their bigneſſes excite ſenſations of ſeveral colours: much after the manner that the vibrations of the air, according to their ſeveral bigneſſes, excite ſenſations of ſeveral Sounds? And particularly do not the moſt refrangible rays excite the ſhorteſt vibrations, for making a ſenſation of deep Violet; the leaſt refrangible, the largeſt, for making a ſenſation of deep Red; and the ſeveral intermediate ſorts of rays, vibrations of ſeveral intermediate bigneſſes, to make ſenſations of the ſeveral intermediate colours? Queries XIII, XIV, XV. Of the perception of different colours.

Qu. 14. May not the harmony and diſcord of colours, ariſe from the proportions of the vibrations propagated through the fibres of the Optick nerves into the brain; as the harmony and diſcord of ſounds ariſe from the proportions of the vibrations of the air? For ſome colours, if they be viewed together, are agreeable to one another, as thoſe of gold and Indigo, and others diſagree. Harmony of colours.

Qu. 15. Are not the ſpecies of objects, ſeen with both eyes, united where the Optick nerves meet before they come into the brain; the fibres on the right ſide of both nerves uniting there, and after union going thence into the brain, in the nerve which is on the right ſide of the head; and the fibres on the left ſide of both nerves uniting in the ſame place, and after union going into the brain, in the nerve which is on the left ſide of the head; and theſe two nerves meeting in the brain, in ſuch a manner that their fibres make but one entire ſpecies or picture; half of which, on the right ſide of the ſenſorium, comes from the right ſide of both eyes, through the right ſide of both Optick nerves, to the place where the nerves meet, and from thence on the right ſide of the head into the brain; and the other half, on the left ſide of the ſenſorium, comes in like manner from the left ſide of both eyes. For the Optick nerves of ſuch animals, as look the ſame way with both eyes, as of men, dogs, ſheep, oxen, &c. meet before they come into the brain; but the Optick nerves of ſuch animals as do not look the ſame way with both eyes, as of fiſhes and of the chameleon, do not meet, if I am rightly informed. Reaſon of ſingle Viſion.

Queries XVI, XVII. Duration of the vibrations of the Optic Nerve, which produce the ſenſe of Sight.

Qu. 16. When a man in the dark preſſes either corner of his eye with his finger, and turns his eye away from his finger, he will ſee a circle of colours like thoſe in the feather of a peacock's tail. If the eye and the finger remain quiet, theſe colours vaniſh in a ſecond minute of time; but if the finger be moved with a quavering motion, they appear again ([h]). Do not theſe colours ariſe from ſuch motions, excited in the bottom of the eye, by the preſſure and motion of the finger, as at other times are excited there by light for cauſing viſion? And do not the motions once excited continue about a ſecond of time before they ceaſe ([i])? And when a man by a ſtroke upon his eyes ſees a flaſh of light, are not the like motions excited in the *Retina* by the ſtroke ([k])? And when a coal of fire, moved nimbly in the circumference of a circle, makes the whole circumference appear like a circle of fire; is it not becauſe the motions excited in the bottom of the eye, by the rays of light, are of a laſting nature; and continue till the coal of fire, in going round, returns to its former place? And conſidering the laſtingneſs of the motions excited in the bottom of the eye by light, are they not of a vibrating nature?

Efficient cauſe of the fits of Tranſmiſſion and Reflexion.

Qu. 17. If a ſtone be thrown into ſtagnating water, the waves excited thereby continue ſome time to ariſe, in the place where the ſtone fell into the water, and are propagated from thence in concentrick circles, upon the ſurface of the water, to great diſtances. And the vibrations or tremors excited in the air by percuſſion, continue a little time to move from the place of percuſſion, in concentrick ſpheres, to great diſtances. And in like manner, when a ray of light falls upon the ſurface of any pellucid body, and is there refracted or reflected: may not waves of vibrations, or tremors, be thereby excited in the refracting or reflecting medium at the point of incidence; and continue to ariſe there, and to be propagated from thence, as long as they continue to ariſe and be propagated, when they are excited in the bottom of the eye by the preſſure or motion of the finger, or by the light which comes from the coal of fire in experiments above-mentioned?

([h]) *If the eye—appear again.*] Not in the firſt edition, nor in Dr. *Clarke's* firſt *Latin.*
([i]) *And do not—before they ceaſe.*] Not in the firſt edition, nor in Dr. *Clarke's* firſt *Latin.*
([k]) *—Retina by the ſtroke.*] With theſe words the Book ends in the firſt edition. The remaining

mentioned? And are not these vibrations propagated from the point of incidence to great distances? And do they not overtake the rays of light, and by overtaking them successively, do they not put them into the fits of easy Reflexion and easy Transmission described above? For if the rays endeavour to recede from the densest part of the vibration, they may be alternately accelerated and retarded by the vibrations overtaking them. Queries XVIII, XIX.

Qu. 18. If in two large tall cylindrical vessels of glass inverted, two little thermometers be suspended so as not to touch the vessels, and the air be drawn out of one of these vessels, and these vessels, thus prepared, be carried out of a cold place into a warm one; the thermometer *in Vacuo* will grow warm as much, and almost as soon as the thermometer which is not *in Vacuo*: and when the vessel is carried back into the cold place, the thermometer *in Vacuo* will grow cold, almost as soon as the other thermometer. Is not the heat of the warm room conveyed through the *Vacuum* by the vibrations of a much subtiler medium than air, which, after the air was drawn out, remained in the *Vacuum*? And is not this medium the same with that medium by which light is refracted or reflected, and by whose vibrations light communicates heat to bodies, and is put into fits of easy Reflexion and easy Transmission? And do not the vibrations of this medium, in hot bodies, contribute to the intenseness and duration of their heat? And do not hot bodies communicate their heat to contiguous cold ones, by the vibrations of this medium propagated from them into the cold ones? And is not this medium exceedingly more rare and subtile than the air, and exceedingly more elastick and active? And doth it not readily pervade all bodies? And is it not (by its elastick force) expanded through all the heavens? Probability of a Medium more subtile than Air;

Qu. 19. Doth not the refraction of light proceed from the different density of this æthereal medium in different places, the light receding always from the denser parts of the medium? And is not the density thereof greater in free and open spaces, which may be the mechanical efficient of refraction.

ing paragraph of this Query, and the eight Queries that immediately follow, namely, the 17th, 18th, 19th, 20th, 21st, 22d, 23d, 24th, first appeared in the second *English* edition, and are not found in Dr. *Clarke's* first *Latin*.

void

Queries xx, xxi.

void of air and other groſſer bodies, than within the pores of water, glaſs, cryſtal, gems, and other compact bodies? For when light paſſes through glaſs or cryſtal, and, falling very obliquely upon the farther ſurface thereof, is totally reflected; the total reflexion ought to proceed rather from the Denſity and Vigour of the medium without and beyond the glaſs, than from the Rarity and Weakneſs thereof.

This medium denſer in empty ſpaces than within denſe bodies.

Qu. 20. Doth not this æthereal medium, in paſſing out of water, glaſs, cryſtal, and other compact and denſe bodies into empty ſpaces, grow denſer and denſer by degrees; and by that means refract the rays of light not in a point, but by bending them gradually in curve lines? And doth not the gradual condenſation of this medium extend to ſome diſtance from the bodies; and thereby cauſe the inflexions of the rays of light, which paſs by the edges of denſe bodies, at ſome diſtance from the bodies?

The mechanical efficient of Gravity.

Qu. 21. Is not this medium much rarer within the denſe bodies of the ſun, ſtars, planets and comets, than in the empty celeſtial ſpaces between them? And in paſſing from them to great diſtances, doth it not grow denſer and denſer perpetually; and thereby cauſe the Gravity of thoſe great bodies towards one another, and of their parts towards the bodies; every body endeavouring to go from the denſer parts of the medium towards the rarer? For if this medium be rarer within the ſun's body than at its ſurface, and rarer there than at the hundredth part of an inch from its body, and rarer there than at the fiftieth part of an inch from its body, and rarer there than at the orb of *Saturn*; I ſee no reaſon why the increaſe of denſity ſhould ſtop any where, and not rather be continued through all diſtances from the ſun to *Saturn*, and beyond. And though this increaſe of denſity may at great diſtances be exceeding ſlow, yet if the Elaſtick force of this medium be exceeding great, it may ſuffice to impel bodies from the denſer parts of the medium towards the rarer, with all that power which we call Gravity. And that the Elaſtick force of this medium is exceeding great, may be gathered from the ſwiftneſs of its vibrations. Sounds move about 1140 *Engliſh* feet in a ſecond minute of time; and in ſeven or eight minutes of time, they move about one hundred *Engliſh* miles. Light moves from

from the ſun to us in about ſeven or eight minutes of time; which diſtance is about 70000000 *Engliſh* miles, ſuppoſing the horizontal parallax of the ſun to be about 12″. And the vibrations or pulſes of this medium, that they may cauſe the alternate fits of eaſy Tranſmiſſion and eaſy Reflexion, muſt be ſwifter than light; and by conſequence above 700000 times ſwifter than ſounds. And therefore the Elaſtick force of this medium, in proportion to its denſity, muſt be above 700000 × 700000 (that is, above 490000000000) times greater than the Elaſtick force of the air is in proportion to its denſity. For the velocities of the pulſes of elaſtick mediums are in a ſubduplicate *ratio* of the elaſticities and the rarities of the mediums taken together.

Queries XXI, XXII.

As attraction is ſtronger in ſmall magnets than in great ones, in proportion to their bulk; and gravity is greater in the ſurfaces of ſmall planets than in thoſe of great ones, in proportion to their bulk; and ſmall bodies are agitated much more by Electric Attraction than great ones; ſo the ſmallneſs of the rays of light may contribute very much to the power of the agent, by which they are refracted. And ſo if any one ſhould ſuppoſe that *æther*, like our air, may contain particles which endeavour to recede from one another (for I do not know what this *æther* is) and that its particles are exceedingly ſmaller than thoſe of air, or even than thoſe of light: the exceeding ſmallneſs of its particles may contribute to the greatneſs of the force, by which thoſe particles may recede from one another; and thereby make that medium exceedingly more rare and Elaſtick than air, and by conſequence exceedingly leſs able to reſiſt the motions of projectiles, and exceedingly more able to preſs upon groſs bodies, by endeavouring to expand itſelf.

Qu. 22. May not planets and comets, and all groſs bodies, perform their motions more freely, and with leſs reſiſtance in this æthereal medium than in any fluid, which fills all ſpace adequately without leaving any pores, and by conſequence is much denſer than quick-ſilver or gold? And may not its reſiſtance be ſo ſmall as to be inconſiderable? For inſtance; if this *æther* (for ſo I will call it) ſhould be ſuppoſed 700000 times more Elaſtick than our air, and above 700000 times more rare; its reſiſtance

Small reſiſtance of the æthereal medium.

Queries XXIII, XXIV.

would be above 600000000 times leſs than that of water. And ſo ſmall a reſiſtance would ſcarce make any ſenſible alteration in the motions of the planets in ten thouſand years. If any one would aſk how a medium can be ſo rare, let him tell me how the air, in the upper parts of the atmoſphere, can be above an hundred thouſand times rarer than gold. Let him alſo tell me, how an Electrick body can by friction emit an exhalation ſo rare and ſubtle, and yet ſo potent, as by its emiſſion to cauſe no ſenſible diminution of the weight of the electrick body; and to be expanded through a ſphere, whoſe diameter is above two feet, and yet to be able to agitate and carry up leaf-copper, or leaf-gold, at the diſtance of above a foot from the electrick body? And how the *effluvia* of a magnet can be ſo rare and ſubtile, as to paſs through a plate of glaſs without any reſiſtance or diminution of their force, and yet ſo potent as to turn a magnetick needle beyond the glaſs?

The æthereal medium the mechanical efficient of Viſion.

Qu. 23. Is not Viſion performed chiefly by the vibrations of this medium, excited in the bottom of the eye by the rays of light, and propagated through the ſolid, pellucid and uniform *Capillamenta* of the Optick nerves into the place of ſenſation? And is not Hearing performed by the vibrations either of this or ſome other medium, excited in the Auditory nerves by the tremors of the air, and propagated through the ſolid, pellucid and uniform *Capillamenta* of thoſe nerves into the place of ſenſation? And ſo of the other ſenſes.

—of Animal motion.

Qu. 24. Is not Animal motion performed by the vibrations of this medium, excited in the brain by the power of the will, and propagated from thence through the ſolid, pellucid and uniform *Capillamenta* of the nerves into the muſcles, for contracting and dilating them? I ſuppoſe that the *Capillamenta* of the nerves are each of them Solid and Uniform, that the vibrating motion of the æthereal medium may be propagated along them from one end to the other uniformly, and without interruption: for obſtructions in the nerves create palſies. And that they may be ſufficiently uniform, I ſuppoſe them to be pellucid when viewed ſingly; though the reflexions in their cylindrical ſurfaces may make the whole nerve, compoſed of many *Capillamenta*, appear Opake and White.

White. For Opacity arises from Reflecting surfaces, such as may disturb and interrupt the motions of this medium. Query xxv.

Qu. 25. Are there not other original properties of the rays of light, besides those already described? An instance of another original property we have in the refraction of Island Crystal, described first by *Erasmus Bartholine*, and afterwards more exactly by *Hugenius*, in his Book *De la Lumiere*. This crystal is a pellucid fissile stone, clear as water or crystal of the rock, and without colour; enduring a red heat without losing its transparency, and, in a very strong heat, calcining without fusion: steeped a day or two in water, it loses its natural polish: being rubbed on cloth, it attracts pieces of straws and other light things, like ambar or glass; and with *aqua fortis* it makes an ebullition. It seems to be a sort of Talk, and is found in form of an oblique parallelopiped, with six parallelogram sides and eight solid angles. The obtuse angles of the parallelograms are each of them 101 degrees and 52 minutes; the acute ones 78 degrees and 8 minutes. Two of the solid angles opposite to one another, as C and E, are encompassed each of them with three of these Obtuse angles; and each of the other six, with one Obtuse and two Acute ones. It cleaves easily in planes parallel to any of its sides, and not in any other planes. It cleaves with a glossy polite surface, not perfectly plane, but with some little unevenness. It is easily scratched, and, by reason of its softness, it takes a polish very difficultly. It polishes better upon polished looking-glass than upon metal; and perhaps better upon piteh, leather or parchment. Afterwards it must be rubbed with a little oil or white of an egg, to fill up its scratches; whereby it will become very transparent and polite. But for several experiments, it is not necessary to polish it. If a piece of this crystalline stone be laid upon a book; every letter of the book, seen through it, will appear double, by means of a double refraction. And if any beam of light falls either perpendicularly, or in any oblique angle, upon any surface of this crystal; it becomes divided into two beams by means of the same double refraction. Which beams are of the same colour with the incident beam of light, and seem equal to one another in the Quantity of their

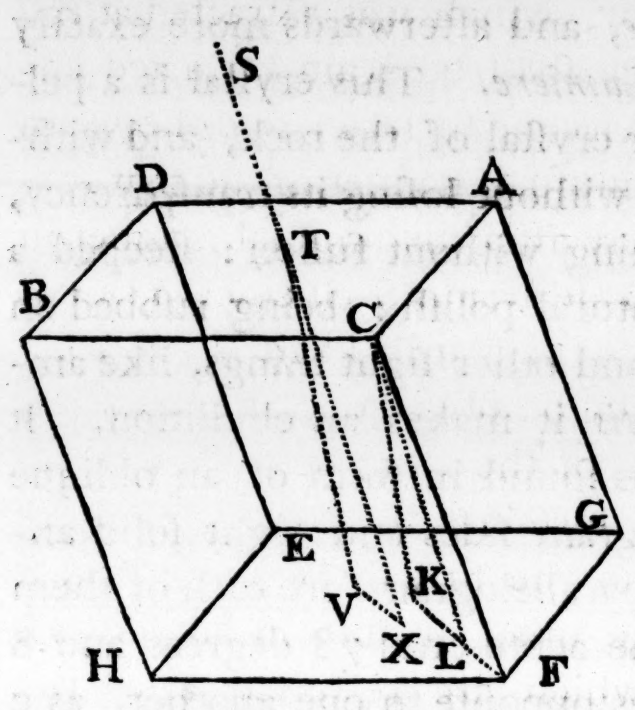
Original properties of rays of Light.

Island Crystal.

See the following Scheme, p. 228.

Query xxv. Iſland Cryſtal.

light, or very nearly equal. One of theſe refractions is performed by the uſual rule of Opticks; the ſine of incidence out of air into this cryſtal being to the ſine of refraction as five to three. The other refraction, which may be called the unuſual refraction, is performed by the following Rule.



Let ADBC repreſent the Refracting ſurface of the cryſtal; C, the biggeſt ſolid angle at that ſurface; GEHF, the oppoſite ſurface; and CK, a perpendicular on that ſurface. This perpendicular makes with the edge of the cryſtal CF, an angle of 19 deg. 3′. Join KF; and in it take KL, ſo that the angle KCL be 6 deg. 40′. and the angle LCF 12 deg. 23′. And if ST repreſent any beam of light incident at T, in any angle, upon the Refracting ſurface ADBC, let TV be the refracted beam determined by the given proportion of the ſines 5 to 3, according to the uſual rule of Opticks. Draw VX parallel and equal to KL. Draw it the ſame way from V, in which L lieth from K; and joining TX, this line TX ſhall be the other refracted beam, carried from T to X, by the unuſual refraction.

If therefore the incident beam ST be perpendicular to the Refracting ſurface, the two beams TV and TX, into which it ſhall become divided, ſhall be parallel to the lines CK and CL; one of thoſe beams going through the cryſtal perpendicularly, as it ought to do by the uſual laws of Opticks; and the other, TX, by an unuſual refraction, diverging from the perpendicular, and making with it an angle VTX of about $6\frac{2}{3}$ degrees, as is found by experience. And hence the plane VTX, and ſuch like planes, which are parallel to the plane CFK, may be called the planes of Perpendicular refraction. And the coaſt, towards which the lines KL and VX are drawn, may be called the coaſt of an unuſual refraction.

In

In like manner cryſtal of the rock has a double refraction: but the differences of the two refractions is not ſo great and manifeſt as in iſland cryſtal. Queries XXV, XXVI.

When the beam ST, incident on iſland cryſtal, is divided into two beams, TV and TX, and theſe two beams arrive at the farther ſurface of the glaſs; the beam TV, which was refracted at the firſt ſurface after the uſual manner, ſhall be again refracted entirely after the uſual manner at the ſecond ſurface; and the beam TX, which was refracted after the unuſual manner in the firſt ſurface, ſhall be again refracted entirely after the unuſual manner in the ſecond ſurface; ſo that both theſe beams ſhall emerge out of the ſecond ſurface in lines parallel to the firſt incident beam ST. Iſland Cryſtal.

And if two pieces of iſland cryſtal be placed one after another, in ſuch manner that all the ſurfaces of the latter be parallel to all the correſponding ſurfaces of the former: the rays, which are refracted after the uſual manner in the firſt ſurface of the firſt cryſtal, ſhall be refracted after the uſual manner in all the following ſurfaces; and the rays, which are refracted after the unuſual manner in the firſt ſurface, ſhall be refracted after the unuſual manner in all the following ſurfaces. And the ſame thing happens, though the ſurfaces of the cryſtals be any ways inclined to one another, provided that their planes of Perpendicular refraction be parallel to one another.

And therefore there is an original difference in the rays of light, by means of which ſome rays are in this experiment conſtantly refracted after the uſual manner, and others conſtantly after the unuſual manner: for if the difference be not original, but ariſes from new modifications impreſſed on the rays at their firſt refraction, it would be altered by new modifications in the three following refractions; whereas it ſuffers no alteration, but is conſtant, and has the ſame effect upon the rays in all the refractions. The unuſual refraction is therefore performed by an Original Property of the rays. And it remains to be enquired, whether the rays have not more Original Properties than are yet diſcovered?

Qu. 26. Have not the rays of light ſeveral ſides, endued with ſeveral original Properties? For if the planes of Perpendicular refraction of the ſecond cryſtal, be at right angles with the planes of The rays of Light have ſeveral ſides.

Query xxiv. Rays of Light

of Perpendicular refraction of the firſt cryſtal; the rays, which are refracted after the uſual manner in paſſing through the firſt cryſtal, will be all of them refracted after the unuſual manner in paſſing through the ſecond cryſtal; and the rays, which are refracted after the unuſual manner in paſſing through the firſt cryſtal, will be all of them refracted after the uſual manner in paſſing through the ſecond cryſtal. And therefore there are not two ſorts of rays differing in their nature from one another, one of which is conſtantly and in all poſitions refracted after the uſual manner, and the other conſtantly and in all poſitions after the unuſual manner. The difference between the two ſorts of rays, in the Experiment mentioned in the 25th Queſtion, was only in the poſitions of the ſides of the rays to the planes of Perpendicular refraction. For one and the ſame ray is here refracted ſometimes after the uſual, and ſometimes after the unuſual manner, according to the poſition which its ſides have to the cryſtals. If the ſide of the ray is poſited the ſame way to both cryſtals, it is refracted after the ſame manner in them both: but if that ſide of the ray, which looks towards the coaſt of the unuſual refraction of the firſt cryſtal, be 90 degrees from that ſide of the ſame ray, which looks towards the coaſt of the unuſual refraction of the ſecond cryſtal (which may be effected by varying the poſition of the ſecond cryſtal to the firſt, and by conſequence to the rays of light) the ray ſhall be refracted after ſeveral manners in the ſeveral cryſtals. There is nothing more required to determine whether the rays of light, which fall upon the ſecond cryſtal, ſhall be refracted after the uſual or after the unuſual manner; but to turn about this cryſtal, ſo that the coaſt of this cryſtal's unuſual refraction may be on this or on that ſide of the ray. And therefore every ray may be conſidered as having four ſides or quarters; two of which, oppoſite to one another, incline the ray to be refracted after the unuſual manner, as often as either of them are turned towards the coaſt of unuſual refraction; and the other two, whenever either of them are turned towards the coaſt of unuſual refraction, do not incline it to be otherwiſe refracted than after the uſual manner. The two firſt may therefore be called the ſides of unuſual refraction. And ſince theſe diſpoſitions were

in the rays, before their incidence on the ſecond, third and fourth ſurfaces of the two cryſtals; and ſuffered no alteration, ſo far as appears, by the refraction of the rays in their paſſage through thoſe ſurfaces; and the rays were refracted by the ſame laws in all the four ſurfaces: it appears, that thoſe diſpoſitions were in the rays originally, and ſuffered no alteration by the firſt refraction; and that by means of thoſe diſpoſitions the rays were refracted at their incidence on the firſt ſurface of the firſt cryſtal; ſome of them after the uſual, and ſome of them after the unuſual manner, accordingly as their ſides of unuſual refraction were then turned towards the coaſt of the unuſual refraction of that cryſtal, or ſideways from it. Queries XXVI XXVII. have ſeveral ſides.

Every ray of light has therefore two oppoſite ſides, originally endued with a property on which the unuſual refraction depends, and with other two oppoſite ſides not endued with that property. And it remains to be enquired, whether there are not more properties of light by which the ſides of the rays differ, and are diſtinguiſhed from one another.

In explaining the difference of the ſides of the rays abovementioned, I have ſuppoſed that the rays fall perpendicularly on the firſt cryſtal. But if they fall obliquely on it, the ſucceſs is the ſame. Thoſe rays which are refracted after the uſual manner in the firſt cryſtal, will be refracted after the unuſual manner in the ſecond cryſtal, ſuppoſing the planes of Perpendicular refraction to be at right angles with one another, as above; and on the contrary.

If the planes of the Perpendicular refraction of the two cryſtals be neither parallel nor perpendicular to one another, but contain an Acute angle: the two beams of light which emerge out of the firſt cryſtal, will be each of them divided into two more at their incidence on the ſecond cryſtal. For in this caſe the rays, in each of the two beams, will ſome of them have their ſides of unuſual refraction, and ſome of them their other ſides, turned toward the coaſt of the unuſual refraction of the ſecond cryſtal.

Qu. 27. Are not all hypotheſes erroneous which have hitherto been invented for explaining the phænomena of light, by new modifications of the rays? For thoſe phænomena depend not upon new

Query XXVIII.

new modifications, as has been ſuppoſed, but upon the Original and Unchangeable properties of the rays.

Light conſiſts not in preſſion, or motion, propagated through a fluid Medium.

Qu. 28. Are not all hypotheſes erroneous, in which light is ſuppoſed to conſiſt in preſſion or motion, propagated through a fluid medium? For in all theſe hypotheſes, the phænomena of light have been hitherto explained, by ſuppoſing that they ariſe from new modifications of the rays; which is an erroneous ſuppoſition.

If light conſiſted only in preſſion propagated without actual motion; it would not be able to agitate and heat the bodies, which refract and reflect it. If it conſiſted in motion propagated to all diſtances in an inſtant; it would require an infinite force every moment, in every ſhining particle, to generate that motion. And if it conſiſted in preſſion or motion, propagated either in an inſtant or in time, it would bend into the ſhadow. For preſſion or motion cannot be propagated in a fluid in right lines beyond an obſtacle, which ſtops part of the motion, but will bend and ſpread every way into the quieſcent medium, which lies beyond the obſtacle ([l]). Gravity tends downwards; but the preſſure of water, ariſing from gravity, tends every way with equal force; and is propagated as readily, and with as much force, ſideways as downwards, and through crooked paſſages as through ſtrait ones. The waves on the ſurface of ſtagnating water, paſſing by the ſides of a broad obſtacle which ſtops part of them, bend afterwards, and dilate themſelves gradually into the quiet water behind the obſtacle. The waves, pulſes or vibrations of the air, wherein ſounds conſiſt, bend manifeſtly; though not ſo much as the waves of water. For a bell, or a cannon, may be heard beyond a hill, which intercepts the ſight of the ſounding body; and ſounds are propagated as readily through crooked pipes, as through ſtreight ones. But light is never known to follow crooked paſſages, nor to bend into the ſhadow. For the fixed ſtars, by the interpoſition of any of the planets, ceaſe to be ſeen. And ſo do the parts of the ſun by the interpoſition of the moon, *Mercury* or *Venus*. The rays, which paſs very near to the edges of any body, are bent a little by the action of the body, as we ſhewed above; but this

([l]) Princip. Lib. II. Prop. XLII.

bending

bending is not towards, but from the ſhadow, and is performed only in the paſſage of the ray by the body, and at a very ſmall diſtance from it. So ſoon as the ray is paſt the body, it goes right on. Query XXVIII.

To explain the unuſual refraction of iſland cryſtal by preſſion or motion propagated, has not hitherto been attempted (to my knowledge) except by *Huygens*; who for that end ſuppoſed two ſeveral vibrating mediums within that cryſtal. But when he tried the refractions in two ſucceſſive pieces of that cryſtal, and found them ſuch as is mentioned above: he confeſſed himſelf at a loſs for explaining them. For preſſions or motions, propagated from a ſhining body through an uniform medium, muſt be on all ſides alike; whereas by thoſe experiments it appears, that the rays of light have different properties in their different ſides. He ſuſpected that the pulſes of *æther*, in paſſing through the firſt cryſtal, might receive certain new modifications; which might determine them to be propagated in this or that medium within the ſecond cryſtal, according to the poſition of that cryſtal. But what modifications thoſe might be, he could not ſay; nor think of any thing ſatisfactory in that point. And if he had known that the unuſual refraction depends not on new modifications, but on the original and unchangeable diſpoſitions of the rays, he would have found it as difficult to explain how thoſe diſpoſitions, which he ſuppoſed to be impreſſed on the rays by the firſt cryſtal, could be in them before their incidence on that cryſtal; and in general, how all rays, emitted by ſhining bodies, can have thoſe diſpoſitions in them from the beginning. To me, at leaſt, this ſeems inexplicable, if light be nothing elſe than preſſion or motion propagated through *æther*.

Mais pour dire comment cela ſe fait, je n'ay rien trove juſqu' ici qui me ſatisfaſſe. C. H. de la lumiere. c. 5. p. 91.

And it is as difficult to explain by theſe hypotheſes, how rays can be alternately in fits of eaſy Reflexion and eaſy Tranſmiſſion; unleſs perhaps one might ſuppoſe, that there are in all ſpace two æthereal vibrating mediums, and that the vibrations of one of them conſtitute light, and the vibrations of the other are ſwifter, and as often as they overtake the vibrations of the Firſt, put them

Query xxviii. No denſe matter

into thoſe fits (m). But how two *æthers* can be diffuſed through all ſpace, one of which acts upon the other, and by conſequence is re-acted upon, without retarding, ſhattering, diſperſing and confounding one another's motions, is inconceivable. And againſt filling the heavens with fluid mediums, unleſs they be exceeding rare, a great objection ariſes from the regular and very laſting motions of the planets and comets in all manner of courſes through the heavens. For thence it is manifeſt, that the heavens are void of all ſenſible reſiſtance, and by conſequence of all ſenſible matter.

For the reſiſting power of fluid mediums ariſes partly from the attrition of the parts of the medium, and partly from the *vis inertiæ* of the matter. That part of the reſiſtance of a ſpherical body, which ariſes from the attrition of the parts of the medium, is very nearly as the diameter; or at the moſt, as the *factum* of the diameter, and the velocity of the ſpherical body together. And that part of the reſiſtance, which ariſes from the *vis inertiæ* of the matter, is as the ſquare of that *factum* (n). And by this difference the two ſorts of reſiſtance may be diſtinguiſhed from one another in any medium; and theſe being diſtinguiſhed, it will be found that almoſt all the reſiſtance of bodies of a competent magnitude, moving in air, water, quick-ſilver, and ſuch like fluids, with a competent velocity, ariſes from the *vis inertiæ* of the parts of the fluid.

Now that part of the reſiſting power of any medium, which ariſes from the tenacity, friction or attrition of the parts of the medium, may be diminiſhed by dividing the matter into ſmaller parts, and making the parts more ſmooth and ſlippery: but that part of the reſiſtance, which ariſes from the *vis inertiæ*, is proportional to the denſity of the matter, and cannot be diminiſhed by dividing the matter into ſmaller parts, nor by any other means than

(m) The remaining part of this paragraph, in the firſt of the two *Latin* editions of Dr. *Clarke*, ſtood thus. Verum duo ibi confingere Ætherum genera, ubi nulla ratio cogat ut vel unum quidem admittamus; exiſtimare porro duo Ætherum genera ita in omnia ſpatio unà ineſſe poſſe, ut tamen invicem non intermiſceantur, nec in unum Medium coeant; comminiſci denique duo illa Ætherum genera diſtinctas ita habere poſſe vibrationes, ut tamen duo Luminis genera non conſtituant; hæ quidem mihi videntur difficultates inexplicabiles. Præterea nulla eſſe omnino iſtiuſmodi media fluida, inde colligo, quòd Planetæ & Cometæ regulari adeo & diuturno motu per ſpatia

than by decreaſing the denſity of the medium. And for theſe reaſons the denſity of fluid mediums is very nearly proportional to their reſiſtance. Liquors which differ not much in denſity, as water, ſpirit of wine, ſpirit of turpentine, hot oil, differ not much in reſiſtance. Water is thirteen or fourteen times lighter than quick-ſilver, and by conſequence thirteen or fourteen times rarer; and its reſiſtance is leſs than that of quick-ſilver in the ſame proportion, or thereabouts, as I have found by experiments made with pendulums. The open air, in which we breathe, is eight or nine hundred times lighter than water, and by conſequence eight or nine hundred times rarer; and accordingly its reſiſtance is leſs than that of water in the ſame proportion, or thereabouts; as I have alſo found by experiments made with pendulums (°). And in thinner air the reſiſtance is ſtill leſs; and at length, by rarifying the air, becomes inſenſible. For ſome feathers falling in the open air meet with great reſiſtance; but in a tall glaſs, well emptied of air, they fall as faſt as lead or glaſs, as I have ſeen tried ſeveral times. Whence the reſiſtance ſeems ſtill to decreaſe in proportion to the denſity of the fluid. For I do not find by any experiments, that bodies moving in quick-ſilver, water or air, meet with any other ſenſible reſiſtance, than what ariſes from the denſity and tenacity of thoſe ſenſible fluids; as they would do, if the pores of thoſe fluids, and all other ſpaces, were filled with a denſe and ſubtile fluid (P). Now if the reſiſtance in a veſſel, well emptied of water, was but an hundred times leſs than in the open air, it would be about a million of times leſs than in quick-ſilver. But it ſeems to be much leſs in ſuch a veſſel; and ſtill much leſs in the heavens, at the height of three or four hundred miles from the earth, or above. For Mr. *Boyle* has ſhewed, that air may be rarified above ten thouſand times in veſſels of glaſs; and the heavens are much emptier of air, than any *vacuum* we can make below. For ſince the air is compreſſed by the weight of the incumbent atmoſphere, and the denſity of air is

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ſpatia cæleſtia undique & quaquaverſum & in omnes partes ferantur. Inde enim liquet ſpatia cæleſtia omnis ſenſibilis reſiſtentiæ, & conſequenter omnis ſenſibilis materiæ, expertia eſſe.

In the ſecond *Engliſh* edition, and all that ſucceeded, it appeared as we now give it.

(°) Princip. Lib. II. Prop. XXXV. Cor. 5. and Prop. XXXVIII. Cor. 1.

(°) Princip. Lib. II. Sect. VI. Scholium.

(P) See Princip. Lib. II. Sect. VI. Scholium towards the end.

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proportional to the force compreſſing it; it follows by computation, that at the height of about ſeven and a half *Engliſh* miles from the earth, the air is four times rarer than at the ſurface of the earth; and at the height of 15 miles, it is ſixteen times rarer than that at the ſurface of the earth; and at the height of $22\frac{1}{2}$, 30, or 38 miles, it is reſpectively 64, 256, or 1024 times rarer, or thereabouts; and at the height of 76, 152, 228 miles, it is about 1000000, 1000000000000 or 1000000000000000000 times rarer; and ſo on (q).

Heat promotes fluidity very much, by diminiſhing the tenacity of bodies. It makes many bodies fluid, which are not fluid in cold; and increaſes the fluidity of tenacious liquors, as of oil, balſam and honey, and thereby decreaſes their reſiſtance. But it decreaſes not the reſiſtance of water conſiderably; as it would do, if any conſiderable part of the reſiſtance of water aroſe from the attrition, or tenacity, of its parts. And therefore the reſiſtance of water ariſes principally, and almoſt entirely, from the *vis inertiæ* of its matter; and by conſequence, if the heavens were as denſe as water, they would not have much leſs reſiſtance than water; if as denſe as quick-ſilver, they would not have much leſs reſiſtance than quick-ſilver; if abſolutely denſe, or full of matter without any *vacuum*, let the matter be never ſo ſubtile and fluid, they would have a greater reſiſtance than quick-ſilver. A ſolid globe in ſuch a medium would loſe above half its motion in moving three times (r) the length of its diameter; and a globe not ſolid, ſuch as are the planets, would be retarded ſooner. And therefore to make way for the regular and laſting motions of the planets and comets, it is neceſſary to empty the heavens of all matter; except perhaps ſome very thin vapours, ſteams or *effluvia*, ariſing from the atmoſpheres of the earth, planets and comets, and from ſuch an exceeding rare æthereal medium as we deſcribed above. A denſe fluid can be of no uſe for explaining

(q) Concerning the rarity of the air at all heights above the earth's ſurface, ſee the ſixth Section of my diſcourſe on the Barometer publiſhed in the Volume of the Philoſophical Tranſactions.

(r) — *three times.*] Our Author ſeems to make the ſuppoſition of a ſolid, or abſolutely denſe globe moving in a fluid *Plenum*. The denſity therefore of the globe is ſuppoſed equal to that of the fluid, wherein it is ſuppoſed to move. Should we not, therefore, according to the 4th Cor. of Princip. Lib. II. Prop. xxxviii. for *three times*, read *twice?*

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the phænomena of Nature, the motions of the planets and comets being better explained without it (s). It ſerves only to diſturb and retard the motions of thoſe great bodies, and make the frame of Nature languiſh: and in the pores of bodies, it ſerves only to ſtop the vibrating motions of their parts, wherein their heat and activity conſiſts. And as it is of no uſe, and hinders the operations of Nature, and makes her languiſh; ſo there is no evidence for its exiſtence, and therefore it ought to be rejected. And if it be rejected, the hypotheſes that light conſiſts in preſſion or motion propagated through ſuch a medium, are rejected with it. Query XXVIII. in the planetary regions.

And for rejecting ſuch a medium, we have the authority of thoſe the oldeſt and moſt celebrated philoſophers of *Greece* and *Phœnicia*, who made a *vacuum* and atoms, and the gravity of atoms, the firſt principles of their philoſophy; tacitly attributing gravity to ſome other cauſe than denſe matter (t). Later philoſophers baniſh the conſideration of ſuch a cauſe out of Natural Philoſophy, feigning hypotheſes for explaining all things mechanically, and referring other cauſes to metaphyſicks. Whereas the main buſineſs of natural philoſophy is to argue from phænomena without feigning hypotheſes, and to deduce cauſes from effects, till we come to the very Firſt cauſe; which certainly is not mechanical: and not only to unfold the mechaniſm of the world, but chiefly to reſolve theſe and ſuch like Queſtions. *What is there in places* [*almoſt*] (u) *empty of matter, and whence is it that the ſun and planets gravitate towards one another, without* [*denſe*] (u) *matter between them? Whence is it that Nature doth nothing in vain; and whence ariſes all that order and beauty which we ſee in the world? To what end are comets; and whence is it that planets move all one and the ſame way in orbs concentrick, while comets move all manner of ways in orbs very excentrick; and what*

(s) *A denſe fluid—without it.*] For this ſentence we had in Dr. *Clarke's* firſt *Latin*, Materia illa fictitia & commentitia, quâ cæli ſunt repleti, nullo modo utilis eſt ad explicanda Phænomena Naturæ; quando Planetarum quidem & Cometarum motus, ope Gravitatis meliùs explicantur ſine illâ; gravitaſque per illam nondum fuit explicata.

(t) — *other cauſe than denſe matter.*] Alii alicui cauſæ à materiâ diverſæ. Dr. *Clarke's* firſt *Latin.*

(u) — *almoſt—denſe.*] Theſe two words have nothing anſwering to them in Dr. *Clark's* firſt *Latin.*

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hinders the fixed ſtars from falling upon one another (x)*? How came the bodies of animals to be contrived with ſo much art, and for what ends were their ſeveral Parts? Was the eye contrived without ſkill in Opticks, and the ear without knowledge of ſounds? How do the motions of the body follow from the will; and whence is the inſtinct in animals? Is not the ſenſory of animals that place, to which the ſenſitive ſubſtance is preſent; and into which the ſenſible ſpecies of things are carried through the nerves and brain, that there they may be perceived by their immediate preſence to that ſubſtance? And theſe things being rightly diſpatched, Does it not appear from phænomena, that there is a Being incoporeal, living, intelligent, omnipreſent, who, in infinite ſpace, as it were in his ſenſory* (y)*, ſees the things themſelves intimately, and throughly perceives them, and comprehends them wholly by their immediate preſence to himſelf: of which things the images only, carried through the organs of ſenſe into our little ſenſoriums, are there ſeen and beheld by that which in us perceives and thinks.* And though every true ſtep made in this philoſophy brings us not immediately to the knowledge of the Firſt cauſe, yet it brings us nearer to it, and on that account is to be highly valued.

Of the Form of Light.

Qu. 29. Are not the rays of light very ſmall bodies emitted from ſhining ſubſtances? For ſuch bodies will paſs through uniform mediums in right lines, without bending into the ſhadow; which is the nature of the rays of light. They will alſo be capable of ſeveral properties, and be able to conſerve their properties unchanged in paſſing through ſeveral mediums; which is another condition of the rays of light. Pellucid ſubſtances act upon the rays of light, at a diſtance, in refracting, reflecting and inflecting them; and the rays mutually agitate the parts of thoſe ſubſtances at a diſtance, for heating them; and this action and reaction, at a diſtance, very much reſembles an Attractive force between

(x) — *the fixed Stars from falling.*] Quo minus Sol & Stellæ fixæ in ſe mutuò irruant. Dr. *Clarke's* firſt *Latin.*

(y) — *in infinite ſpace as it were in his ſenſory.*]

Νὺξ δὲ οἱ ἀψευδὴς, βασιλήιος, ἀφθίτῳ αἰθὴρ,
Ὧι δὴ πανία κλυει καὶ φραζεται· ὐδέ τις ἐστιν
Αὐδὴ ὐδ' ἐνοπὴ, ὐδ' αυ κλυτῷ, ὐδὲ μεν ὀσσα,
Ἡ λήθει Διὸς ὐας ὑπερμενέος Κρονίωνος.

I am

tween bodies. If refraction be performed by attraction of the rays, the ſines of incidence muſt be to the ſines of refraction in a given proportion; as we ſhewed in our principles of philoſophy (z): and this rule is true by experience. The rays of light, in going out of glaſs into a *vacuum*, are bent towards the glaſs; and if they fall too obliquely on the *vacuum*, they are bent backwards into the glaſs, and totally reflected; and this reflexion cannot be aſcribed to the reſiſtance of an abſolute *vacuum*, but muſt be cauſed by the power of the glaſs attracting the rays at their going out of it into the *vacuum*, and bringing them back. For if the farther ſurface of the glaſs be moiſtened with water or clear oil, or liquid and clear honey; the rays, which would otherwiſe be reflected, will go into the water, oil, or honey; and therefore are not reflected, before they arrive at the farther ſurface of the glaſs, and begin to go out of it. If they go out of it into the water, oil or honey; they go on, becauſe the attraction of the glaſs is almoſt balanced, and rendered ineffectual by the contrary attraction of the liquor. But if they go out of it into a *vacuum*, which has no attraction to balance that of the glaſs; the attraction of the glaſs either bends and refracts them, or brings them back and reflects them. And this is ſtill more evident by laying together two priſms of glaſs, or two object-glaſſes of very long teleſcopes, the one plane, the other a little convex, and ſo compreſſing them that they do not fully touch, nor are too far aſunder. For the light, which falls upon the farther ſurface of the firſt glaſs, where the interval between the glaſſes is not above the ten hundred thouſandth part of an inch, will go through that ſurface, and through the air or *vacuum* between the glaſſes, and enter into the ſecond glaſs; as was explained in the firſt, fourth and eighth Obſervations of the firſt Part of the ſecond Book. But if the ſecond glaſs be taken away; the light, which goes out of the ſecond ſurface of the firſt glaſs into the air or *vacuum*, will not go on forwards, but turns back into the

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I am not ſingular in imagining a reſemblance between Sir *Iſaac Newton's* notion of ſpace as the ſenſory of God, and that which we find in theſe Orphic Verſes. The learned *Geſner* has remarked it. *His verſibus poterat uti Clarckius ad naturam ſpatii ſui declarandam, quatenus illud ſenſorium quoddam Dei dici poſſe putavit.* It muſt be confeſſed, this opinion is more tolerable in poetry than in philoſophy.

(z) Princip. Lib. II. Prop. XCIV.

firſt

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firſt glaſs, and is reflected; and therefore it is drawn back by the power of the firſt glaſs, there being nothing elſe to turn it back. Nothing more is requiſite for producing all the variety of colours and degrees of refrangibility, than that the rays of light be bodies of different ſizes; the leaſt of which may make Violet, the weakeſt and darkeſt of the colours, and be more eaſily diverted by Refracting ſurfaces from the right courſe; and the reſt, as they are bigger and bigger, may make the ſtronger and more lucid colours, Blue, Green, Yellow and Red, and be more and more difficultly diverted. Nothing more is requiſite for putting the rays of light into fits of eaſy Reflexion and eaſy Tranſmiſſion, than that they be ſmall bodies, which by their attractive powers, or ſome other force, ſtir up vibrations in what they act upon; which vibrations being ſwifter than the rays, overtake them ſucceſſively, and agitate them; ſo as by turns to increaſe and decreaſe their velocities, and thereby put them into thoſe fits. And laſtly, the unuſual refraction of iſland cryſtal looks very much as if it were performed by ſome kind of Attractive virtue, lodged in certain ſides both of the rays, and of the particles of the cryſtal. For were it not for ſome kind of diſpoſition, or virtue, lodged in ſome ſides of the particles of the cryſtal, and not in their other ſides, and which inclines and bends the rays towards the coaſt of unuſual refraction; the rays, which fall perpendicularly on the cryſtal, would not be refracted towards that coaſt rather than towards any other coaſt, both at their incidence and at their emergence, ſo as to emerge perpendicularly, by a contrary ſituation of the coaſt of unuſual refraction, at the ſecond ſurface; the cryſtal acting upon the rays, after they have paſſed through it and are emerging into the air, or, if you pleaſe, into a *vacuum*. And ſince the cryſtal by this diſpoſition or virtue does not act upon the rays, unleſs when one of their ſides of unuſual refraction looks towards that coaſt; this argues a virtue or diſpoſition in thoſe ſides of the rays, which anſwers to and ſympathizes with that virtue or diſpoſition of the cryſtal, as the poles of two magnets anſwer to one another. And as magnetiſm may be intended and remitted, and is found only in the magnet and in iron: ſo this virtue, of refracting the Perpendicular, rays is greater in iſland cryſtal,

leſs

leſs in cryſtal of the rock, and is not yet found in other bodies. I do not ſay that this virtue is Magnetical; it ſeems to be of another kind: I only ſay, that whatever it be, it is difficult to conceive how the rays of light, unleſs they be bodies, can have a permanent virtue in two of their ſides, which is not in their other ſides; and this without any regard to their poſition to the ſpace, or medium, through which they paſs. Queries xixx, xxx.

What I mean in this Queſtion by a *vacuum*, and by the attractions of the rays of light towards glaſs or cryſtal, may be underſtood by what was ſaid in the 18th, 19th and 20th Queſtions ([aa]).

Qu. 30. Are not groſs bodies and light convertible into one another; and may not bodies receive much of their activity from the particles of light which enter their compoſition? For all Fixed bodies, being heated, emit light ſo long as they continue ſufficiently hot; and light mutually ſtops in bodies, as often as its rays ſtrike upon their parts, as we ſhewed above. I know no body leſs apt to ſhine than water; and yet water, by frequent diſtillations, changes into fixed earth, as Mr. *Boyle* has tried; and then this earth, being enabled to endure a ſufficient heat, ſhines by heat like other bodies. Mutual convertibility of groſs bodies and light.

The changing of bodies into light, and light into bodies, is very conformable to the courſe of Nature, which ſeems delighted with tranſmutations. Water, which is a very fluid taſteleſs ſalt, ſhe changes by heat into vapour, which is a ſort of air; and by cold into ice, which is a hard, pellucid, brittle, fuſible ſtone; and this ſtone returns into water by heat, and vapour returns into water by cold. Earth by heat becomes fire, and by cold returns into earth. Denſe bodies by fermentation rarify into ſeveral ſorts of air; and this air by fermentation, and ſometimes without it, returns into denſe bodies. Mercury appears ſometimes in the form of a fluid metal; ſometimes in the form of a hard brittle metal; ſometimes in the form of a corroſive pellucid ſalt, called ſublimate; ſometimes in the form of a taſteleſs, pellucid, volatile white earth, called *Mercurius dulcis*; or in that of a red opake volatile earth, called Cinnabar; or in that of

([aa]) This laſt paragraph of this Query was not in Dr. *Clark's* firſt *Latin*.

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a red or white precipitate; or in that of a fluid ſalt; and in diſtillation it turns into vapour; and being agitated *in vacuo*, it ſhines like fire. And after all theſe changes it returns again into its firſt form of mercury. Eggs grow from inſenſible magnitudes, and change into animals; tadpoles, into frogs; and worms, into flies. All birds, beaſts and fiſhes, inſects, trees, and other vegetables, with their ſeveral parts, grow out of water and watery tinctures and ſalts; and, by putrefaction, return again into watery ſubſtances. And water, ſtanding a few days in the open air, yields a tincture, which (like that of malt) by ſtanding longer yields a ſediment and a ſpirit; but before putrefaction is fit nouriſhment for animals and vegetables. And among ſuch various and ſtrange tranſmutations, why may not Nature change bodies into light, and light into bodies (bb)?

Elective Attractions.

Qu. 31. Have not the ſmall particles of bodies certain powers, virtues or forces, by which they act at a diſtance, not only upon the rays of light for reflecting, refracting and inflecting them, but alſo upon one another, for producing a great part of the phænomena of Nature? For it is well known that bodies act one upon another by the attractions of gravity, magnetiſm and electricity; and theſe inſtances ſhew the tenor and courſe of Nature, and make it not improbable, but that there may be more Attractive powers than theſe. For Nature is very conſonant and conformable to herſelf. How theſe attractions may be performed, I do not here conſider. What I call attraction, may be performed by Impulſe, or by ſome other means unknown to me. I uſe

(bb) In the firſt edition of Dr. *Clark's Latin* we find added here what follows: "Jam quidem eorum corporum, quæ ſunt ejuſdem generis et virtutis, quanto eſt quodque minutius, tanto fortiorem habet, pro ratione magnitudinis ſuæ, vim attrahentem. Fortior comperta eſt ea vis in parvis Magnetibus, pro ratione ponderis ſui, quàm in majoribus: nam parvorum magnetum partes, cùm ſint inter ſe proximiores, vires ſuas faciliùs in unum conjungunt. Quocirca radii Luminis, cùm ſint corporum omnium (quod ſciamus) minutiſſima, expectandum erit, ut vires attrahentes habere reperiantur omnium fortiſſimas. Quàm fortes autem eæ ſint, regulâ ſequente colligi poterit. Attractio radii Luminis, pro quantitate materiæ ſuæ, eſt ad gravitatem quam corpus aliquod projectum habet, pro quantitate itidem materiæ ſuæ, in compoſitâ ratione velocitatis radii luminis ad velocitatem corporis illius projecti, & flexuræ, ſeu curvaturæ lineæ, quam Radius deſcribit in loco Refractionis, ad flexuram, ſeu curvaturam lineæ quam id corpus projectum deſcribit; ita videlicet, ſi inclinatio Radii ad ſuperficiem refringentem eadem ſit, quæ eſt corporis illius projecti ad Horizontem. Atque ex hâc quidem proportione colligo, attractionem Radiorum Luminis eſſe amplius 1000000000000000 partibus majorem quàm Gravitatem corporum in ſuperficie Terræ, pro quantitate materiæ quæ in eis ineſt; ſi ſcilicet lumen è Sole in Terram circiter

uſe that word here to ſignify only in general any force by which bodies tend towards one another, whatſoever be the cauſe. For we muſt learn, from the phænomena of Nature, what bodies attract one another, and what are the laws and properties of the attraction, before we enquire the cauſe by which the attraction is performed. The attractions of gravity, magnetiſm and electricity, reach to very ſenſible diſtances, and ſo have been obſerved by vulgar eyes; and there may be others which reach to ſo ſmall diſtances as hitherto eſcape obſervation; and perhaps electrical attraction may reach to ſuch ſmall diſtances, even without being excited by friction. Elective Attractions.

For when ſalt of tartar runs *per deliquium*, is not this done by an attraction between the particles of the ſalt of tartar, and the particles of the water, which float in the air in the form of vapours? And why does not common ſalt, or ſalt-petre, or vitriol, run *per deliquium*, but for want of ſuch an attraction? Or why does not ſalt of tartar draw more water out of the air, than in a certain proportion to its quantity, but for want of an attractive force after it is ſatiated with water? And whence is it but from this Attractive power that water, which alone diſtills with a gentle lukewarm heat, will not diſtill from ſalt of tartar without a great heat? And is it not from the like Attractive power between the particles of oil of vitriol and the particles of water, that oil of vitriol draws to it a good quantity of water out of the air; and after it is ſatiated draws no more, and in diſtillation lets go the water very difficultly? And when the water and oil of vitriol, poured ſucceſſively into the ſame veſſel, grow very hot in the

circiter ſeptem vel octo minutorum ſpatio pervenit. Et in ipſo radiorum contactu, eorum vis multo adhuc major eſſe poteſt. Tanta autem vis in radiis non poteſt non ingentes effectus obtinere in illis materiæ particulis, quibuſcum in corporibus componendis conjuncti ſint; ad efficiendum, ut particulæ illæ ſe invicem attrahant, & inter ſe moveantur. Quæ res ut melius intelligatur, Quæſtionem ſequentem proponam.

All this was omitted in the ſecond *Engliſh*, and all the ſucceeding editions. The Rule given for comparing the accelerative attraction of the rays of Light with the accelerative force of Gravity is erroneous; our Author having put the ſimple inſtead of the duplicate proportion of the velocities. For if corpuſcles deſcribe orbits about two different centers; when the radii drawn to thoſe centers are equally inclined to the curves, the accelerative forces will bear to each other a proportion, which will be compounded of the duplicate proportions of the velocities, and the proportion of the curvatures. The moſt ſimple caſe of this Theorem, which is when the radius, drawn to the center of the forces in each orbit, is perpendicular to the curve, is demonſtrated in my Notes upon the XXVIIIth Propoſition of the third Book of the Principia. And from the demonſtration of that caſe, the Mathematical Reader will perceive the truth of the general Propoſition.

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mixing, does not this heat argue a great motion in the parts of the liquors? And does not this motion argue, that the parts of the two liquors, in mixing, coaleſce with violence; and by conſequence, ruſh towards one another with an accelerated motion? And when *aqua fortis*, or ſpirit of vitriol, poured upon filings of iron, diſſolves the filings with a great heat and ebullition, is not this heat and ebullition effected by a violent motion of the parts? And does not that motion argue, that the acid parts of the liquor ruſh towards the parts of the metal with violence, and run forcibly into its pores, till they get between its outmoſt particles and the main maſs of the metal, and ſurrounding thoſe particles looſen them from the main maſs, and ſet them at liberty to float off into the water? And when the Acid particles, which alone would diſtill with an eaſy heat, will not ſeparate from the particles of the metal without a very violent heat, doth not this confirm the attraction between them?

When ſpirit of vitriol, poured upon common ſalt, or ſalt-petre, makes an ebullition with the ſalt and unites with it; and in diſtillation the ſpirit of the common ſalt or ſalt-petre comes over much eaſier than it would do before; and the acid part of the ſpirit of vitriol ſtays behind; does not this argue, that the fixed alcaly of the ſalt attracts the acid ſpirit of the vitriol more ſtrongly than its own ſpirit; and not being able to hold them both, lets go its own? And when oil of vitriol is drawn off from its weight of nitre, and from both the ingredients a compound ſpirit of nitre is diſtilled, and two parts of this ſpirit are poured on one part of oil of cloves or caraway ſeeds, or of any ponderous oil of Vegetable or Animal ſubſtances, or oil of turpentine thickened with a little balſam of ſulphur, and the liquors grow ſo very hot in mixing, as preſently to ſend up a burning flame: does not this very great and ſudden heat argue, that the two liquors mix with violence; and that their parts, in mixing, run towards one another with an accelerated motion, and claſh with the greateſt force? And is it not for the ſame reaſon that well rectified ſpirit of wine, poured on the ſame compound ſpirit, flaſhes; and that the *Pulvis fulminans*, compoſed of ſulphur, nitre, and ſalt of tartar, goes off with a more ſudden and violent exploſion than

gun-powder; the Acid ſpirits of the ſulphur and nitre ruſhing towards one another, and towards the ſalt of tartar, with ſo great a violence, as by the ſhock to turn the whole at once into vapour and flame? Where the diſſolution is ſlow, it makes a ſlow ebullition and a gentle heat; and where it is quicker, it makes a greater ebullition with more heat; and where it is done at once, the ebullition is contracted into a ſudden blaſt or violent exploſion, with a heat equal to that of fire and flame. So when a drachm of the above-mentioned compound ſpirit of nitre was poured upon half a drachm of oil of caraway ſeeds *in vacuo*; the mixture immediately made a flaſh like gun-powder, and burſt the exhauſted receiver, which was a glaſs ſix inches wide, and eight inches deep. And even the groſs body of ſulphur powdered, and with an equal weight of iron filings, and a little water made into paſte, acts upon the iron; and in five or ſix hours grows too hot to be touched, and emits a flame. And by theſe experiments, compared with the great quantity of ſulphur with which the earth abounds, and the warmth of the interior parts of the earth, and hot ſprings, and burning mountains, and with damps, mineral corruſcations, earthquakes, hot ſuffocating exhalations, hurricanes and ſpouts; we may learn, that ſulphureous ſteams abound in the bowels of the earth and ferment with minerals, and ſometimes take fire with a ſudden corruſcation and exploſion; and if pent up in ſubterraneous caverns, burſt the caverns with a great ſhaking of the earth, as in ſpringing of a mine. And then the vapour, generated by the exploſion, expiring through the pores of the earth, feels hot and ſuffocates; and makes tempeſts and hurricanes; and ſometimes cauſes the land to ſlide, or the ſea to boil; and carries up the water thereof in drops, which by their weight fall down again in ſpouts. Alſo ſome ſulphureous ſteams, at all times when the earth is dry, aſcending into the air, ferment there with Nitrous acids; and, ſometimes taking fire, cauſe lightning and thunder, and fiery meteors. For the air abounds with acid vapours fit to promote fermentations; as appears by the ruſting of iron and copper in it, the kindling of fire by blowing, and the beating of the heart by means of reſpiration. Now the above-mentioned motions are ſo great

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great and violent, as to ſhew that in fermentations, the particles of bodies which almoſt reſt, are put into new motions by a very potent principle, which acts upon them only when they approach one another, and cauſes them to meet and claſh with great violence, and grow hot with the motion, and daſh one another into pieces, and vaniſh into air, and vapour, and flame.

When ſalt of tartar *per deliquium*, being poured into the ſolution of any metal, precipitates the metal, and makes it fall down to the bottom of the liquor in the form of mud: does not this argue, that the Acid particles are attracted more ſtrongly by the ſalt of tartar than by the metal, and by the ſtronger attraction go from the metal to the ſalt of tartar? And ſo when a ſolution of iron in *Aqua fortis* diſſolves the *Lapis Calaminaris*, and lets go the iron; or a ſolution of copper diſſolves iron immerſed in it, and lets go the copper; or a ſolution of ſilver diſſolves copper, and lets go the ſilver; or a ſolution of mercury in *Aqua fortis*, being poured upon iron, copper, tin or lead, diſſolves the metal and lets go the mercury: does not this argue, that the Acid particles of the *Aqua fortis* are attracted more ſtrongly by the *Lapis Calaminaris* than by iron; and more ſtrongly by iron, than by copper; and more ſtrongly by copper, than by ſilver; and more ſtrongly by iron, copper, tin and lead, than by mercury? And is it not for the ſame reaſon that iron requires more *Aqua fortis* to diſſolve it than copper, and copper more than the other metals; and that of all metals, iron is diſſolved moſt eaſily, and is moſt apt to ruſt; and next after iron, copper?

When oil of vitriol is mixed with a little water, or is run *per deliquium*, and in diſtillation the water aſcends difficultly, and brings over with it ſome part of the oil of vitriol in the form of ſpirit of vitriol, and this ſpirit being poured upon iron, copper, or ſalt of tartar, unites with the body and lets go the water: doth not this ſhew, that the Acid ſpirit is attracted by the water, and more attracted by the Fixed body than by the water, and therefore lets go the water to cloſe with the fixed body? And is it not for the ſame reaſon that the water and Acid ſpirits, which are mixed together in vinegar, *Aqua fortis*, and ſpirit of ſalt, cohere and riſe together in diſtillation; but if the *menſtruum* be poured

on

on ſalt of tartar, or on lead or iron, or any Fixed body which it can diſſolve, the acid by a ſtronger attraction adheres to the body, and lets go the water? And is it not alſo from a mutual attraction, that the ſpirits of ſoot and ſea-ſalt unite and compoſe the particles of ſal-armoniac; which are leſs volatile than before, becauſe groſſer and freer from water; and that the particles of ſal-armoniac in ſublimation carry up the particles of antimony, which will not ſublime alone; and that the particles of mercury, uniting with the acid particles of ſpirit of ſalt, compoſe mercury ſublimate; and with the particles of ſulphur, compoſe cinnabar; and that the particles of ſpirit of wine and ſpirit of urine well rectified unite; and, letting go the water which diſſolved them, compoſe a conſiſtent body; and that in ſubliming cinnabar from ſalt of tartar, or from quick lime, the ſulphur, by a ſtronger attraction of the ſalt or lime, lets go the mercury, and ſtays with the fixed body; and that when mercury ſublimate is ſublimed from antimony, or from regulus of antimony, the ſpirit of ſalt lets go the mercury, and unites with the antimonial metal, which attracts it more ſtrongly; and ſtays with it, till the heat be great enough to make them both aſcend together; and then carries up the metal with it in the form of a very fuſible ſalt, called butter of antimony; although the ſpirit of ſalt alone be almoſt as volatile as water, and the antimony alone as fixed as lead? Attractions.

When *Aqua fortis* diſſolves ſilver and not gold, and *Aqua regia* diſſolves gold and not ſilver; may it not be ſaid, that *Aqua fortis* is ſubtile enough to penetrate gold as well as ſilver, but wants the Attractive force to give it entrance; and that *Aqua regia* is ſubtile enough to penetrate ſilver as well as gold, but wants the Attractive force to give it entrance? For *Aqua regia* is nothing elſe than *Aqua fortis* mixed with ſome ſpirit of ſalt, or with ſal-armoniac; and even common ſalt, diſſolved in *Aqua fortis*, enables the *menſtruum* to diſſolve gold, though the ſalt be a groſs body. When therefore ſpirit of ſalt precipitates ſilver out of *Aqua fortis*; is it not done by attracting and mixing with the *Aqua fortis*, and not attracting, or perhaps repelling ſilver? And when water precipitates antimony out of the ſublimate of antimony and ſal-armoniac, or out of butter of antimony; is it not done by its

diſſolving,

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diſſolving, mixing with, and weakening the ſal-armoniac or ſpirit of ſalt, and its not attracting, or perhaps repelling the antimony? And is it not for want of an Attractive virtue between the parts of water and oil, of quick-ſilver and antimony, of lead and iron, that theſe ſubſtances do not mix; and by a weak attraction, that quick-ſilver and copper mix difficultly; and from a ſtrong one, that quick-ſilver and tin, antimony and iron, water and ſalts, mix readily? And in general, is it not from the ſame principle that heat congregates homogeneal bodies, and ſeparates heterogeneal ones?

When arſnick with ſoap gives a regulus, and with mercury ſublimate a volatile fuſible ſalt, like butter of antimony; doth not this ſhew, that arſnick, which is a ſubſtance totally volatile, is compounded of fixed and volatile parts, ſtrongly cohering by a mutual attraction, ſo that the volatile will not aſcend without carrying up the fixed? And ſo, when an equal weight of ſpirit of wine and oil of vitriol are digeſted together, and in diſtillation yield two fragrant and volatile ſpirits, which will not mix with one another, and a fixed black earth remains behind; doth not this ſhew, that oil of vitriol is compoſed of volatile and fixed parts ſtrongly united by attraction, ſo as to aſcend together in form of a volatile, acid, fluid ſalt, until the ſpirit of wine attracts and ſeparates the volatile parts from the fixed? And therefore, ſince oil of ſulphur *per campanam* is of the ſame nature with oil of vitriol, may it not be inferred, that ſulphur is alſo a mixture of volatile and fixed parts ſo ſtrongly cohering by attraction, as to aſcend together in ſublimation. By diſſolving flowers of ſulphur in oil of turpentine, and diſtilling the ſolution, it is found that ſulphur is compoſed of an inflammable thick oil or fat bitumen, an acid ſalt, a very fixed earth, and a little metal. The three firſt were found not much unequal to one another, the fourth in ſo ſmall a quantity as ſcarce to be worth conſidering. The Acid ſalt, diſſolved in water, is the ſame with oil of ſulphur *per campanam*; and abounding much in the bowels of the earth, and particularly in markaſites, unites itſelf to the other ingredients of the markaſite, which are, bitumen, iron, copper and earth, and with them compounds alume, vitriol and ſulphur.

With

With the earth alone it compounds alum; with the metal alone, or metal and earth together, it compounds vitriol; and with the bitumen and earth it compounds ſulphur. Whence it comes to paſs, that markaſites abound with thoſe three minerals. And is it not from the mutual attraction of the ingredients, that they ſtick together for compounding theſe minerals, and that the bitumen carries up the other ingredients of the ſulphur, which without it would not ſublime? And the ſame queſtion may be put concerning all, or almoſt all the groſs bodies in nature. For all the parts of animals and vegetables are compoſed of ſubſtances volatile and fixed, fluid and ſolid, as appears by their analyſis; and ſo are ſalts and minerals, ſo far as chemiſts have been hitherto able to examine their compoſition. Attractions.

When mercury ſublimate is re-ſublimed with freſh mercury, and becomes *mercurius dulcis*, which is a white taſteleſs earth ſcarce diſſolvable in water; and *mercurius dulcis*, re-ſublimed with ſpirit of ſalt, returns into mercury ſublimate; and when metals, corroded with a little Acid, turn into ruſt, which is an earth taſteleſs and indiſſolvable in water; and this earth, imbibed with more acid, becomes a metallic ſalt; and when ſome ſtones, as ſpar of lead, diſſolved in proper *menſtruums*, become ſalts: do not theſe things ſhew, that ſalts are dry earth and watery acid united by attraction; and that the earth will not become, a ſalt without ſo much acid as makes it diſſolvable in water? Do not the ſharp and pungent taſtes of acids ariſe from the ſtrong attraction, whereby the acid particles ruſh upon and agitate the particles of the tongue? And when metals are diſſolved in acid *menſtruums*, and the acids, in conjunction with the metal, act after a different manner; ſo that the compound has a different taſte, much milder than before, and ſometimes a ſweet one: is it not becauſe the acids adhere to the metallic particles, and thereby loſe much of their activity? And if the acid be in too ſmall a proportion to make the compound diſſolvable in water, will it not, by adhering ſtrongly to the metal, become unactive and loſe its taſte, and the compound be a taſteleſs earth? For ſuch things, as are not diſſolvable by the moiſture of the tongue, act not upon the taſte.

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As gravity makes the ſea flow round the denſer and weightier parts of the globe of the earth; ſo the attraction may make the watery acid flow round the denſer and compacter particles of earth, for compoſing the particles of ſalt. For otherwiſe the acid would not do the office of a medium between the earth and common water, for making ſalts diſſolvable in the water; nor would ſalt of tartar readily draw off the acid from diſſolved metals, nor metals the acid from mercury. Now as in the great globe of the earth and ſea, the denſeſt bodies by their gravity ſink down in water, and always endeavour to go towards the center of the globe; ſo in particles of ſalt, the denſeſt matter may always endeavour to approach the center of the particle: ſo that a particle of ſalt may be compared to a chaos; being denſe, hard, dry, and earthy in the center; and rare, ſoft, moiſt, and watery in the circumference. And hence it ſeems to be, that ſalts are of a laſting nature, being ſcarce deſtroyed, unleſs by drawing away their watery parts by violence, or by letting them ſoak into the pores of the central earth by a gentle heat in putrefaction, until the earth be diſſolved by the water, and ſeparated into ſmaller particles; which, by reaſon of their ſmallneſs, make the rotten compound appear of a black colour. Hence alſo it may be, that the parts of animals and vegetables preſerve their forms, and aſſimilate their nouriſhment; the ſoft and moiſt nouriſhment eaſily changing its texture by a gentle heat and motion, till it becomes like the denſe, hard, dry, and durable earth in the center of each particle. But when the nouriſhment grows unfit to be aſſimilated, or the central earth grows too feeble to aſſimilate it, the motion ends in confuſion, putrefaction and death.

If a very ſmall quantity of any ſalt or vitriol be diſſolved in a great quantity of water, the particles of the ſalt or vitriol will not ſink to the bottom, though they be heavier in *ſpecie* than the water; but will evenly diffuſe themſelves into all the water, ſo as to make it as ſaline at the top as at the bottom. And does not this imply, that the parts of the ſalt or vitriol recede from one another, and endeavour to expand themſelves, and get as far aſunder as the quantity of water, in which they float, will allow? And does not this endeavour imply, that they have a Repulſive

pulſive force by which they fly from one another; or, at leaſt, that they attract the water more ſtrongly than they do one another? For as all things aſcend in water, which are leſs attracted than water, by the gravitating power of the earth; ſo all the particles of ſalt which float in water, and are leſs attracted than water by any one particle of ſalt, muſt recede from that particle, and give way to the more attracted water. Attractions.

When any ſaline vapour is evaporated to a cuticle and let cool, the ſalt concretes in regular figures; which argues, that the particles of the ſalt, before they concreted, floated in the liquor at equal diſtances in rank and file, and by conſequence that they acted upon one another by ſome power, which at equal diſtances is equal, at unequal diſtances unequal. For by ſuch a power they will range themſelves uniformly, and without it they will float irregularly, and come together as irregularly. And ſince the particles of iſland cryſtal act all the ſame way upon the rays of light, for cauſing the unuſual refraction; may it not be ſuppoſed, that in the formation of this cryſtal, the particles not only ranged themſelves in rank and file for concreting in regular figures, but alſo, by ſome kind of polar virtue, turned their homogeneal ſides the ſame way?

The parts of all homogeneal hard bodies, which fully touch one another, ſtick together very ſtrongly. And for explaining how this may be, ſome have invented hooked atoms, which is begging the queſtion; and others tell us, that bodies are glued together by Reſt; that is, by an occult quality, or rather by nothing (dd): and others, that they ſtick together by conſpiring motions, that is, by relative Reſt amongſt themſelves. I had rather infer from their coheſion, that their particles attract one another by ſome force, which in immediate contact is exceeding ſtrong, at ſmall diſtances performs the Chemical operations abovementioned, and reaches not far from the particles with any ſenſible effect.

All bodies ſeem to be compoſed of hard particles: for otherwiſe fluids would not congeal; as water, oils, vinegar, and ſpirit or oil of vitriol do by freezing; mercury, by fumes of lead; ſpi-

(dd) —*by reſt--by nothing.*]—Quiete; hoc eſt planè nihilo. Dr. *Clarke*'s firſt *Latin.*

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rit of nitre and mercury, by diſſolving the mercury and evaporating the flegm; ſpirit of wine and ſpirit of urine, by deflegming and mixing them; and ſpirit of urine and ſpirit of ſalt, by ſubliming them together to make ſal-armoniac. Even the rays of light ſeem to be hard bodies; for otherwiſe they would not retain different properties in their different ſides. And therefore hardneſs may be reckoned the property of all uncompounded matter. At leaſt, this ſeems to be as evident as the univerſal impenetrability of matter. For all bodies, ſo far as experience reaches, are either hard, or may be hardened; and we have no other evidence of univerſal impenetrability, beſides a large experience without an experimental exception. Now if compound bodies are ſo very hard as we find ſome of them to be, and yet are very porous, and conſiſt of parts which are only laid together; the ſimple particles which are void of pores, and were never yet divided, muſt be much harder. For ſuch hard particles being heaped up together, can ſcarce touch one another in more than a few points; and therefore muſt be ſeparable by much leſs force, than is requiſite to break a ſolid particle, whoſe parts touch in all the ſpace between them, without any pores or interſtices to weaken their coheſion. And how ſuch very hard particles, which are only laid together, and touch only in a few points, can ſtick together, and that ſo firmly as they do, without the aſſiſtance of ſomething which cauſes them to be attracted or preſſed towards one another, is very difficult to conceive.

The ſame thing I infer alſo from the cohering of two poliſhed marbles *in Vacuo*; and from the ſtanding of quick-ſilver in the barometer at the height of 50, 60 or 70 inches, or above, whenever it is well purged of air and carefully poured in, ſo that its parts

(ee) *The atmoſphere by its weight, &c.*] Inſtead of the ſequel of this paragraph, Dr. *Clarke's* firſt *Latin* had what follows.

Nonnulli exiſtimant, marmora illa compreſſa eſſe Æthere quodam ambiente, argentumque vivum eodem Æthere ſurſum in tubum impelli. Verùm ſi ætheri iſti tranſitus patet, vel per argentum vivum, vel per vitrum; fieri non poteſt, ut is argentum vivum ſurſum in tubum impellat. Et ſi tranſitus ei per neutrum patet, jam non poterit is permittere, ut argentum vivum ſubſidat; quomodo illud ſubſidit quidem, ſi vitrum ſuccutiatur, adeoque argentum vivum à ſe disjungit; ſive argentum vivum habeat in ſe bullulas aliquas aëris, quæ impediant, quo minùs partes ejus contingant, & cohæreant inter ſe. Atque etiam idem experimentum obſervatum fuit in aquâ, ab omni priùs aere probè depurgatâ. Quum argentum vivum congelatum ſit fumis plumbi, vel aqua frigore,

parts be every where contiguous both to one another and to the glaſs (ee). The atmoſphere by its weight preſſes the quick-ſilver into the glaſs, to the height of 29 or 30 inches. And ſome other agent raiſes it higher, not by preſſing it into the glaſs, but by making its parts ſtick to the glaſs, and to one another. For upon any diſcontinuation of parts, made either by bubbles or by ſhaking the glaſs, the whole mercury falls down to the height of 29 or 30 inches. Attractions.

And of the ſame kind with theſe experiments are thoſe that follow (ff). If two plane poliſhed plates of glaſs (ſuppoſe two pieces of a poliſhed looking-glaſs) be laid together, ſo that their ſides be parallel, and at a very ſmall diſtance from one another, and then their lower edges be dipped into water, the water will riſe up between them. And the leſs the diſtance of the glaſs is, the greater will be the height to which the water will riſe. If the diſtance be about the hundredth part of an inch, the water will riſe to the height of about an inch; and if the diſtance be greater or leſs in any proportion, the height will be reciprocally proportional to the diſtance very nearly. For the attractive force of the glaſſes is the ſame, whether the diſtance between them be greater or leſs; and the weight of the water drawn up is the ſame, if the height of it be reciprocally proportional to the diſtance of the glaſſes. And in like manner, water aſcends between two marbles, poliſhed plane, when their poliſhed ſides are parallel, and at a very ſmall diſtance from one another. And if ſlender pipes of glaſs be dipped at one end into ſtagnating water, the water will riſe up within the pipe; and the heighth, to which it ariſes, will be reciprocally proportional to the diameter of the cavity of the pipe, and will equal the height to which it riſes between two planes of

gore, partes liquoris congelati ita cohærent, ut corpus durum conſtituant. Atque experimento quidem jam memorato, de liquoribus in barometro ad tam inſolitam uſque altitudinem ſuſpenſis, apparet partes corporum etiam fluidorum cohærere inter ſe. Unde facilè intelligi poteſt, quæcunque demum cauſa efficiat ut partes Glaciei & Metallorum durorum cohæreant inter ſe, eandem itidem efficere, ut partes eorundem corporum etiam liquefactorum cohæreant; licet fortaſſis minùs fortè. Nam particulæ quidem corporum liquefactorum inter ſe huc illuc perpetuò ſublabuntur.

In the ſecond *Engliſh*, and all the ſucceeding editions, the paragraph appeared as we now give it.

(ff) *And of the ſame kind with theſe experiments*, &c.] Nothing of all that follows to the words, *philoſophy to find them out*, was in the firſt *Latin* of Dr. *Clarke*. But the whole firſt appeared in the ſecond *Engliſh*.

glaſs,

Elective

glaſs, if the ſemidiameter of the cavity of the pipe be equal to the diſtance between the planes, or thereabouts. And theſe experiments ſucceed after the ſame manner *in Vacuo* as in the open air (as hath been tried before the Royal Society) and therefore are not influenced by the weight or preſſure of the atmoſphere.

And if a large pipe of glaſs be filled with ſifted aſhes, well preſſed together in the glaſs, and one end of the pipe be dipped into ſtagnating water; the water will riſe up ſlowly in the aſhes, ſo as in the ſpace of a week or fortnight to reach up within the glaſs, to the height of 30 or 40 inches above the ſtagnating water. And the water riſes up to this height by the action only of thoſe particles of the aſhes, which are upon the ſurface of the elevated water; the particles which are within the water, attracting or repelling it as much downwards as upwards. And therefore the action of the particles is very ſtrong. But the particles of the aſhes being not ſo denſe and cloſe together as thoſe of glaſs, their action is not ſo ſtrong as that of glaſs; which keeps quickſilver ſuſpended to the height of 60 or 70 inches, and therefore acts with a force, which would keep water ſuſpended to the height of above 60 feet.

By the ſame principle a ſponge ſucks in water; and the glands in the bodies of animals, according to their ſeveral natures and diſpoſitions, ſuck in various juices from the blood.

If two plane poliſhed plates of glaſs three or four inches broad, and twenty or twenty-five long, be laid, one of them parallel to the horizon, the other upon the firſt, ſo as at one of their ends to touch one another, and contain an angle of about 10 or 15 minutes; and the ſame be firſt moiſtened, on their inward ſides, with a clean cloth dipped into oil of oranges or ſpirit of turpentine; and a drop or two of the oil or ſpirit be let fall upon the lower glaſs at the other end; ſo ſoon as the upper glaſs is laid down upon the lower, ſo as to touch it at one end, as above, and to touch the drop at the other end, making with the lower glaſs an angle of about 10 or 15 minutes; the drop will begin to move towards the concourſe of the glaſſes, and will continue to move with

(gg) That is, a weight of 547 or 820 *lbs.* or upon an average 5½ *cwt.* taking the weight of a cubic

with an accelerated motion, till it arrives at that concourſe of the glaſſes. For the two glaſſes attract the drop, and make it run that way, towards which the attractions incline. And if, when the drop is in motion, you lift up that end of the glaſſes where they meet, and towards which the drop moves, the drop will aſcend between the glaſſes, and therefore is attracted. And as you lift up the glaſſes more and more, the drop will aſcend ſlower and ſlower, and at length reſt; being then carried downward by its weight, as much as upwards by the attraction. And by this means you may know the force, by which the drop is attracted at all diſtances from the concourſe of the glaſſes. Attractions.

Now by ſome experiments of this kind (made by Mr. *Hawkſby*) it has been found that the attraction is almoſt reciprocally in a duplicate proportion of the diſtance of the middle of the drop from the concourſe of the glaſſes, viz. reciprocally in a ſimple proportion, by reaſon of the ſpreading of a drop, and its touching each glaſs in a larger ſurface; and again reciprocally in a ſimple proportion, by reaſon of the attractions growing ſtronger within the ſame quantity of attracting ſurface. The attraction therefore, within the ſame quantity of attracting ſurface, is reciprocally as the diſtance between the glaſſes. And therefore where the diſtance is exceeding ſmall, the attraction muſt be exceeding great. By the Table in the ſecond Part of the ſecond Book, wherein the Thickneſs of coloured plates of water between two glaſſes are ſet down, the Thickneſs of the plate, where it appears very black, is three-eighths of the ten hundredth thouſandth part of an inch. And where the oil of oranges between the glaſſes is of this thickneſs, the attraction, collected by the foregoing rule, ſeems to be ſo ſtrong, as within a circle of an inch in diameter, to ſuffice to hold up a weight equal to that of a cylinder of water of an inch in diameter, and two or three furlongs in length (gg). And where it is of a leſs thickneſs, the attraction may be proportionally greater, and continue to increaſe, until the thickneſs do not exceed that of a ſingle particle of the oil. There are therefore agents in Nature able to make the par-

(gg) cubic foot of water, as it is ſtated in the Principia, Lib. II. Sect. VIII. Schol.

ticles

Elective

ticles of bodies ſtick together by very ſtrong attractions. And it is the buſineſs of Experimental Philoſophy to find them out.

Now the ſmall particles of matter may cohere by the ſtrongeſt attractions, and compoſe bigger particles of weaker virtue; and many of theſe may cohere and compoſe bigger particles, whoſe virtue is ſtill weaker; and ſo on for divers ſucceſſions, until the progreſſion end in the biggeſt particles, on which the operations in chemiſtry, and the colours of Natural bodies depend; and which, by adhering, compoſe bodies of a ſenſible magnitude. If the body is compact, and bends or yields inward to preſſion without any ſliding of its parts, it is Hard and Elaſtick, returning to its figure with a force riſing from the mutual attraction of its parts. If the parts ſlide upon one another, the body is Malleable or Soft. If they ſlip eaſily, and are of a fit ſize to be agitated by heat, and the heat is big enough to keep them in agitation, the body is Fluid; and if it be apt to ſtick to things, it is Humid; and the drops of every Fluid affect a round figure, by the mutual attraction of their parts, as the globe of the earth and ſea affects a round figure, by the mutual attraction of its parts by gravity.

Since metals diſſolved in acids attract but a ſmall quantity of the acid, their Attractive force can reach but to a ſmall diſtance from them. And as in Algebra, where Affirmitive quantities vaniſh and ceaſe, there Negative ones begin; ſo in Mechanicks, where Attraction ceaſes, there a Repulſive virtue ought to ſucceed. And that there is ſuch a virtue, ſeems to follow from the reflexions and inflexions of the rays of light. For the rays are repelled by bodies, in both theſe caſes, without the immediate contact of the reflecting or inflecting body. It ſeems alſo to follow from the emiſſion of light; the ray, ſo ſoon as it is ſhaken off from a ſhining body by the vibrating motion of the parts of the body, and gets beyond the reach of attraction, being driven away with exceeding great velocity. For that force, which is ſufficient to turn it back in reflexion, may be ſufficient to emit it. It ſeems alſo

[hh] —— *quantity for quantity.*] Here followed in Dr. *Clarke's* firſt *Latin*. Ex eo, quòd particulæ Aeris à ſe invicem & à corporibus denſis recedere conentur, fit etiam, ut Aer rarior ſit in tubis vitreis exilibus, quàm in amplioribus ſpatiis; & per raritatem illam vi minore, quàm apertus Aer,

alſo to follow from the production of air and vapour: the particles, when they are ſhaken off from bodies by heat or fermentation, ſo ſoon as they are beyond the reach of the attraction of the body, receding from it, and alſo from one another, with great ſtrength; and keeping at a diſtance, ſo as ſometimes to take up above a million of times more ſpace than they did before, in the form of a denſe body. Which vaſt contraction and expanſion ſeems unintelligible, by feigning the particles of air to be ſpringy and ramous, or rolled up like hoops, or by any other means than a Repulſive power. The particles of fluids, which do not cohere too ſtrongly, and are of ſuch a ſmallneſs as renders them moſt ſuſceptible of thoſe agitations which keep liquors in a *fluor*, are moſt eaſily ſeparated and rarified into vapour; and in the language of the chemiſts, they are volatile, rarifying with an eaſy heat, and condenſing with cold. But thoſe which are groſſer, and ſo leſs ſuſceptible of agitation, or cohere by a ſtronger attraction, are not ſeparated without a ſtronger heat, or perhaps not without fermentation. And theſe laſt are the bodies, which chemiſts call Fixed, and being rarified by fermentation, become true permanent air: thoſe particles receding from one another with the greateſt force, and being moſt difficultly brought together, which upon contact adhere moſt ſtrongly. And becauſe the particles of permanent air are groſſer, and ariſe from denſer ſubſtances than thoſe of vapours; thence it is that true air is more ponderous than vapour, and that a moiſt atmoſphere is lighter than a dry one, quantity for quantity (hh). From the ſame Repelling power it ſeems to be, that flies walk upon the water without wetting their feet; and that the object-glaſſes of long teleſcopes lie upon one another without touching; and that dry powders are difficultly made to touch one another ſo as to ſtick together, unleſs by melting them, or wetting them with water, which by exhaling may bring them together; and that two poliſhed marbles, which by immediate contact ſtick together, are difficultly brought ſo cloſe together as to ſtick. Attractions.

Aer, premat ſuperficiem aquæ in quam inferior tubuli demerſa ſit extremitas; idoque permittat ut aqua illac in tubulam aſcendat: quæ eſt Philtrationis cauſa, uti olim explicavit D. *Hookius*. Porro eidem vi repellenti tribuendum videntur, quòd Muſcæ, &c.

Neceſſity of active principles

And thus Nature will be very conformable to herſelf, and very ſimple; performing all the great motions of the heavenly bodies by the attraction of gravity, which intercedes thoſe bodies; and almoſt all the ſmall ones of their particles, by ſome other Attractive and Repelling powers, which intercede the particles. The *vis inertiæ* is a paſſive principle, by which bodies perſiſt in their motion or reſt; receive motion in proportion to the force impreſſing it, and reſiſt as much as they are reſiſted. By this principle alone there never could have been any motion in the world. Some other principle was neceſſary for putting bodies into motion; and now they are in motion, ſome other principle is neceſſary for conſerving the motion. For from the various compoſition of two motions, it is very certain that there is not always the ſame Quantity of motion in the world. For if two globes, joined by a ſlender rod, revolve about their common center of gravity with an uniform motion, while that center moves on uniformly in a right line drawn in the plane of their circular motion; the ſum of the motions of the two globes, as often as the globes are in the right line deſcribed by their common center of gravity, will be bigger than the ſum of their motions, when they are in a line perpendicular to that right line ([ii]). By this inſtance it appears, that motion may be got or loſt. But by reaſon of the tenacity of fluids, and attrition of their parts, and the weakneſs of elaſticity in ſolids, motion is much more apt to be loſt than got, and is always upon the decay. For bodies, which are either abſolutely hard, or ſo ſoft as to be void of elaſticity, will not rebound from one another. Impenetrability makes them only ſtop. If two equal bodies meet directly *in Vacuo*, they will, by the laws of motion, ſtop where they meet, and loſe all their motion, and remain in reſt; unleſs they be Elaſtick, and receive new motion from their ſpring. If they have ſo much elaſticity as ſuffices to make them rebound with a quarter, or half, or three-quarters of the force with which they come together, they will loſe three-quarters, or half, or a quarter of their motion. And this may be

([ii]) The contrary ſeems to be true; that the ſum of the motions will be the greateſt, when the rod connecting the revolving bodies is perpendicular to the right line, along which the common center of gravity is moved. But in either way the different quantity of that ſum of motion, in

be tried, by letting two equal pendulums fall againſt one another from equal heights. If the pendulums be of lead or ſoft clay, they will loſe all, or almoſt all, their motions: if of Elaſtick bodies, they will loſe all but what they recover from their Elaſticity (kk). If it be ſaid, that they can loſe no motion, but what they communicate to other bodies; the conſequence is, that *in Vacuo* they can loſe no motion, but when they meet, they muſt go on and penetrate one another's dimenſions (kk). If three equal round veſſels be filled, the one with water, the other with oil, the third with molten pitch, and the liquors be ſtirred about alike to give them a vortical motion; the pitch, by its tenacity, will loſe its motion quickly; the oil, being leſs tenacious, will keep it longer; and the water, being leſs tenacious, will keep it longeſt, but yet will loſe it in a ſhort time. Whence it is eaſy to underſtand, that if many contiguous *vortices* of molten pitch were each of them as large as thoſe, which ſome ſuppoſe to revolve about the ſun and fixed ſtars; yet theſe, and all their parts, would, by their tenacity and ſtiffneſs, communicate their motion to one another, till they all reſted among themſelves. *Vortices* of oil or water, or ſome fluider matter, might continue longer in motion; but unleſs the matter were void of all tenacity and attrition of parts, and communication of motion (which is not to be ſuppoſed) the motion would conſtantly decay. Seeing therefore the variety of motion, which we find in the world, is always decreaſing; there is a neceſſity of conſerving and recruiting it by active principles: ſuch as are the cauſe of gravity, by which planets and comets keep their motions in their orbs, and bodies acquire great motion in falling; and the cauſe of fermentation, by which the heart and blood of animals are kept in perpetual motion and heat; the inward parts of the earth are conſtantly warmed, and in ſome places grow very hot; bodies burn and ſhine; mountains take fire; the caverns of the earth are blown up; and the ſun continues violently hot and lucid, and warms all things by his light. For we meet with very little motion in the world, beſides what

to prevent the decay of Motion.

in theſe two poſitions of the rod, equally makes for our Author's aſſertion. Of which, perhaps, there is yet a more ſtriking proof in the prodigious generation of motion by the colliſion of Elaſtic Bodies in certain arrangements. Vid. Hugen. *De Motu Corporum ex percuſſione.*

(kk) *If it be ſaid—Dimenſions.*] Nothing anſwering to this ſentence in Dr. *Clarke's* firſt *Latin.*

is owing to theſe active principles ([ll]). And if it were not for theſe principles, the bodies of the earth, planets, comets, ſun, and all things in them, would grow cold and freeze, and become inactive maſſes; and all putrefaction, generation, vegetation and life would ceaſe, and the planets and comets would not remain in their orbs.

Of the Creation of Matter.

All theſe things being conſidered, it ſeems probable to me, that God in the beginning formed matter in ſolid, maſſy, hard, impenetrable, moveable ([mm]) particles; of ſuch ſizes and figures, and with ſuch other properties, and in ſuch proportion to ſpace ([nn]), as moſt conduced to the end for which he formed them; and that theſe primitive particles being ſolids, are incomparably harder than any Porous bodies compounded of them; even ſo very hard, as never to wear or break in pieces: no ordinary power being able to divide what God himſelf made One, in the firſt creation. While the particles continue entire, they may compoſe bodies of one and the ſame nature and texture in all ages: but ſhould they wear away, or break in pieces, the nature of things, depending on them, would be changed. Water and earth, compoſed of old worn particles and fragments of particles, would not be of the ſame nature and texture now, with water and earth compoſed of entire particles, in the beginning. And therefore that nature may be laſting, the changes of corporeal things are to be placed only in the various ſeparations, and new aſſociations, and motions of theſe permanent particles; compound bodies being apt to break, not in the midſt of ſolid particles, but where thoſe particles are laid together, and only touch in a few points.

Principles of Motion.

It ſeems to me farther, that theſe particles have not only a *Vis inertiæ*, accompanied with ſuch Paſſive laws of motion as naturally reſult from that force; but alſo that they are moved by certain Active principles, ſuch as is that of gravity, and that which cauſes fermentation, and the coheſion of bodies. Theſe principles

([ll]) —— *theſe active principles.*] In the firſt *Latin* of Dr. *Clarke* the ſentence ſtood thus. Nam admodum paulum motûs in Mundo invenimus, præterquam quod vel ex his Principiis actuoſis, vel ex imperio Voluntatis, manifeſto oritur. The next ſentence, *And if it were not for theſe principles—not remain in their orbs*, does not appear at all in the firſt *Latin*.

([mm]) —— *impenetrable, moveable.*] Impenetrabiles, inertes & mobiles. Dr. *Clarke's* firſt *Latin*.

([nn]) —— *to ſpace.*]—eoque numero et quantitate pro ratione ſpatii in quo futurum erat ut moverentur.

ples I conſider not as Occult qualities, ſuppoſed to reſult from the ſpecifick forms of things, but as general laws of Nature, by which the things themſelves are formed: their truth appearing to us by phænomena, though their cauſes be not yet diſcovered. For theſe are manifeſt qualities, and their cauſes only are occult (oo). And the *Ariſtotelians* gave the name of Occult qualities not to manifeſt qualities, but to ſuch qualities only as they ſuppoſed to lie hid in bodies, and to be the unknown cauſes of manifeſt effects: ſuch as would be the cauſes of gravity, and of Magnetick and Electrick attractions, and of fermentations, if we ſhould ſuppoſe that theſe forces, or actions, aroſe from qualities unknown to us, and uncapable of being diſcovered and made manifeſt. Such Occult qualities put a ſtop to the improvement of Natural Philoſophy, and therefore of late years have been rejected. To tell us, that every ſpecies of things is endowed with an occult ſpecifick quality, by which it acts and produces manifeſt effects, is to tell us nothing: but to derive two or three general principles of motion from phænomena, and afterwards to tell us how the properties and actions of all corporeal things follow from thoſe manifeſt principles, would be a very great ſtep in philoſophy, though the cauſes of thoſe principles were not yet diſcovered: and therefore I ſcruple not to propoſe the principles of motion above-mentioned, they being of very general extent, and leave their cauſes to be found out (PP).

An intelligent Creator.

Now by the help of theſe principles, all material things ſeem to have been compoſed of the hard and ſolid particles above-mentioned; variouſly aſſociated, in the firſt creation, by the counſel of an intelligent Agent. For it became Him who created them, to ſet them in order. And if he did ſo, it is unphiloſophical to ſeek for any other origin of the world, or to pretend that it might ariſe out of a chaos by the mere laws of Nature; though being once formed, it may continue by thoſe laws for many ages. For

verentur. Dr. *Clarke's* firſt *Latin*; and ſo the ſentence ſtands in Dr. *Clarke's* ſecond *Latin* edition, which in moſt places was ſo corrected as to agree exactly with the ſecond *Engliſh*. And to make ſenſe of the paſſage, ſomething is evidently wanting here to anſwer to the words of the *Latin*, *in quo futurum erat ut moverentur*. For to ſpeak of particles of matter as bearing proportion to ſpace indefinitely, were abſurd.

(oo) *And the Ariſtotelians——have been rejected.*] Not in Dr. *Clarke's* firſt *Latin*.

(PP) *and leave their cauſes to be found out.*] Not in Dr. *Clarke's* firſt *Latin*.

while comets move in very excentrick orbs in all manner of poſitions, blind Fate could never make all the planets move one and the ſame way in orbs concentrick, ſome inconſiderable irregularities excepted, which may have riſen from the mutual actions of comets and planets upon one another, and which will be apt to increaſe, till this ſyſtem wants a reformation. Such a wonderful uniformity in the planetary ſyſtem muſt be allowed the effect of choice. And ſo muſt the uniformity in the bodies of animals, they having generally a right and a left ſide ſhaped alike, and on either ſide of their bodies two legs behind, and either two arms, or two legs, or two wings before upon their ſhoulders; and between their ſhoulders a neck (qq) running down into a backbone, and a head upon it; and in the head two ears, two eyes, a noſe, a mouth, and a tongue, alike ſituated. Alſo the firſt contrivance of thoſe very artificial parts of animals, the eyes, ears, brain, muſcles, heart, lungs, midriff, glands, larynx, hands, wings, ſwimming bladders, natural ſpectacles, and other organs of ſenſe and motion; and the inſtinct of brutes and inſects, can be the effect of nothing elſe than the wiſdom and ſkill of a powerful ever-living Agent; who, being in all places, is more able by his will to move the bodies within his boundleſs uniform ſenſorium (rr), and thereby to form and reform the parts of the univerſe, than we are by our will to move the parts of our own bodies (ss).

God not the Soul of the World.

And yet we are not to conſider the world as the body of God, or the ſeveral parts thereof as the parts of God. He is an uniform Being, void of organs, members or parts; and they are his creatures ſubordinate to him, and ſubſervient to his will; and he is no more the ſoul of them, than the ſoul of a man is the ſoul of the ſpecies of things carried through the organs of ſenſe into the place of its ſenſation, where it perceives them by means of its immediate preſence, without the intervention of any third

(qq) —— *running down into a backbone.*] Not in the firſt *Latin* of Dr. *Clarke.*

(rr) —— *his boundleſs uniform ſenſorium*]—infinito ſuo ſenſorio. Dr. *Clarke's* firſt *Latin.*

(ss) —— *than we are by our will to move the parts of our own bodies.*]—quam Anima noſtra, quæ eſt in nobis imago Dei, voluntate ſuâ ad corporis noſtri membra movenda valet. Dr. *Clarke's* firſt *Latin.*

(tt) *And yet we are not to conſider——things themſelves.*] Not in Dr. *Clarke's* firſt *Latin.*

(uu) —— *and in ſeveral proportions to ſpace.*]—vario quoque numero & quantitate pro ratione ſpatii

third thing. The organs of ſenſe are not for enabling the ſoul to perceive the ſpecies of things in its ſenſorium, but only for conveying them thither; and God has no need of ſuch organs, he being every where preſent to the things themſelves (tt). And ſince ſpace is diviſible *in infinitum*, and matter is not neceſſary in all places, it may be alſo allowed, that God is able to create particles of matter of ſeveral ſizes and figures, and in ſeveral proportions to ſpace (uu), and perhaps of different denſities (xx) and forces, and thereby to vary the laws of Nature, and make worlds of ſeveral ſorts in ſeveral parts of the univerſe. At leaſt, I ſee nothing of contradiction in all this.

As in Mathematicks, ſo in Natural Philoſophy, the inveſtigation of difficult things by the method of analyſis, ought ever to precede the method of compoſition. This analyſis conſiſts in making experiments and obſervations, (yy) and in drawing general concluſions from them by induction, and admitting of no objections againſt the concluſions, but ſuch as are taken from experiments, or other certain truths. For hypotheſes are not to be regarded in Experimental Philoſophy. And although the arguing from experiments and obſervations by induction be no demonſtration of general concluſions; yet it is the beſt way of arguing which the nature of things admits of, and may be looked upon as ſo much the ſtronger, by how much the induction is more general. And if no exception occur from phænomena, the concluſion may be pronounced generally. But if at any time afterwards any exception ſhall occur from experiments; it may then begin to be pronounced, with ſuch exceptions as occur. By this way of analyſis we may proceed from compounds to ingredients; and from motions to the forces producing them; and in general, from effects to their cauſes; and from particular cauſes to more general ones, till the argument end in the moſt general. This is the method of Analyſis. And the Syntheſis conſiſts in aſ-

ſpatii in quo inſunt. Dr. *Clarke's* firſt and ſecond *Latin*.

(xx) How the primordial atoms, perfectly ſimple and perfectly ſolid, i. e. without pore, ſhould differ in denſity, I cannot underſtand. It is true, different figures and ſizes of the firſt ſolid atoms may produce very different denſities of the compounds ſeverally formed from them, though in the ſame degree of compoſition. Perhaps this might be our Author's meaning.

(yy) —— *and in drawing general concluſions.*] Of all that follows to the words *Exceptions as occur*, there was nothing in the firſt *Latin* of Dr. *Clarke*.

ſuming

ſuming the cauſes diſcovered, and eſtabliſhed as principles, and by them explaining the phænomena proceeding from them, and proving the explanations.

In the two firſt Books of theſe Opticks, I proceeded by this analyſis to diſcover and prove the original differences of the rays of light in reſpect of refrangibility, reflexibility, and colour; and their alternate fits of eaſy Reflexion and eaſy Tranſmiſſion; and the properties of bodies, both opake and pellucid, on which their reflexions and colours depend. And theſe diſcoveries being proved, may be aſſumed in the method of compoſition for explaining the phænomena ariſing from them: an inſtance of which method I gave in the end of the firſt Book. In this third Book I have only begun the analyſis of what remains to be diſcovered about light, and its effects upon the frame of nature; hinting ſeveral things about it, and leaving the hints to be examined and improved by the farther experiments and obſervations of ſuch as are inquiſitive. And if Natural Philoſophy in all its parts, by purſuing this method, ſhall at length be perfected; the bounds of Moral Philoſophy will be alſo enlarged. For ſo far as we can know by Natural Philoſophy what is the Firſt cauſe, what power he has over us, and what benefits we receive from him; ſo far our duty towards him, as well as that towards one another, will appear to us by the light of Nature. And, no doubt, if the worſhip of falſe gods had not blinded the heathen, their Moral Philoſophy would have gone farther than to the four Cardinal Virtues. And inſtead of teaching the tranſmigration of ſouls, and to worſhip the ſun and moon, and dead heroes; they would have taught us to worſhip our true Author and Benefactor, as their anceſtors, did under the government of *Noah* and his ſons, before they corrupted themſelves (ZZ).

(ZZ) —— *as their anceſtors—themſelves.*] Not in the firſt *Latin* of Dr. *Clarke*, nor in the ſecond *Engliſh*. In Dr. *Clarke's* ſecond *Latin* the concluding ſentence ſtands thus. Quod quidem fecerunt majores ipſorum, antequam animum moreſque ſuos corruperant. Lex enim moralis ab origine gentibus univerſis erant ſeptem illa Nonchidarum præcepta: quorum præceptorum primum erat, unum eſſe agnoſcendum ſummum Dominum Deum, ejuſque cultum non eſſe in alios transferendum. Etenim ſine hoc principio nihil eſſet virtus aliud niſi merum nomen. In the third *Engliſh* the final ſentence appeared as we now give it.

THE END OF THE OPTICS.

Fig. 1.

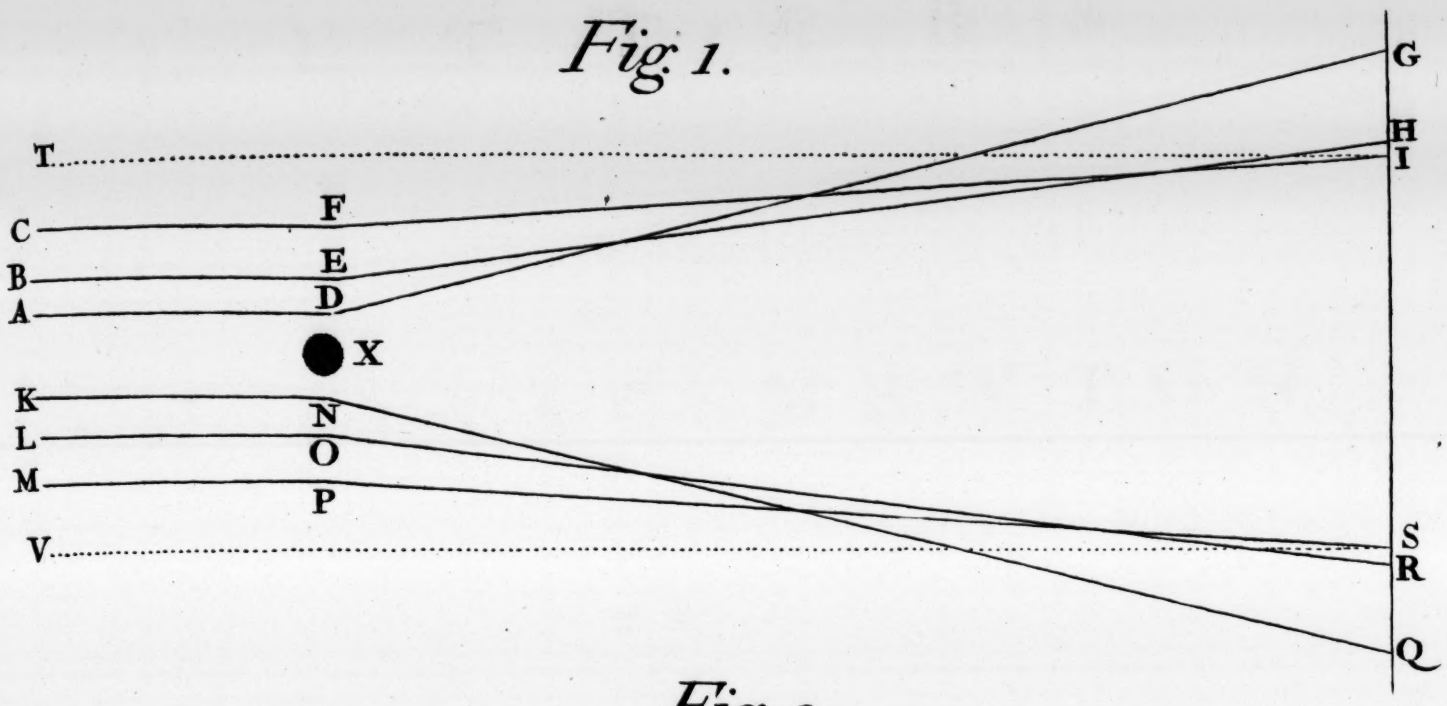


Fig. 2.

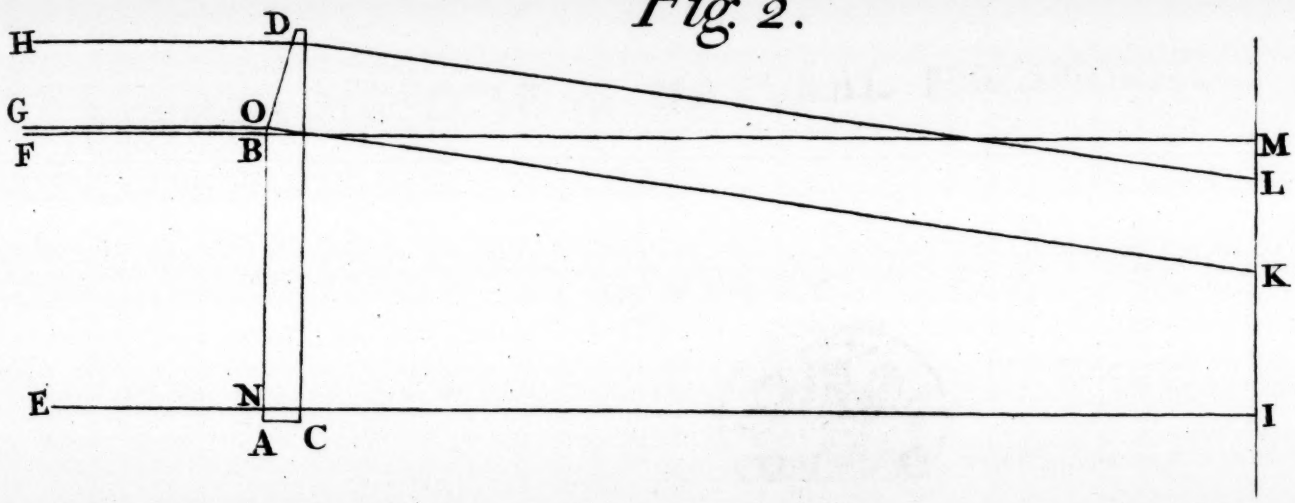
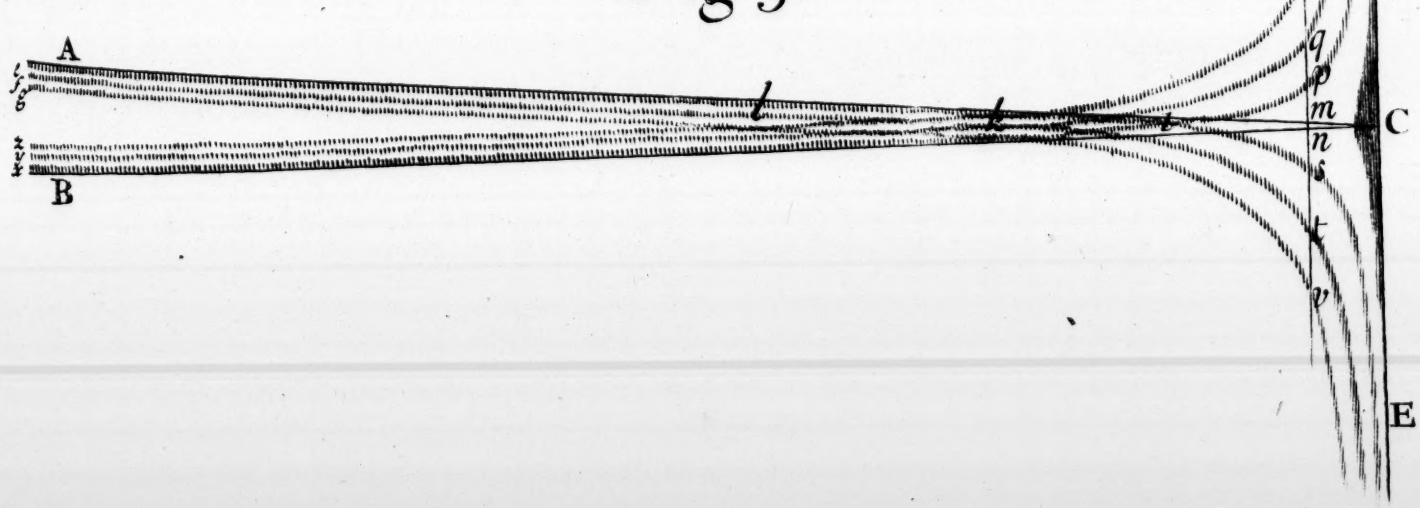


Fig. 3.



LETTERS

OF

SIR ISAAC NEWTON

ON

VARIOUS SUBJECTS IN NATURAL PHLOSOPHY.

NOW FIRST

PUBLISHED AT LENGTH FROM THE ORIGINAL PAPERS EXTANT

IN THE

ARCHIVES OF THE ROYAL SOCIETY OF LONDON.

I. *Letters relating to Reflecting Teleſcopes,* Pag. 269

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LETTERS, &c.

RELATING TO

REFLECTING TELESCOPES.

I.

To Mr. OLDENBURG.

SIR, Cambridge, March 16, 167½.

THE book, which my carrier by forgetfulneſs diſappointed me of the laſt week, I have now received, and thank you for it. With the teleſcope which I have made, I have ſometimes ſeen objects, and particularly the moon, very diſtinct in thoſe parts of it, which were near the ſides of the viſible angle. And at other times, when it hath been otherwiſe put together, it hath exhibited things, not without ſome confuſion. Which difference I attributed chiefly to ſome imperfection, that might poſſibly be, either in the figures of the metals or eye-glaſs; and once I found it cauſed by a little tarniſhing of the metal, in 4 or 5 days of moiſt weather.

One of the fellows of our College is making ſuch another teleſcope, with which laſt night I looked on Jupiter; and he ſeemed as diſtinct and ſharply defined, as I have ſeen him in other teleſcopes. When he hath finiſhed it, I will examine more ſtrictly, and ſend you an account of its performances. For it ſeems to be ſomething better than that which I made. Yours, &c.

II.

To Mr. OLDENBURG.

SIR, Cambridge, March 19, 167½.

IN my laſt letter I gave you occaſion to ſuſpect, that the inſtrument which I ſent you is, in ſome reſpect or other, indiſpoſed; or

or that the metals are tarniſhed. And by your letter of *March* 16, I am fully confirmed in that opinion. For whilſt I had it, it repreſented the moon in ſome parts of it as diſtinctly, as other teleſcopes uſually do, which magnify as much as that. Yet I very well know, that that inſtrument hath its imperfections, both in the compoſition of the metal, and in its being badly caſt; as you may perceive by a ſcabrous place near the middle of the metal on the poliſhed ſide, and alſo in the figure of the metal near that ſcabrous place. And in all thoſe reſpects, that inſtrument is capable of further improvement.

You ſeem to intimate, that the proportion of 38 to 1 holds only for its magnifying objects at ſmall diſtances. But if for ſuch diſtances, ſuppoſe 500 feet, it magnify at that rate, by the rules of Optics it muſt, for the greateſt diſtance imaginable, magnify more than $37\frac{3}{4}$ to 1; which is ſo inconſiderable a diminiſhing, that it may be even then as 38 to 1.

Here is made another inſtrument like the former, which does very well. Yeſterday I compared it with a ſix-foot teleſcope; and found it not only to magnify more, but alſo more diſtinctly. And to-day I found, that I could read in one of the Philoſophical Tranſactions, placed in the ſun's light, at an hundred foot diſtance; and that at an hundred and twenty foot diſtance, I could diſcern ſome of the words. When I made this trial, its aperture, defined next the eye, was equivalent to more than an inch and a third part of the object metal. This may be of ſome uſe to thoſe, that ſhall endeavour any thing in reflexions; for hereby they will in ſome meaſure be enabled to judge of the goodneſs of their inſtruments. I am, &c.

III.

ISAACI NEWTONI Matheſeos in Acad. Cantab. Prof. INVENTUM NOVUM quo Teleſcopia longa inſigniter contrahantur citra Effecti fraudes. Exhibitum. Soc. Reg. Lond. $167\frac{1}{2}$ *.

AB Speculum concavum metallicum, fundo Tubi adhæreus; cujus radius 14 digit. Anglicor.

CD

* This deſcription was drawn up by Mr. *Oldenburg*, by order of the Royal Society, to be tranſmitted

CD ſpeculum metallicum planum ovale, bacillo ferreo agglutinatum, et circulo æneo, intra tubi cavitatem mobili, infixum.

F lens vitrea, cujus latus ſuperius planum, inferius convexum, radius autem circa digiti ſemiſſis.

GGGG pars tubi interior, circulo æreo HI firmiter conſtricta, ita ut non facilè moveri poſſit.

*p*QKL pars tubi poſterior, circulo æneo, *p*Q, immobiliter infixa.

O uncus ferreus, circulo æneo *p*Q affixus, ultra tubi axem extenſus; cui clavus cochleatus N immiſſus tubi partem poſteriorem antrorſum pellit, vel retrahit, ad ſpeculorum debitam diſtantiam inveſtigandam, parte priore fixâ remanente.

NMGI ferrum curvatum, quod tubum ſuſtinet; globo ligneo verſatili S, clavo R affixum.

Centrum ſpeculi plani, CD, locatur in axe Tubi; ita ut in ipſum perpendicularis, à centro lentis demiſſa, cum axe angulum rectum conſtituat; et objecti ſpecies, à ſpeculo concavo in idem repercuſſa, verſus lentis focum E reflectatur.

Corona ventilogio ornamenti gratiâ impoſita, 300 circiter pedibus diſtans, cùm uno oculo huic tubo admoto ſpectabatur altero in chartam ſubjectam, 11 circiter digitis ab ipſo diſtantem, magnitudine et figurâ A inſignitâ apparebat. Tubus autem vulgaris 25 digit. longus, lente objectivâ convexâ, oculari verò utrinque concavâ (cujus radius 2. digit eſt) oculo admoto, figuram coronæ magnitudine B, eâdem obſervatâ chartæ ab oculo diſtantiâ, alteri oculo exhibebat, conſentientibus poſt repetita experimenta DD. Brounker, Wren, Hook.

IV.

To Mr. OLDENBURG.

SIR,

Cambridge, Jan. 6, $167\frac{1}{2}$.

THE deſcription of the inſtrument you ſent is very well; only the radius of the concave metal, which you put 14 inches, is more juſtly $12\frac{2}{3}$ or 13 inches; and the radius of the eye-glaſs, which you put half an inch, is the twelfth part of it, if not leſs: for the metal collects the ſun's rays at $6\frac{1}{3}$ inches diſtance, and the

tranſmitted to Mr. *Hugens* at *Paris*. The Society judging this the beſt method of ſecuring the fame of this great invention to the Author.

eye-

eye-glaſs is leſs than $\frac{1}{6}$ of an inch diſtance from its vertex. By the tools alſo to which they were ground, I know their dimenſions; and particularly meaſuring the diameter of the hemiſpherical concave, in which the eye-glaſs was ground, I find it the ſixth part of an inch.

Perhaps it may give ſome ſatisfaction to Mr. *Hugens* to underſtand, in what degree it repreſents things diſtinct, and free from colours; and to know the aperture by which it admits light. And after the words [verſus focum E reflectatur] it may not be amiſs to add this note.

Conferendo diſtantias foci iſtius à verticibus lentis et ſpeculi concavi, hoc eſt EF $\frac{1}{6}$ dig. et ETV $6\frac{1}{3}$ dig. prodit ratio 1 ad 38; quâ judicatur objecta 38 vicibus circiter ampliari.

And to this proportion is very conſentaneous the obſervation of the crown on the weather-cock. For the ſcheme repreſents it bigger by $2\frac{1}{2}$ times, when ſeen through this, than when through an ordinary perſpective. And ſo ſuppoſing that to magnify 13 or 14 times (as by the deſcription it ſhould) this by experiment proportionably muſt magnify almoſt as much as I have aſſigned it.

To the objection, that with it objects are difficultly found; I may anſwer, that that is the inconvenience of all tubes that magnify much. And that after a little uſe, the inconvenience will grow leſs; for I could readily enough find any day objects, by knowing which way they were poſited from other objects, that I accidentally ſaw in it. But in the night to find ſtars, I confeſs is troubleſome enough; yet this may be eaſily remedied by two ſights affixed to the iron rod, by which the tube is ſuſtained; and ſuch I once intended ſhould have been made, before I ſent it away from me; but that I thought the defect would not be judged material. If ſuch ſights be not found a ſufficient remedy, there may be an ordinary proſpective glaſs faſtened to the ſame frame with the tube, and directed towards the ſame object, as *Des Cartes* in his *Dioptrics* hath deſcribed, for remedying the ſame inconvenience of his beſt teleſcopes.

The plane ſide of the eye-glaſs is apt to be ſoiled with the duſt falling down upon it. And therefore the little leaden ring, put into the orifice of the bigger leaden barrel to moderate its aperture, muſt be ſometimes taken out, and the glaſs wiped with

leather done upon the ſmall end of a ſtick, or other ſuch like contrivance. But care muſt be taken, that the ſaid ring be not loſt; for without it objects appear very confuſed, at the edges of the apparent ſpace. So if the concave metal contract any dullneſs, by moiſture or otherwiſe; it ought to be taken out and rubbed with gentle leather, but not with putty, or any thing that may wear the metal.

I am very ſenſible of the honour done me by the biſhop of Sarum, in propoſing me a candidate, and which I hope will be further conferred upon me by my election into the Society. And if ſo, I ſhall endeavour to teſtify my gratitude, by communicating what my poor and ſolitary endeavours can effect, towards promoting the Philoſophical deſign. I am, &c.

V.

To Mr. OLDENBURG.

SIR, Cambridge, Jan. 18, 167½.

UNDERSTANDING, by your laſt, that ſome of the fellows of the Hon. Society, in order to a bigger reflective Teleſcope, are deviſing a fit metalline matter; let me preſume to give them this caution, that whilſt they ſeek for a white, hard and durable metalline compoſition, they reſolve not upon ſuch an one as is full of ſmall pores, only diſcoverable by a microſcope: for though ſuch an one may, to appearance, take a good poliſh, yet the edges of thoſe ſmall pores will wear away faſter in the poliſhing, than the other parts of the metal; and ſo, however the metal ſeem polite, yet it ſhall not reflect with ſuch an accurate regularity as it ought to do. Thus tin-glaſs, mixed with ordinary bell-metal, makes it more white, and apt to reflect a greater quantity of light; but withal, its fumes raiſed in the fuſion, like ſo many aerial bubbles, fill the metal full of the microſcopical pores. But white arſenick both blanches the metal, and leaves it ſolid, without any ſuch pores; eſpecially if the fuſion hath not been too violent. What the ſtellate regulus of *Mars* (which I have ſome-

Compoſition for the great Mirror.

Compoſition times uſed) or rather ſuch like ſubſtance, will do, deſerves particular examination.

Let me add further this intimation; that putty, or other ſuch like powder, with which it is poliſhed, by the ſharp angles of its particles, fretteth the metal, if it be not very fine, and filleth it full of ſuch ſmall holes as I ſpeak of. Wherefore care muſt be taken of that, before judgment be given, whether the metal be, throughout the body of it, porous or not.

I deſire, that in your next letter you would inform me, for what time the Society continue their weekly meetings; becauſe if they continue them for any time, I am purpoſing them, to be conſidered of and examined, an account of a Philoſophical diſcovery, which induced me to the making of the ſaid Teleſcope; and I doubt not but will prove much more grateful than the communication of that inſtrument; being in my judgment the oddeſt, if not the moſt conſiderable detection, which hath hitherto been made in the operations of Nature.

VI.

To Mr. OLDENBURG.

SIR, Cambridge, Jan. 29, 167½.

NOT having tried many proportions of the arſenick and metal, I do not affirm which is abſolutely beſt, but think there may conveniently be uſed any quantity of arſenick equalling in weight between a ſixth and an eighth part of the copper; a greater proportion making the metal brittle.

The way which I uſed is this: I firſt melted the copper alone, then put in the arſenick, which being melted, I ſtirred them a little together, bewaring, in the mean time, not to draw in breath near the pernicious fumes. After this I put in tin; and again, ſo ſoon as that was melted (which was very ſuddenly) I ſtirred them well together, and immediately poured them off.

I know not, whether by letting them ſtand longer on the fire after the tin was melted, a higher degree of fuſion would have made the metal porous; but I thought that way I proceeded ſafeſt.

In

In that metal, which I ſent to *London*, there was no arſenick, but a ſmall proportion of ſilver; as I remember, one ſhilling in three ounces of metal. But I thought the ſilver did as much harm in making the metal ſoft, and ſo leſs fit to be poliſhed, as good in rendering it white and luminous. for the large Mirror.

At another time I mixed arſenick one ounce, copper ſix ounces, and tin two ounces; and this an acquaintance of mine hath poliſhed better than I did the other. I am, &c.

VII.

To Mr. OLDENBURG.

SIR, Cambridge, Feb. 20, 167½.

I Received your's February 17th. And having conſidered Mr. *Hook's* obſervations on my diſcourſe, am glad that ſo acute an objector hath ſaid nothing that can enervate any part of it. For I am ſtill of the ſame judgment, and doubt not but that upon ſeverer examinations it will be found as certain a truth, as I have aſſerted it. You ſhall very ſuddenly have my anſwer.

In Mr. *Hugenius's* letter there are ſeveral handſome and ingenious remarks: and what he ſays concerning the grinding of Parabolical Conoids by geometrical rules, I do with him deſpair of. But I doubt not but that the thing may be in ſome meaſure accompliſhed by mechanical deviſes. This is all at preſent from, yours, &c.

VIII.

To Mr. OLDENBURG.

SIR, March 26, 1672.

ABOUT ten days ſince, at night, I ſaw a dull ſtar ſouth-weſt of *Perſeus*, which I now take to have been that comet of which you gave me information; but it was very ſmall, and had not any viſible tail, which made me regard it no further, and I fear it will now be difficult to find it. A Comet.

Performance and imperfections of the metalline Reflector.

Since my laſt letter, I have further compared, and find that of metal to repreſent as well the moon as nearer objects, ſomething diſtincter than the other. But I muſt tell you alſo, that I find that other (which I borrowed to make the compariſon) to be none of the beſt in the kind, and therefore would not have you rely on the obſervations made with it, but rather eſtimate the performances of the metalline teleſcope by the diſtances of between 100 and 120 feet, at which I and others could read in the Tranſactions, as I found by meaſure; at which time the aperture was $1\frac{1}{3}$ of an inch, which I knew by trying, that an obſtacle of that breadth was requiſite to intercept all the light which came from one point of the object. I ſhould tell you alſo, that the little plain piece of metal next to the glaſs is not truly figured, whereby it happens, that objects are not ſo diſtinct at the middle as at the edges. And, I hope, that by correcting its figure (in which I find more difficulty than one would expect) they will appear all over diſtinct and diſtincter in the middle than at the edges. And I doubt not, but then the performances will be greater. But yet I find, that there is more light loſt by reflection of the metal I have hitherto uſed than by tranſmiſſion through glaſſes, for which reaſon a ſhallower charge would probably do better for obſcure objects, ſuppoſe ſuch a one as would make it magnify 34 or 32 times; but of bright objects at any diſtance it ſeems capable of magnifying 38 or 40 times with ſufficient diſtinctneſs; and for all objects the ſame charge, I believe, may be allowed, if the ſteely matter employed at *London* be more ſtrongly reflective, than this which I have uſed.

Lengths, apertures, and charges.

The performances of one of theſe inſtruments of any length being known, it will appear, by this following Table, what may be expected from thoſe of other lengths by this way, if art can accompliſh what is promiſed by the theory. In the firſt column is expreſſed the length of the teleſcope in feet, which doubled, gives the ſemidiameter of the ſphere on which the concave metal is to be ground. In the ſecond columns are the proportions of the apertures for thoſe ſeveral lengths; the third column are the proportions of the charges or diameters of the ſpheres on

which

Lengths.	Apertures.	Charges.
½	100	100
1	168	119
2	283	141
3	383	157
4	476	168
5	562	178
6	645	186
8	800	200
10	946	211
12	1084	221
16	1345	238
20	1591	254
24	1824	263

Lengths, apertures, and charges.

which the convex ſuperficies of the eye-glaſſes are to be ground.

The uſe of this Table will beſt appear by example. Suppoſe, therefore, a half-foot teleſcope may diſtinctly magnify 30 times with an inch aperture, and be it required to know what ought to be the analogous conſtitution and performance of a four-foot teleſcope: by the ſecond column, as 100 to 476, ſo are the apertures; as alſo the numbers of times which they magnify. And conſequently ſince the half-foot tube hath an inch aperture, and magnifyeth 30 times; a four-foot tube proportionally ſhould have $4\frac{76}{100}$ inches aperture, and magnify 143 times. And by the third column, as 100 to 168, ſo are their charges. And therefore if the diameter of the convexity of the eye-glaſs for a half-foot teleſcope be $\frac{1}{5}$ of an inch, that for a four ſhould be $\frac{168}{500}$; that is, about $\frac{1}{3}$ of an inch. In like manner if a half-foot teleſcope may diſtinctly magnify 36 times with $1\frac{1}{4}$ of an inch aperture, a four-foot teleſcope ſhould with equal diſtinctneſs magnify 171 times with 6 inches aperture. And one of ſix foot ſhould magnify 232 times with $8\frac{2}{3}$ inches aperture; and ſo of other lengths. But what the event will really be, we muſt wait to ſee determined by experience; only this I thought fit to inſinuate, that they which intend to make trials in other lengths, may more readily know how to deſign their inſtruments. Thus for a four-foot tube, ſince the aperture ſhould be 5 or 6 inches, there will be required a piece of metal 7 or 8 inches broad at leaſt; becauſe the figure will ſcarcely be true to the edges: and the thickneſs of the metal muſt be proportional to the breadth, leaſt it bend in the grinding. The metals being poliſhed, there may be trials made with ſeveral eye-glaſſes to find what charge may, with beſt advantage, be made uſe of. Thus much of theſe teleſcopes; and at preſent I ſhall trouble you no farther, than to thank you for your laſt intelligence, by which you have obliged, Sir,

&c.

IX.

IX.

To Mr. OLDENBURG.

SIR, March 30, 1672.

Advantages of Reflectors.

I Doubt not but Monſ. *Auzout* will allow the advantage of reflections in the theory to be very great, when he ſhall be informed himſelf of the different refrangibility of the ſeveral rays of light; and for the pratique part, it is in ſome meaſure manifeſt, by the inſtruments already made, to what degree of vivacity and brightneſs a metalline ſubſtance may be poliſhed: nor is it improbable, but that there may be new ways of poliſhing found out for metal, which will far excel thoſe that are yet in uſe. And when a metal is once well poliſhed, it will be a long while preſerved from tarniſhing, if diligence be uſed to keep it dry, and cloſe ſhut up from the air. For the principal cauſe of tarniſhing ſeems to be the condenſing of moiſture on its poliſhed ſurface; which, by an acid ſpirit wherewith the atmoſphere is impregnated, corrodes and ruſts it; or at leaſt at its exhaling, leaves it covered over with a thin ſkin, conſiſting partly of an earthy ſediment of that moiſture, and partly of the duſt which, flying to and fro in the air, had ſettled and adhered to it. When there is not occaſion to make frequent uſe of the inſtruments, there may be other ways to preſerve the metals for a long time, as perhaps by immerging them in ſpirit of wine, or ſome other convenient liquor. And if they chance to tarniſh, yet their poliſh may be recovered by rubbing them with a ſoft piece of leather, or other tender ſubſtance, without the aſſiſtance of any fretting powders, unleſs they happen to be ruſty, for then they muſt be new poliſhed. I am very ſenſible, that metal reflects leſs light than glaſs tranſmits; and for that inconvenience I gave you a remedy in my laſt letter, by aſſigning a ſhallower charge in proportion to the aperture than is uſed in other teleſcopes. But as I have found ſome metalline ſubſtances to be more ſtrongly reflective, and to poliſh better, and to be freer from tarniſhing than others, ſo I hope there may be in time found out ſome ſubſtance much freer from thoſe inconveniences, than any yet known.

In the mean time, to remedy in ſome meaſure theſe inconveniences, I ſhall propound a way of uſing, inſtead of the little oval metal, a glaſs or chryſtal figured like a triangular priſm, as you ſee it repreſented in the firſt ſcheme by the figure A, B, C. Its ſide AB*ba* I ſuppoſe to perform the office of that metal, by reflecting towards the eye-glaſs the light which comes from the concave DE, which light I ſuppoſe to enter into this priſm at its ſide CB*bc* and CB*bc*; and leſt any colours ſhould be produced by the refraction of thoſe planes, it is requiſite that the angles of the priſm at A*a* and B*b* be preciſely equal. Which may be moſt conveniently performed by making them half right angles, and conſequently the third angle at C*c* a right one. The plane AB*ba*, without being foliated, will reflect all the light incident on it, eſpecially if the priſm be made of chryſtal. But to exclude all unneceſſary light, it is convenient that it be all over covered with ſome black ſubſtance, excepting two circular ſpaces of the planes AC and B*c* for the uſeful light to paſs through, as you ſee it deſigned in the ſecond Scheme. The length of this priſm ſhould be ſuch, that its ſides A*c* and B*c* may be four ſquare; and ſo much of the angles B and *b* as are ſuperfluous ought to be ground off, to give paſſage to as much light as is poſſible from the object to the concave. There is one very conſiderable advantage of this priſm which the oval metal is not capable of, without uſing two eye-glaſſes, and it is that of its ſides AC*ca* and BC*cb* be ground convex lens. The manner you have expreſſed in the third Scheme, where ſuppoſe G to be the focus of the concave, and F of the eye-glaſs at which the rays croſs twice before their arrival at the eye. But it is convenient, that the firſt trials be made with priſms whoſe ſides are all of them plane. And thus much concerning Monſ. *Auzout's* conſiderations.

Remedy of want of Light.

To the queries of Monſ. *Denys*, I anſwer, 1. That a tube of ſix inches is capable of bearing an aperture (limitted next the eye) ſo large, that an obſtacle of $1\frac{1}{4}$ or $1\frac{1}{3}$ of an inch in breadth, ſhall be requiſite to intercept all the light coming from one point of the object towards the concave metal.

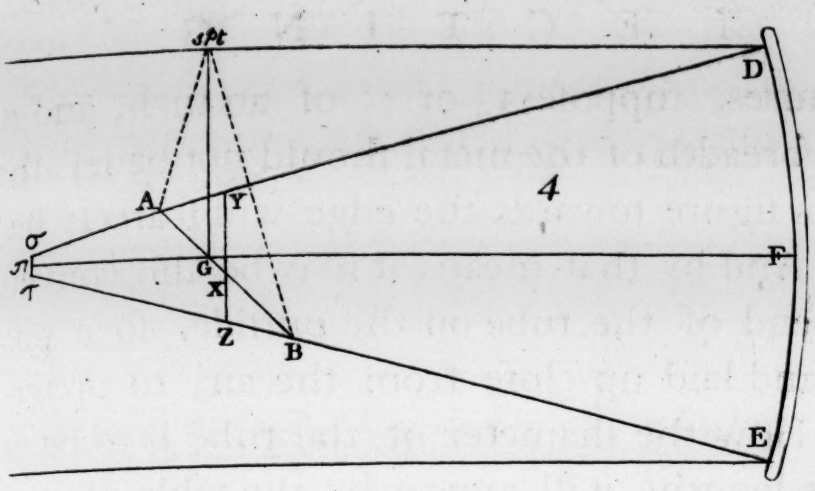
But it is convenient, that the tube be a little wider than that aperture

Adjuſtment of the diameter

of the Tube to its Length.

aperture preciſely requires, ſuppoſe $1\frac{1}{2}$ or $1\frac{2}{3}$ of an inch, and not more; and the whole breadth of the metal ſhould not be leſs than two inches, becauſe its figure towards the edge will ſcarcely be ſo true as to be uſeful. And by that means it may be alſo conveniently faſtened to the end of the tube on the outſide, ſo at pleaſure to be taken off, and laid up cloſe from the air, to preſerve it from tarniſhing. How the diameter of the tube is to be enlarged according to its length, will appear by the table of apertures and charges which I ſent you in my laſt letter of *March* 28; namely, the cube of its length ſhould be proportional to the ſquare of its diameter or aperture at the metal. So that the advantage of augmenting the length of tubes, is by this way far greater than by refractions, where their length ought to be proportional to the ſquare of the diameter of the aperture. 2. The breadth, or ſhorteſt diameter of the little oval metal for a tube of ſix inches, ſhould not be greater than $\frac{1}{3}$, nor leſs than $\frac{1}{4}$ of an inch. And the longeſt diameter ſhould be to the ſhorteſt, as about 10 to 7. But you may more exactly determine theſe diameters for tubes of all lengths after this manner. In the fourth figure, let AB repreſent the oval ſet edgwiſe; DE, the concave; FG, its axis; GP, the reflex of that axis; *st*, the diameter of the hole through which the light is tranſmitted to the eye, and *p* the center of that hole. Produce F, G to π, ſo that Gπ may be equal to G*p*; erect $\pi\sigma$ and $\pi\tau$ equal to *ps* and *pt*; and from σ and τ draw two lines, σD and τE, to the outmoſt parts of the concave within the tube, interſecting AB in A and B; and AB ſhall be the long diameter of the oval, which biſect in X; and perpendicular to FX erect XY and XZ, occurring with σd and τE in Y and Z; and a mean proportional between XY and XZ, doubled, ſhall be the other ſhort diameter: for by viewing the Scheme you will eaſily perceive, that an oval, deſcribed with thoſe rectangular conjugate diameters, is of ſufficient bigneſs to reflect all the uſeful light towards the eye, if it be rightly placed in the tube; and a broader metal would not only intercept ſo many of the beſt rays, but ſome of the ſcattering light, reflected every way from its ſuperfluous parts, would fall on the eye-glaſs, and make the object appear ſomething confuſed,

and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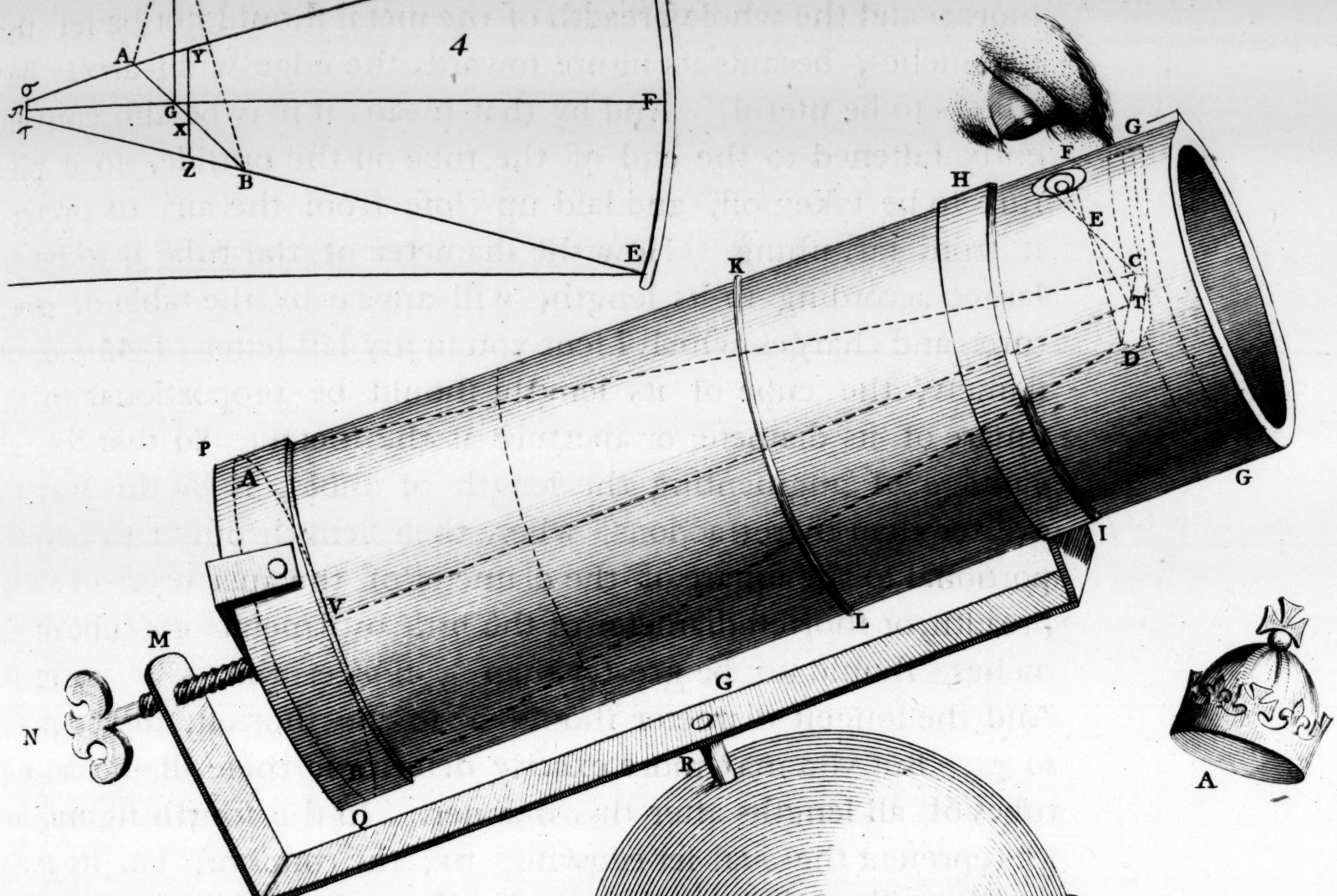
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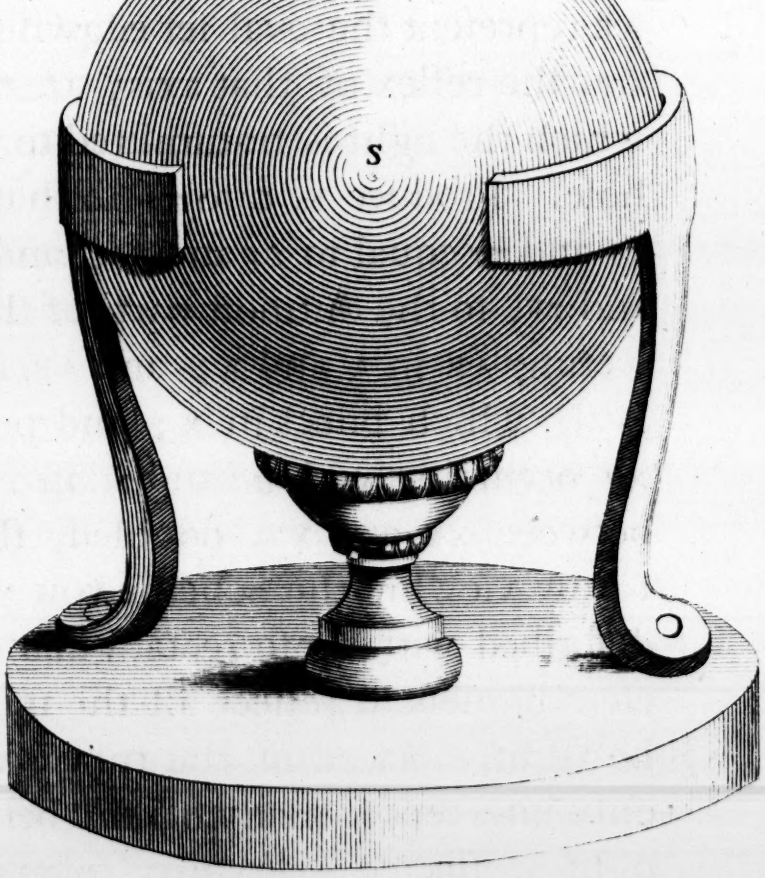
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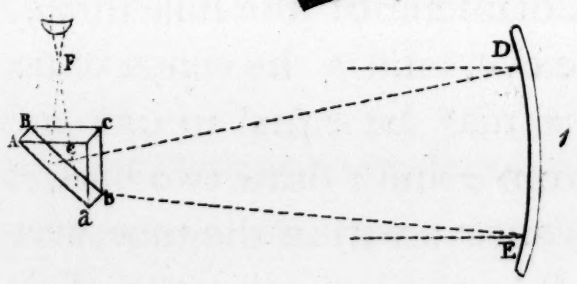
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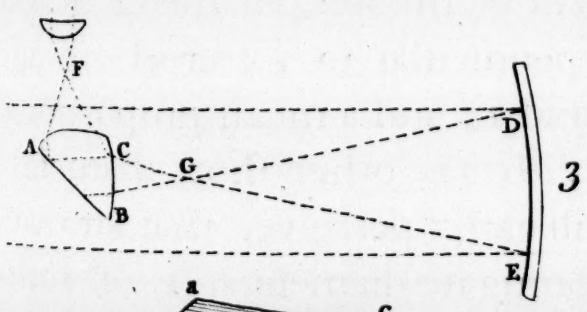
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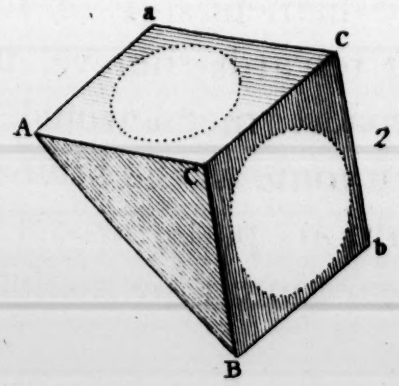
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and as it were in a miſt. This, Sir, is that which, in anſwer to your letter, my preſent thoughts ſuggeſt to your faithful ſervant,

&c.

X.

To Mr. OLDENBURG.

SIR, May 4, 1672.

Of Mr. Caſſegrain's Teleſcope.

I Should be glad to meet with any improvement of the catadioptrical teleſcope, but that deſign of it which, as you inform me, M. *Caſſegrain* hath communicated three months ſince, and now printed in the *French* Journal, I fear will not anſwer expectation; for when I firſt applied myſelf to try the effects of reflections, Mr. *Gregory's Optica Promota* (printed in the year 1663) being fallen into my hands, where there is an inſtrument deſcribed (pag. 94) like that of Mr. *Caſſegrain*, with a hole in the midſt of the object-glaſs, to tranſmit the light to an eye-glaſs placed behind it; I had thence an occaſion of conſidering that ſort of conſtructions, and found their diſadvantages ſo great, that I ſaw it neceſſary, before I attempted any thing in the practice, to alter the deſign of them, and place the eye-glaſs at the ſide of the tube, rather than at the middle.

Diſadvantages of the Gregorian Conſtruction.

The diſadvantages of it you will underſtand by theſe particulars: 1. There will be more light loſt in the metal, by reflexion from the little convex ſpeculum, than from the oval plane: for it is an obvious obſervation, that light is moſt copiouſly reflected from any ſubſtance when incident moſt obliquely. 2. The convex ſpeculum will not reflect the rays ſo truly as the oval plane, unleſs it be of an hyperbolic figure, which is incomparably more difficult to form than a plane; and, if truly formed, yet would only reflect thoſe rays truly which reſpect the axis. 3. The errors of the ſaid convex will be much augmented by the too great diſtance through which the rays reflected muſt paſs, before their arrival at the eye-glaſs; for which reaſon, I find it convenient to make the tube no wider than is neceſſary, that the eye-glaſs may be placed as near to the oval plane as is poſſible, without obſtructing any uſeful light in its

its paſſage to your object-metal. 4. The errors of the object-metal will be more augmented by reflexion from the convex than from the plane, becauſe of the inclination or deflexion of the convex on all ſides from the points on which every ray ought to be incident. 5thly. For theſe reaſons there is requiſite an extraordinary exactneſs in the figure of the little convex; whereas I find by experience, that it is much more difficult to communicate an exact figure to ſuch ſmall pieces of metal, than to thoſe that are greater. 6. Becauſe the errors at the perimeter of the concave object-metal cauſed by the ſphericalneſs of its figure, are very much augmented by the little convex; it will not, without indiſtinctneſs, bear ſo large an aperture, as in the other conſtruction. 7. By reaſon that the little convex conduces very much to the magnifying virtue of the inſtrument which the oval plane doth not, it will magnify much more in proportion to the ſphere on which the great concave is ground, than in the other deſign. And ſo magnifying objects much more than it ought to do in proportion to its aperture, it muſt repreſent them very obſcure and dark; and not only ſo, but alſo confuſed, by reaſon of its being overcharged. Nor is there any convenient remedy for this; for if the little convex be made of a larger ſphere, that will cauſe a greater inconvenience, by intercepting too many of the beſt rays; or if the charge of the eye-glaſs be made ſo much ſhallower as is neceſſary, the angle of viſion will thereby become ſo little, that it will be very difficult and troubleſome to find an object; and of that object, when found, there will be but a very ſmall part ſeen at once.

Diſadvantages of Caſſegrain's Conſtruction.

By this you may perceive, that the three advantages which Monſ. *Caſſegrain* propounds to himſelf are rather diſadvantages; for, according to his deſign, the aperture of the inſtrument will be but ſmall, the object dark and confuſed, and alſo difficult to be found. Nor do I ſee why the reflexion is more upon the ſame axis, and ſo more natural in one caſe than in the other, ſince the axis itſelf is reflected towards the eye by the oval plane. And the eye may be defended from external light as well at the ſide as at the bottom of the tube. You ſee, therefore, that

that the advantages of this deſign are none; but the diſadvantages ſo great and unavoidable, that I fear it never will be put in practice with good effect; and when I conſider, that by reaſon of its reſemblance with other teleſcopes, it is ſomething more obvious than the other conſtruction, I am apt to believe, that thoſe who have attempted any thing in Catoptricks, have ever tried it in the firſt place; and that their bad ſucceſs in that attempt hath been the cauſe why nothing hath been done in reflexions.

For Mr. *Gregory*, ſpeaking of theſe inſtruments in the aforeſaid book, p. 95, ſaith, *De mechanicâ horum ſpeculorum et lentium, ab aliis fruſtra tentatâ, ego, in mechanicis minùs verſatus, nihil dico*; ſo that there have been trials made of theſe teleſcopes, but yet in vain. And I am informed, that about 7 or 8 years ſince, Mr. *Gregory* himſelf, at *London*, cauſed one of ſix foot to be made by Mr. *Reive*, which I take to be according to the aforeſaid deſign deſcribed in his book, becauſe though made by a very ſkillful artiſt, yet it was without ſucceſs.

I could wiſh, therefore, M. *Caſſegrain* had tried his deſign, before he divulged it; but if for further ſatisfaction he pleaſe hereafter to try it, I believe the ſucceſs will inform him, that ſuch projects are of little moment, till they be put in practice. I am, Sir, your humble ſervant,

&c.

XI.

To Mr. COLLINS.

SIR, Dec. 10, 1672.

Of Gregory's ſecond Conſtruction.

MR. *GREGORY* is pleaſed to conſider further the moſt advantageous conſtruction of cata-dioptrical teleſcopes. And as his deſign in his *Optica Promota* excels that of Mr. *Caſſegrain* (though they differ ſo ſlightly, that I thought it not worth the while to take notice of the difference) the advantage being, that the little concave ellipſis comes nearer to a ſpherical figure than the convex hyperbola, ſo I conceive his preſent propoſal excels them both; of making the ſpeculum plane. And this I conjecture is the

way which Signior *Salvetti*, one of the grand duke's muſicians, mentioned in the laſt Tranſactions, intends to make experiment of; excepting that, inſtead of the convex eye-glaſs, he may probably ſubſtitute a concave one to erect the object. But yet I cannot think it the beſt; it being liable to the 1ſt, 3d and laſt of theſe difficulties urged againſt Mr. *Caſſegrain*; and, in my judgment, not wholly capable of the advantages which Mr. *Gregory* propounds.

Advantages of oblique Reflection.

The firſt diſadvantage was, that more light is loſt in direct than oblique reflexion. I am convinced by ſeveral obſervations, that reflexion is not made by the ſolid parts of a body, as is commonly preſumed, but by the confine of the two mediums, whereof one is within and the other without the body. And as ſtones are reflected by water, when thrown obliquely, which force their way into it, when thrown directly downwards; ſo the rays of light (whether corporeal like ſtones or not) are moſt eaſily and copiouſly reflected when incident moſt obliquely. This you may obſerve in the paſſage of light out of glaſs into air; which is reflected more and more copiouſly, as the obliquity is increaſed, until beyond a certain degree of obliquity it be wholly reflected. Alſo in the reflection of light by an imperfectly poliſhed plate of braſs or ſilver, or any other metal; you may obſerve, that the image of objects, which by direct reflexion appear dull and confuſed, appear by very oblique reflexion pretty diſtinct and vigorous. This advantage of oblique reflexion would be inconſiderable, if metal reflected almoſt all the light directly incident on it. But ſo far as I can obſerve, there is at leaſt a third part, if not the better half, of the light loſt and ſtifled in the metal at every reflexion; and it is of ſome eſtimation, if a third or fourth part of that may be redeemed, by ſetting the flat ſpeculum obliquely. As for Mr. *Gregory's* inſinuation, that direct rays have the advantage of oblique; becauſe a direct ball is directed more regularly from a rough wall than an oblique one; if he pleaſe to conſider how different are the cauſes and circumſtances of thoſe reflexions, poſſibly, upon ſecond thoughts, he may apprehend why the contrary ought to

happen in light: at leaſt the experiment of the rudely-poliſhed plate of metal may perſuade him.

The next diſadvantage, ariſing from the diſtance of the little ſpeculum from the eye-glaſs, being allowed; I paſs to the laſt: which is to this effect. That if to diminiſh the magnifying virtues of the inſtrument, the little ſpeculum be made of a larger ſphere (as it is in Mr. *Gregory's* deſign, a plane being equivalent to a ſphere whoſe center is indefinitely diſtant) that would cauſe too many of the beſt rays to be intercepted. And though in his deſign ſcarce a fourth part of the whole light be intercepted, yet thoſe rays ſeem to me of more value than twice their number next the circumference of the tube, becauſe they principally conduce to diſtinct viſion. Their loſs will be judged conſiderable by thoſe, that have thought the loſs of ſcarce the 40th part of the light in my way worthy of being objected, by reaſon that they were the beſt of the rays.

There are yet other conſiderations by which Mr. *Gregory's* tube may perhaps be thought leſs advantageous. As, that unleſs the ſpeculum, F, be made ſo broad as to intercept more than a quarter, or perhaps than a third part of the whole light, it will be difficult to enlarge the aperture, as is requiſite for viewing dull and obſcure objects: that the eye-glaſs, if placed at the bottom, will ſcarcely be well defended from the unuſeful glaring light, which in the day-time comes from objects on all ſides the flat ſpeculum; at leaſt not ſo well as by ſetting it at the ſide; and that an artificer can ſcarcely poliſh the great concave ſo truly when perforated in the middle: for the metal near the hole will be apt to wear away too faſt, as it doth near the exterior limb. And though the hole may be made after it is poliſhed, yet if the figure happen to be leſs true, or if afterwards the metal chance to tarniſh, it muſt be poliſhed again.

As for the advantages propounded by Mr. *Gregory*, I ſee not why the firſt ſhould be reckoned for one, viz. that the diſtance EF grows almoſt the one-half leſs, and therefore the errors of the concave CD are almoſt diminiſhed upon the plane F by one-half. For how much thoſe errors of the concave CD are increaſed or diminiſhed, is to be eſtimated by the divarication of the rays, not

not at the plane F, but at the focus of that concave CD. And there the errors in both caſes will be alike, provided the ſpeculum F be accurately plane. But if there be any irregularities in the figure of the ſpeculum F, they will cauſe errors ſo much greater in one caſe than in the other, as that ſpeculum is remoter from the eye-glaſs; which in large teleſcopes may be more than 15 or 20 inches.

Great advantage of the ſhortneſs of the Tube.

The other advantage, viz. that his tube will be little more than half the length of mine, I ſhould allow to be very conſiderable; if I thought, that with equal art in the mechaniſm, it would be made to do the ſame effect. The greateſt difficulty is in forming the great concave; which when once well done, perhaps it may be thought moſt advantageous to make the beſt uſe of it with a longer tube.

Convenience of varying the charge greater in the Newtonian Conſtruction.

The ſuppoſed advantage of teleſcopes with convex or concave ſpeculums, in that they may have any deſireable charge, by altering the diſtances of the eye-glaſs and ſpecula, agrees more conveniently with my deſign of the inſtrument, if that ſpeculum be made uſe of, which I deſcribed in a letter to Mr. *Oldenburg*, in anſwer to Mr. *Hugens's* conſiderations on theſe ſubjects; which poſſibly you may have ſeen. For inſtance; to double the charge, the eye-glaſs in the other way muſt be drawn out almoſt as far behind the great concave as the little ſpeculum is before it; whereby the length of the tube will be almoſt doubled. Whereas in my way it need be drawn out no farther from the ſide of the tube than a quarter of the tube's diameter. The charge may be alſo conveniently varied, by having 2 or 3 eye-glaſſes of ſeveral depths ſet in a girdle; any of which may be adjuſted to the metal F by ſliding the girdle about the tube, or by ſliding the ring within the tube to which the metal F is faſtened.

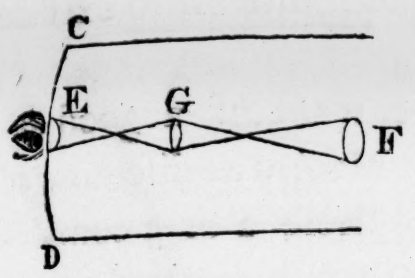
That teleſcopes with convex or concave ſpeculums ſhould be overcharged, is not neceſſary; but yet it is not avoidable without running upon one of the other two inconveniencies, deſcribed in the 7th particular of my conſiderations on Mr. *Caſſegrain's* tube, as I there intimated.

To diminiſh ſome of the aforeſaid diſadvantages, there may be ſtill new variations or additions to theſe deſigns. As, for inſtance,

by

by uſing two eye-glaſſes. Suppoſe CD repreſent the great concave; F, the little ſpeculum; E, the eye-glaſs; and G, another double convex-glaſs, between E and F, on both ſides of which the rays croſs. This way of redoubling theſe tubes ſeems not inferior to the reſt. For thus the object appears erect; the ſpeculum F intercepts leſs light, and the charge may be varied at pleaſure, only by changing the poſitions of G and F. But yet this is not without imperfections: and particularly, beſides thoſe common with the other deſigns, this glaſs G will intercept many of the beſt rays in their paſſage from the concave CD to the little ſpeculum F; unleſs it be made leſs than is conſiſtent with ſome other inconveniencies. And by the iterated decuſſations of the rays, objects will be rendered leſs diſtinct; as is manifeſt in dioptric teleſcopes, where 2 or 3 eye-glaſſes are applied to erect the object.

Advantage of a double Eye-glaſs.



As to the attempt in which Mr. *Reeves* was employed, I preſumed it had been done with much more accurateneſs than Mr. *Gregory* now ſignifies: becauſe Mr. *Hook*, who you know is a curious and accurate experimenter, affirms, in his conſiderations on my letter to Mr. *Oldenburg* concerning refractions and colours, publiſhed in the Tranſactions, No. 80, that he made ſeveral experiments with that inſtrument. And though he lays the blame on Mr. *Reeves's Encheiria*, yet he ſays not that he blamed him then, when the experiment was made. His words are theſe; "I have made ſeveral trials both for teleſcopes and microſcopes "by reflexions, which I have mentioned in my Micrography; "but deſerted it as to teleſcopes, when I conſidered, that the focus "of a ſpherical concave is not a point, but a line; and that the "rays are leſs truely reflected to a point by a concave, than re-"fracted by a convex. Which made me ſeek that by refraction, "which I found could not be expected by reflexion. Nor, in-"deed, could I find any effect of it, by one of ſix-foot radius, "which about 7 or 8 years ſince Mr. *Reeves* made for Mr. *Gre-*"*gory*, with which I made ſeveral trials. But it *now appears* "that it was for a want of a good *encheiria*, from which cauſe

"many

" many good experiments have been loſt. Both which conſide-
" rations diſcouraged me from attempting further that way; eſpe-
" cially ſince I found the parabola much more difficult to execute
" than the hyperbola or ellipſis."

From hence I might well infer, that the want of a good *encheiria* appeared not till now: and that Mr. *Hook* was diſcouraged from attempting further that way, only by theſe 2 or 3 conſiderations: that a convex, as he preſumes, refracts more truly than a concave reflects: that he found no effect by one of ſix foot radius, which till now he attributed to ſome other cauſe than the want of a good *encheiria*; namely, to the ſuppoſed leſs true reflexion of a ſpherical concave: and that he apprehended a greater difficulty of deſcribing a parabola than an hyperbola or ellipſis. Nor could I well interpret the cauſe, from which many good experiments have been loſt, to have been other than the want of a good *encheiria*; which till afterwards appears not to have been wanting. I contend not, that this was Mr. *Hook's* meaning; but only that his words ſeemed to import thus much. Which gave me occaſion to think, there was no diligence wanting in making the experiment; eſpecially ſince he expreſſeth, that he made ſeveral trials with it.

And that you may not think I ſtrained Mr. *Gregory's* ſenſe, where he ſpeaks of hyperbolic and elliptic glaſſes and ſpeculums attempted in vain; I could aſk, to what end thoſe ſpeculums were attempted, if not to compoſe optic inſtruments? Which is all I would infer from thoſe words. For that theſe inſtruments, if at all attempted, were attempted in vain, is evident by the want of ſucceſs.

This, Sir, I have ſaid, not that I deſire to diſcourage the trial of any practical way; or to contend with Mr. *Gregory* about ſo ſlender a difference. For I doubt not, but when he wrote his *Optica Promota*, he could have deſcribed more faſhions than one of theſe teleſcopes, and perhaps have run through all the poſſible caſes of them, if he had thought it worth his pains. Becauſe Mr. *Caſſegrain* propounded his ſuppoſed invention pompouſly, as if the main buſineſs was in the contrivance of theſe inſtruments; I thought fit to ſignify, that that was none of his

contrivance,

contrivance, nor ſo advantageous as he imagined. And I have now ſent you theſe further conſiderations on Mr. *Gregory's* anſwer, only to let you ſee, that I choſe the moſt eaſy and practicable way to make the firſt trials. Others may try other ways. Nor do I think it material which way theſe inſtruments are perfected, ſo they be perfected.

XII.

Mr. NEWTON'S anſwer to a letter of Mr. GREGORY'S, dated March 7, 1673, directed to Mr. COLLINS.

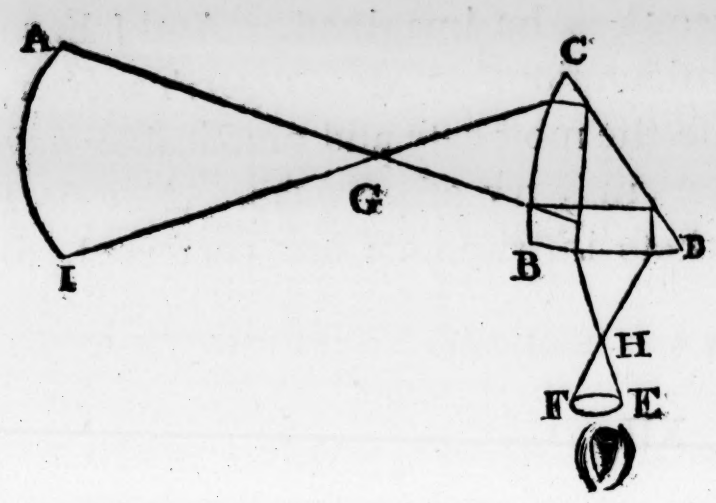
SIR, Cambridge, April 9, 1763.

HAVING peruſed Mr. *Gregory's* candid reply, I have thought good to ſend theſe further conſiderations upon the differences that ſtill are between us. And, firſt, that a well poliſhed plate reflects, at the obliquity of 45 degrees, more truly than direct ones, ſeems to me very certain. For the flat *tubercula*, or ſhallow vallies, ſuch as may be the remains of ſcratches almoſt worn out, will cauſe the leaſt errors in the obliqueſt rays, which fall on all ſides the hill, excepting on the middle of the foreſide and backſide of it; that is, where the hill inclines directly towards or directly from the ray. For if the ray fall on that ſection of the hill, its error is, in all obliquities, juſt double of the hill's declivity. But if it fall on any other part of the hill, its error is leſs than double, if it be an oblique ray; and that ſo much the leſs, by how much the ray is obliquer. But if it be a direct ray, its error is juſt double to the declivity, and therefore greateſt in that caſe. I preſume, Mr. *Gregory*, if you think it convenient to tranſmit this to him, will eaſily apprehend me.

Advantage of oblique Reflection proved.

How the charge may be varied at pleaſure in my teleſcope, will appear by this figure, where A repreſents the great concave; E, the eye-glaſs; and BCD, a priſm of glaſs or cryſtal; whoſe ſides, BC and BD, are not flat, but ſpherically convex. So that the rays which come from G, the focus of the great concave A, may, by the refraction of the firſt ſide BC, be reduced into parallelism; and after reflexion from the baſe, CD, be made, by the

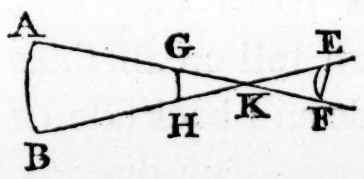
To vary the charge at pleaſure in the Newtonian conſtruction.



Limitation of the aperture of the Eye-glaſs.

refraction of the next ſide, BD, to converge to the focus of the eye-glaſs H. The teleſcope being thus formed, it appears how the charge may be altered, by varying the diſtances of the glaſſes and ſpeculum.

As for the objection, that Mr. *Gregory's* teleſcope will be either overcharged or have too ſmall an angle of viſion, &c. I apprehend, that the difference between us lies in limiting the aperture of the eye-glaſs. Mr. *Gregory* puts it equal to that of the little concave; but I ſhould rather determine it by this proportion: That if a middle point be taken between the eye-glaſs and its focus; the apertures of the eye-glaſs and concave be proportional to their diſtances from that point. That is, ſuppoſe AB the little concave; EF, the eye-glaſs; GH, their common focus or image; and K, the mean diſtance between GH and EF. From the extremityof AB draw AK, and BK, cutting on the eye-glaſs at F and E; and EF ſhall be its aperture.



Reaſon of the Limitation.

The reaſon of this limitation is, that the ſuperfluous light, which comes on all ſides of the ſpeculum, AB, to the ſpace, GH, in which the picture of the object is made, may fall beſides the eye-glaſs. For if it ſhould paſs through it to the eye, it would exceedingly blend thoſe parts of the picture with which it is mixed; and ſuch are thoſe parts of it which extend themſelves beyond the lines AK, BK: as I remember I ſaid in my former letter; that the ſcattering light, which falls on the eye-glaſs, will diſturb the viſion. And this is to be underſtood of any ſtraggling light, which comes not from the picture. But if it come from the picture to the eye-glaſs, the diſturbance will be much greater, ſo as not to be allowed of. Againſt the firſt I ſee no very convenient remedy; and againſt the laſt, none but aſſigning a ſmall aperture to the eye-glaſs, ſuppoſing the teleſcope is uſed in the day-time, or in too great light, or to view the moon, or any

ſtar

ſtar very near her, or near the brighter planets. And if for this reaſon the aperture be limitted by any rule; the angle of viſion will become very ſmall, as I affirmed: for inſtance, in that caſe when Mr. *Gregory* in his poſtſcript puts it about 20 degrees, it will be reduced to leſs than half a degree. Yet I confeſs there is a way, by which the angle of viſion may be ſomething enlarged, but it will not be very conſiderably, unleſs the eye-glaſs be alſo deeper charged.

Why I aſſign a concave with an eye-glaſs to magnify ſmall objects (in Tranſ. p. 3080) and yet an eye-glaſs without ſuch a concave to magnify the image of the great concave, which is equivalent to a ſmall object, is, becauſe that image doth not require to be magnified ſo much as an object by a microſcope: and, further, becauſe the angle of the pencil of rays, which flow from any point of the ſmall object, that the object may appear ſufficiently luminous, ought to be as great as poſſible: and a concave will with equal diſtinctneſs reflect the rays at a greater angle of the pencil than in a lens: but in the teleſcope the angles of theſe pencils are not ſo great as to tranſcend the limits, at which an eye-glaſs may, with ſufficient diſtinctneſs, refract them; and therefore in theſe inſtruments, I chooſe to lay all the ſtreſs of magnifying upon the eye-glaſs as it is well capable of; and the exceſs only upon the concave. Why the Eye-glaſs is uſed without a Concave.

Concerning my citation of Mr. *Gregory* againſt Mr. *Caſſegrain*, the force of it lies only in the inference, that optic inſtruments moſt probably, according to Mr. *Caſſegrain's* deſign, had been tried by reflexion; which I think I might well infer, without having regard to the ſpecific figure of the ſpeculum, which Mr. *Gregory* there ſpoke of. And therefore I think it cannot be ſaid, that I made him ſpeak of ſpheric figures, where his meaning was of hyperbolic or elliptic ones. But if I ſhould be ſo underſtood, becauſe I put the figure of the great concave to be ſpherical, wherever I ſpecified it; I know not why I might not, by way of conſequence, make that interpretation. For it is not probable, that any man would attempt hyperbolic and elliptic figures of ſpeculums, until the event of ſpherical ones had been firſt tried.

And accordingly the trial of Mr. *Gregory* with Mr. *Reeve* was by a ſpherical figure; which trial, although I am now ſatisfied that it was made very rudely, yet by the information that I had of it, when I wrote the letter about Mr. *Caſſegrain*, I apprehended it to have been made with great diligence and curioſity; as I ſignified in my former letter at large. And this I hope may excuſe me for ſpeaking of it in the Tranſactions, as if it had been tried with more accuracy than really it was. And thus much concerning the teleſcope.

The deſign of the burning ſpeculum appears to me very plauſible, and worthy of being put in practice. What artiſts may think of it, I know not; but the greateſt difficulty in the practice, that occurs to me, is to proportion the two ſurfaces ſo, that the force of both may be in the ſame point according to the theory; but perhaps it is not neceſſary to be ſo curious. For it ſeems to me, that the effect would ſcarce be ſenſibly leſs, if both ſides ſhould be ground to the concave and gage of the ſame tool.

LETTERS

RELATING TO THE

THEORY OF LIGHT AND COLOURS

To Mr. OLDENBURG.

SIR,

Cambridge, Feb. , 167½.

TO perform my late promiſe to you, I ſhall without further ceremony acquaint you, that in the year 1666 (at which time I applied myſelf to the grinding of optick-glaſſes of other figures than ſpherical) I procured me a triangular glaſs-priſm, to try therewith the celebrated phænomena of colours. And in order thereto, having darkened my chamber, and made a ſmall hole in my window-ſhuts, to let in a convenient quantity of the ſun's light, I placed my priſm at its entrance, that it might be thereby refracted to the oppoſite wall. It was at firſt a very pleaſing divertiſement, to view the vivid and intenſe colours produced thereby; but after a while applying myſelf to conſider them more circumſpectly, I became ſurpriſed to ſee them in an oblong form; which, according to the received laws of refractions, I expected ſhould have been circular. They were terminated at the ſides with ſtreight lines, but at the ends the decay of light was ſo gradual, that it was difficult to determine juſtly what was their figure, yet they ſeemed ſemicircular.

Of the coloured priſmatic Spectrum.

Comparing the length of this coloured *ſpectrum* with its breadth, I found it about five times greater; a diſproportion ſo extravagant, that it excited me to a more than ordinary curioſity of examining from whence it might proceed. I could ſcarce think, that the various thickneſs of the glaſs, or the termination with

Proportion of the length of the Spectrum to its breadth.

The diſproportion no effect of the confine of the ſhade.

with ſhadow or darkneſs, could have any influence on light to produce ſuch an effect: yet I thought it not amiſs, firſt to examine thoſe circumſtances, and ſo tried what would happen by tranſmitting light through parts of the glaſs of divers thickneſſes, or through holes in the window of divers bigneſſes, or by ſetting the priſm without, ſo that the light might paſs through it, and be refracted, before it was terminated by the hole: but I found none of thoſe circumſtances material. The faſhion of the colours was in all theſe caſes the ſame.

Nor of any unevenneſs in the glaſs.

Then I ſuſpected, whether by any unevenneſs in the glaſs, or other contingent irregularity, theſe colours might be thus dilated. And to try this, I took another priſm like the former, and ſo placed it, that the light paſſing through them both, might be refracted contrary ways, and ſo by the latter returned into that courſe from which the former had diverted it: for by this means I thought the regular effects of the firſt priſm would be deſtroyed by the ſecond priſm, but the irregular ones more augmented, by the multiplicity of refractions. The event was, that the light, which by the firſt priſm was diffuſed into an oblong form, was by the ſecond reduced into an orbicular one, with as much regularity as when it did uot at all paſs through them. So that whatever was the cauſe of that length, it was not any contingent irregularity.

Nor of the different incidence of rays from different parts of the Sun.

I then proceeded to examine more critically, what might be effected by the difference of the incidence of rays coming from divers parts of the ſun; and to that end, meaſured the ſeveral lines and angles belonging to the image. Its diſtance from the hole or priſm was 22 foot; its utmoſt length $13\frac{1}{4}$ inches; its breadth $2\frac{5}{8}$; the diameter of the hole $\frac{1}{4}$ of an inch. The angle which the rays, tending towards the middle of the image, made with thoſe lines, in which they would have proceeded without refraction, was 44 deg. 56 min. and the vertical angle of the priſm 63 deg. 12 min. Alſo the refractions on both ſides the priſm, that is, of the incident and emergent rays, were, as near as I could make them, equal; and conſequently about 54 deg. 4 min. And the rays fell perpendicularly upon the wall. Now ſubducting the diameter of the hole from the length and breadth

of the image, there remains 13 inches in the length, and $2\frac{3}{8}$ the breadth, comprehended by thoſe rays which paſſed through the center of the ſaid hole; and conſequently the angle of the hole, which that breadth ſubtended, was about 31 min. anſwerable to the ſun's diameter; but the angle which its length ſubtended, was more than 5 ſuch diameters, namely, 2 deg. 49 min.

Having made theſe obſervations, I firſt computed from them the refractive power of that glaſs, and found it meaſured by the ratio of the ſines 20 to 31; and then by that ratio I computed the refractions of two rays flowing from oppoſite parts of the ſun's *diſcus*, ſo as to differ 31 min. in their obliquity of incidence, and found that the emergent rays ſhould have comprehended an angle of about 31 min. as they did before they were incident.

But becauſe this computation was founded on the hypotheſis of the proportionality of the ſines of incidence and refraction, which though by my own experience I could not imagine to be ſo erroneous, as to make that angle but 31 min. which in reality was 2 deg. 49 min. yet my curioſity cauſed me again to take my priſm: and having placed it at my window, as before, I obſerved, that by turning it a little about its axis to and fro, ſo as to vary its obliquity to the light, more than an angle of 4 or 5 degrees, the colours were not thereby ſenſibly tranſlated from their place on the wall; and conſequently by that variation of incidence, the quantity of refraction was not ſenſibly varied. By this experiment, therefore, as well as by the former computation, it was evident, that the difference of the incidence of rays, flowing from divers parts of the ſun, could not make them after decuſſation diverge at a ſenſibly greater angle, than that at which they before converged; which being at moſt but about 31 or 32 min. there ſtill remained ſome other cauſe to be found out, from whence it could be 2 deg. 49 min.

Nor from a curvilinear motion of the refracted Rays.

Then I began to ſuſpect, whether the rays, after their trajection through the priſm, did not move in curve lines, and according to their more or leſs curvity, tend to divers parts of the wall. And it increaſed my ſuſpicion, when I remembered that I had often ſeen a tennis-ball, ſtruck with an oblique racket, deſcribe ſuch a curve line. For, a circular as well as a progreſſive mo-

tion being communicated to it by that ſtroke, its parts, on that ſide where the motions conſpire, muſt preſs and beat the contiguous air more violently than on the other, and there excite a reluctancy and re-action of the air proportionably greater. And for the ſame reaſon, if the rays of light ſhould poſſibly be globular bodies, and by their oblique paſſage out of one medium into another acquire a circulating motion; they ought to feel the greater reſiſtance from the ambient æther, on that ſide where the motions conſpire, and thence be continually bowed to the other. But notwithſtanding this plauſible ground of ſuſpicion, when I came to examine it, I could obſerve no ſuch curvity in them. And beſides (which was enough for my purpoſe) I obſerved, that the difference betwixt the length of the image and the diameter of the hole, through which the light was tranſmitted, was proportionable to their diſtance.

Experimentum Crucis.

The gradual removal of theſe ſuſpicions at length led me to the *experimentum crucis*, which was this. I took two boards, and placed one of them cloſe *behind the priſm at the window*, ſo that the light might paſs through a ſmall hole, made in it for the purpoſe, and fall on the other board, which I placed at about 12 feet diſtance, having firſt made a ſmall hole in it alſo for ſome of that incident light to paſs through. Then I placed another priſm behind this ſecond board, ſo that the light trajected through both the boards might paſs through that alſo, and be again refracted before it arrived at the wall. This done, I took the firſt priſm in my hand, and turned it to and fro ſlowly about its axis, ſo much as to make the ſeveral parts of the image, caſt on the ſecond board, ſucceſſively paſs through the hole in it, that I might obſerve to what places on the wall the ſecond priſm would refract them. And I ſaw, by the variation of thoſe places, that the light, tending to that end of the image towards which the refraction of the firſt priſm was made, did in the ſecond priſm ſuffer a refraction conſiderably greater than the light tending to the other end. And ſo the true cauſe of the length of that image was detected to be no other, than that *light* is not ſimilar or homogeneal, but conſiſts of *difform rays, ſome of which are more refrangible than others*; ſo that without any difference in their

their incidence on the ſame medium, ſome ſhall be more *refracted* than others; and therefore that, according to their *particular degrees of refrangibility*, they were tranſmitted through the priſm to divers parts of the oppoſite wall.

True cauſe of the imperfection of Teleſcopes.

When I underſtood this, I left off my aforeſaid glaſs-works; for I ſaw, that the perfection of teleſcopes was hitherto limited, not ſo much for want of glaſſes truly figured according to the preſcriptions of optick authors (which all men have hitherto imagined) as becauſe that *light* itſelf is an heterogeneous mixture of *differently refrangible rays*: ſo that were a glaſs ſo exactly figured as to collect any one ſort of rays into one point, it could not collect thoſe alſo into the ſame point, which, having the ſame incidence upon the ſame medium, are apt to ſuffer a different refraction. Nay, I wondered, that ſeeing the difference of refrangibility was ſo great as I found, teleſcopes ſhould arrive to that perfection they are now at: for, meaſuring the refractions in one of my priſms, I found, that ſuppoſing the common ſine of incidence upon one of its planes was 44 parts, the ſine of refraction of the utmoſt rays on the red end of the colours, made out of the glaſs into the air, would be 68 parts; and the ſine of refraction of the utmoſt rays on the other end, 69 parts; ſo that the difference is about a 24th or 25th part of the whole refraction. And, conſequently, the object-glaſs of any teleſcope cannot collect all the rays which come from one point of an object, ſo as to make them convene at its focus in leſs room than in a circular ſpace, whoſe diameter is the 50th part of the diameter of its aperture; which is an irregularity, ſome hundreds of times greater than a circularly figured *lens*, of ſo ſmall a ſection as the object-glaſſes of long teleſcopes are, would cauſe by the unfitneſs of its figure, were light uniform.

This made me take reflections into conſideration; and finding them regular, ſo that the angle of reflection of all ſorts of rays was equal to their angle of incidence, I underſtood, that by their mediation optick inſtruments might be brought to any degree of perfection imaginable, provided a reflecting ſubſtance could be found which would poliſh as finely as glaſs, and reflect as much light as glaſs tranſmits, and the art of communicating to it a parabolick

rabolick figure be alſo attained. But theſe ſeemed very great difficulties, and I have alſo thought them inſuperable, when I farther conſidered, that every irregularity in a reflecting ſuperficies makes the rays ſtray 5 or 6 times more out of their due courſe, than the like irregularities in a refracting one: ſo that a much greater curioſity would be here requiſite, than in figuring glaſſes for refraction.

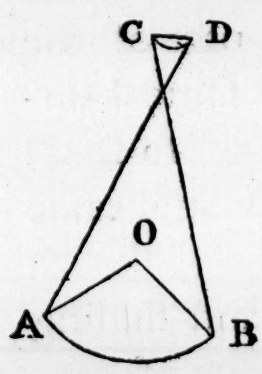
First Reflecting Teleſcope.

Amidſt theſe thoughts, I was forced from *Cambridge*, *anno* 1666, by the intervening plague, and it was more than two years before I proceeded further. But then having thought on a tender way of poliſhing, proper for metal, whereby, as I imagined, the figure would be corrected to the laſt; I began to try what might be effected in this kind, and by degrees ſo far perfected an inſtrument, in the eſſential parts of it like that I ſent to *London*, by which I could diſcern *Jupiter's* four concomitants, and ſhewed them divers times to two others of my acquaintance. I could alſo diſcern the moon-like *phaſe* of *Venus*, but not very diſtinctly, nor without ſome niceneſs in diſpoſing the inſtrument.

From that time I was interrupted till this laſt autumn, when I made another. And as that was ſenſibly better than the firſt, eſpecially for day-objects, ſo I doubt not but they will be ſtill brought to a much greater perfection by their endeavours, who, as you inform me, are taking care about it at *London*.

Deſign of a Microſcope by Reflection.

I have ſometimes thought to make a microſcope, which ſhould have, inſtead of an object-glaſs, a reflecting piece of metal. For



theſe inſtruments ſeem as capable of improvement as teleſcopes, and perhaps more; becauſe but one reflective piece of metal is requiſite in them, as you may perceive by the diagram; where AC repreſenteth the object-metal; CD, the eye-glaſs; F, their common focus; and O, the other focus of the metal, in which the object is placed.

But to return from this digreſſion, I told you, that light is not ſimilar or homogeneral, but conſiſts of difform rays, ſome of which are more refrangible than others: ſo that of thoſe which are alike incident on the ſame medium, ſome ſhall be more refracted than others; and that not by virtue of the glaſs, or other external

external cauſe, but from a pre-diſpoſition, which every particular ray hath to ſuffer refraction.

I SHALL now proceed to acquaint you with another more notable *diſformity* in its rays, wherein the origin of colours is unfolded: concerning which I ſhall lay down the doctrine firſt; and then, for its examination, give you an inſtance of two of the experiments, as a ſpecimen of the reſt. Origin of Colours.

The doctrine you will find comprehended and illuſtrated in the following propoſitions.

I. As the rays of light differ in degrees of refrangibility, ſo they alſo differ in their diſpoſition to exhibit this or that particular colour. Colours are not qualifications of light, derived from refractions, or reflections of natural bodies (as it is generally believed) but original and connate properties, which in divers rays are divers. Some rays are diſpoſed to exhibit a Red colour, and no other; ſome a Yellow, and no other; ſome a Green, and no other; and ſo of the reſt. Nor are there only rays proper and particular to the more eminent colours, but even to all their intermediate gradations.

II. To the ſame degree of refrangibility ever belongs the ſame colour, and to the ſame colour ever belongs the ſame degree of refrangibility. The leaſt refrangible rays are all diſpoſed to exhibit a Red colour; and contrarily, thoſe rays which are diſpoſed to exhibit a Red colour, are all the leaſt refrangible: ſo the moſt refrangible rays are all diſpoſed to exhibit a deep Violet colour; and contrarily, thoſe which are apt to exhibit ſuch a Violet colour, are all the moſt refrangible: and ſo to all the intermediate colours in a continued ſeries belong intermediate degrees of refrangibility. And this analogy betwixt colours and refrangibility is very preciſe and ſtrict; the rays always either exactly agreeing in both, or proportionably diſagreeing in both.

III. The ſpecies of colour, and degree of refrangibility proper to any particular ſort of rays, is not mutably by refraction, nor by reflection from natural bodies, nor by any other cauſe that I could yet obſerve. When any one ſort of rays hath been well parted from thoſe of other kinds, it hath afterwards obſtinately retained its colour, notwithſtanding my utmoſt endeavours to change

Origin of

change it. I have refracted it with priſms, and reflected it with bodies, which in day-light were of other colours; I have intercepted it with the coloured film of air, interceding two compreſſed plates of glaſs; tranſmitted it through coloured mediums, and through mediums irradiated with other ſorts of rays, and diverſly terminated it; and yet could never produce any new colour out of it. It would, by contracting or dilating, become more briſk, or faint, and by the loſs of many rays, in ſome caſes very obſcure and dark; but I could never ſee it changed in ſpecie.

IV. Yet ſeeming tranſmutations of colours may be made, where there is any mixture of divers ſorts of rays: for in ſuch mixtures, the component colours appear not; but, by their mutual allaying each other, conſtitute a middling colour. And therefore, if by refraction, or any other of the aforeſaid cauſes, the difform rays, latent in ſuch a mixture, be ſeparated, there ſhall emerge colours different from the colour of the compoſition. Which colours are not new generated, but only made apparent by being parted; for if they be again intirely mixed and blended together, they will again compoſe that colour, which they did before ſeparation. And, for the ſame reaſon, tranſmutations, made by the convening of divers colours, are not real; for when the difform rays are again ſevered, they will exhibit the very ſame colours, which they did before they entered the compoſition; as you ſee Blue and Yellow powders, when finely mixed, appear to the eye Green; and yet the colours of the component corpuſcles are not thereby really tranſmuted, but only blended. For when viewed with a good microſcope, they ſtill appear Blue and Yellow interſperſedly.

V. There are therefore two ſorts of colours, the one original and ſimple, the other compounded of theſe. The original or primary colours are, Red, Yellow, Green, Blue, and a Violet-Purple, together with Orange, Indico, and an indefinite variety of intermediate gradations.

VI. The ſame colours in ſpecie, with theſe primary ones, may be alſo produced by compoſition. For a mixture of Yellow and Blue makes Green; of Red and Yellow makes Orange; of Orange and Yellowiſh Green makes Yellow. And, in general, if

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rated by the priſm are not too far diſtant from one another; they, by their mutual alloy, compound that colour, which in the ſaid ſeries appeareth in the midway between them. But thoſe which are ſituated at too great a diſtance, do not ſo. Orange and Indico produce not the intermediate Green, nor Scarlet and Green the intermediate Yellow.

VII. But the moſt ſurprizing and wonderful compoſition was that of Whiteneſs. There is no one ſort of rays which alone can exhibit this. It is ever compounded; and to its compoſition are requiſite all the aforeſaid primary colours, mixed in a due proportion. I have often with admiration beheld, that all the colours of the priſm being made to converge, and thereby to be again mixed, as they were in the light before it was incident upon the priſm, reproduced light entirely and perfectly White, and not at all ſenſibly differing from a direct light of the ſun, unleſs when the glaſſes I uſed were not ſufficiently clear; for then they would a little incline it to their colour.

VIII. Hence, therefore, it comes to paſs, that Whiteneſs is the uſual colour of light; for light is a confuſed aggregate of rays indued with all ſorts of colours, as they were promiſcuouſly darted from the various parts of luminous bodies. And of ſuch a confuſed aggregate, as I ſaid, is generated Whiteneſs, if there be a due proportion of the ingredients; but if any one predominate, the light muſt incline to that colour; as it happens in the blue flame of brimſtone; the yellow flame of a candle; and the various colours of the fixed ſtars.

IX. Theſe things conſidered, the manner how colours are produced by the priſm is evident. For, of the rays, conſtituting the incident light, ſince thoſe which differ in colour proportionally differ in refrangibility, they, by their unequal refractions, muſt be ſevered and diſperſed into an oblong form in an orderly ſucceſſion, from the leaſt refracted Scarlet, to the moſt refracted Violet. And for the ſame reaſon it is, that objects, when looked upon through a priſm, appear coloured. For the differm rays, by their unequal refractions, are made to diverge towards ſeveral parts of the *retina*, and there expreſs the images of things coloured;

Origin of

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Origin of loured; as in the former caſe they did the ſun's image upon a wall. And by this inequality of refractions, they become not only coloured, but alſo very confuſed and indiſtinct.

X. Why the colours of the rainbow appear in falling drops of rain, is alſo from hence evident. For thoſe drops, which refract the rays diſpoſed to appear Purple in greateſt quantity to the ſpectator's eye, refract the rays of other ſorts ſo much leſs, as to make them paſs beſide it; and ſuch are the drops on the inſide of the primary bow, and on the outſide of the ſecondary or exterior one. So thoſe drops, which refract in greateſt plenty the rays apt to appear Red toward the ſpectator's eye, refract thoſe of other ſorts ſo much more, as to make them paſs beſide it; and ſuch are the drops on the exterior part of the primary, and interior part of the ſecondary bow.

XI. The odd phænomena of an infuſion of *lignum nephriticum*, leaf-gold, fragments of coloured glaſs, and ſome other tranſparently coloured bodies, appearing in one poſition of one colour, and of another in another, are on theſe grounds no longer riddles. For thoſe are ſubſtances apt to reflect one ſort of light, and tranſmit another; as may be ſeen in a dark room, by illuminating them with ſimilar or uncompounded light. For then they appear of that colour only with which they are illuminated; but yet in one poſition more vivid and luminous than in another, accordingly as they are diſpoſed more or leſs to reflect or tranſmit the incident colour.

XII. From hence alſo is manifeſt the reaſon of an unexpected experiment, which Mr. *Hook*, ſomewhere in his *Micrography*, relates to have made with two wedge-like tranſparent veſſels, filled the one with a Red, the other with a Blue liquor: namely, that though they were ſeverally tranſparent enough, yet both together became opake: for if one tranſmitted only Red, and the other only Blue, no rays could paſs through both.

XIII. I might add more inſtances of this nature, but I ſhall conclude with this general one: That the colours of all natural bodies have no other origin than this; that they are variouſly qualified to reflect one ſort of light in greater plenty than another. And this I have experienced in a dark room, by illuminating

thoſe

thoſe bodies with uncompounded light of divers colours. For by that means any body may be made to appear of any colour. They have there no appropriate colour, but ever appear of the colour of the light caſt upon them; but yet with this difference, that they are moſt briſk and vivid in the light of their own day-light colour. *Minium* appeareth there of any colour indifferently, with which it is illuſtrated; but yet moſt luminous in Red: and ſo Biſe appeareth indifferently of any colour, with which it is illuſtrated; but yet moſt luminous in Blue: and therefore *Minium* reflecteth rays of any colour, but moſt copiouſly thoſe endowed with Red; and conſequently when illuſtrated with day-light, that is, with all ſorts of rays promiſcuouſly blended, thoſe qualified with Red ſhall abound moſt in the reflected light, and by their prevalence cauſe it to appear of that colour. And for the ſame reaſon Biſe, reflecting Blue moſt copiouſly, ſhall appear Blue by the exceſs of thoſe rays in its reflected light; and the like of other bodies. And that this is the entire and adequate cauſe of their colours is manifeſt, becauſe they have no power to change or alter the colours of any ſort of rays incident apart, but put on all colours indifferently, with which they are enlightened. Colour.

Theſe things being ſo, it can be no longer diſputed, whether there be colours in the dark, or whether they be the qualities of the objects we ſee; no, nor perhaps whether light be a body. For, ſince colours are the qualities of light, having its rays for their intire and immediate ſubject, how can we think thoſe rays qualities alſo, unleſs one quality may be the ſubject of, and ſuſtain another? which, in effect, is to call it ſubſtance. We ſhould not know bodies for ſubſtances, were it not for their ſenſible qualities; and the principal of thoſe being now found due to ſomething elſe, we have as good reaſon to believe that to be a ſubſtance alſo. Light not a Quality.

Beſides, who ever thought any Quality to be a heterogeneous aggregate, ſuch as light is diſcovered to be? But to determine more abſolutely what light is, after what manner refracted, and by what modes or actions it produceth in our minds the phantaſms of colours, is not ſo eaſy: and I ſhall not mingle conjectures with certainties.

REVIEWING what I have written, I ſee the diſcourſe itſelf will lead to divers experiments ſufficient for its examination: and therefore I ſhall not trouble you farther than to deſcribe one of thoſe which I have already inſinuated.

Proof by Experiment.

In a darkened room make a hole in the ſhut of a window, whoſe diameter may conveniently be about a third part of an inch, to admit a convenient quantity of the ſun's light: and there place a clear and colourleſs priſm, to refract the entering light towards the further part of the room; which, as I ſaid, will thereby be diffuſed into an oblong coloured image. Then place a lens of about 3 foot radius, ſuppoſe a broad object-glaſs of a three foot teleſcope, at the diſtance of about 4 or 5 foot from thence, through which all thoſe colours may at once be tranſmitted, and made by its refraction to convene at a further diſtance of about 10 or 12 feet. If at that diſtance you intercept this light with a ſheet of white paper, you will ſee the colours converted into Whiteneſs again by being mingled. But it is requiſite that the priſm and lens be placed ſteady, and that the paper, on which the colours are caſt, be moved to and fro; for by ſuch motion you will not only find at what diſtance the Whiteneſs is moſt perfect, but alſo ſee how the colours gradually convene and vaniſh into whiteneſs; and afterwards, having croſſed one another in that place where they compound whiteneſs, are again diſſipated and ſevered, and, in an inverted order, retain the ſame colours which they had before they entered the compoſition. You may alſo ſee, that if any of the colours at the lens be intercepted, the whiteneſs will be changed into the other colours. And therefore, that the compoſition of whiteneſs be perfect, care muſt be taken that none of the colours fall beſides the lens. In the annexed deſign of this experiment, ABC [*vid. Lect. Opt. Par. II. Tab. III. fig.* 14.] expreſſeth the priſm ſet endwiſe to ſight, cloſe by the hole F, of the window EG. Its vertical angle, ACB, may conveniently be about 63 deg. MN deſigneth the lens: its breadth, $2\frac{1}{2}$, or 3 inches. OF, one of the ſtraight lines, in which difform rays may be conceived to flow ſucceſſively from the ſun. FP, and FT, two of thoſe rays unequally refracted, which the lens makes to converge towards X, and after decuſſation to diverge

verge again. And PT, x, π, the paper, at divers diſtances, on which the colours are projected; which in x conſtitute whiteneſs, but are Red and Yellow in T, and τ, and Blue and Purple in P, and π.

If you proceed further to try the impoſſibility of changing any uncompounded colour, which I have aſſerted in the 3d and 13th Propoſitions, it is requiſite that the room be made very dark; leſt any ſcattering light, mixing with the colour, diſturb and allay it, and render it compound contrary to the deſign of the experiment: it is alſo requiſite that there be a perfecter ſeparation of the colours, than, after the manner above deſcribed, can be made by the refraction of one ſingle priſm; and how to make ſuch further ſeparations, will ſcarce be difficult to them that conſider the diſcovered laws of refractions. But if trials ſhall be made with colours not throughly ſeparated, there muſt be allowed changes proportionable to the mixture. Thus if compound Yellow light fall upon the Blue biſe, the biſe will not appear perfectly Yellow, but rather Green: becauſe there are in the Yellow mixture many rays endued with Green; and Green, being leſs remote from the uſual Blue colour of biſe than Yellow, is the more copiouſly reflected by it. Immutability of Homogeneal Light.

In like manner, if any one of the priſmatic colours, ſuppoſe Red, be intercepted, on deſign to try the aſſerted impoſſibility of reproducing the colour out of the others which are pretermitted; it is neceſſary, either that the colours be very well parted before the Red be intercepted, or that together with the Red, the neighbouring colours, into which any Red is ſecretly diſperſed, that is, the Yellow and perhaps Green too, be intercepted: or elſe, that allowance be made for the emerging of ſo much Red out of the Yellow-green, as may poſſibly have been diffuſed, and ſcatteringly blended in thoſe colours. And if theſe things be obſerved, the new production of Red, or of any other intercepted colour, will be found impoſſible.

This I conceive is enough for introduction to experiments of this kind; which, if any of the Royal Society ſhall be ſo curious as to proſecute, I ſhall be very glad to be informed with what ſucceſs: that if any thing ſeem to be defective, or to thwart this relation,

I may have an opportunity of giving further direction about it; or of acknowledging my errors, if I have committed any.

II.

NEWTONUS OLDENBURGO S.

Reply to Ignatius Pardies.

ACCEPI obſervationes Reverendi Patris Ignatii Pardies in Epiſtolam meam de Lucis refractionibus et Coloribus ad te conſcriptam; quo nomine me illi devinctum agnoſco; atque hæc difficultatibus, quas propoſuit, diluendis reſcribo.

The length of the priſmatic image of the Sun not cauſed by unequal incidence of the Rays.

Imprimis ait longitudinem ſolaris imaginis, à refractione priſmatis refractione effectam, non aliâ indigere cauſâ, quàm diverſa radiorum ab oppoſitis partibus ſolaris diſci profluentium incidentia; adeoque non probare diverſam refrangibilitatem radiorum. Et quo aſſertionis ejus veritatem confirmet, oſtendit caſum, in quo ex diverſâ incidentiâ triginta minutorum, differentia refractionis poteſt eſſe duorum graduum cum viginti tribus minutis, vel etiam paulo major, prout exigit meum experimentum. Sed hallucinatus eſt reverendus Pater: nam refractiones à diverſâ parte priſmatis, quantum poteſt inæquales ſtatuit R. P. Pardies, cùm tamen ego tum in experimentis, tum in calculo de experimentis iſtis inito, æquales adhibuerim. Sit autem ABC * priſmatis ſectio ad axem ejus perpendicularis; XL, & YK, radii duo in F (medio foraminis) decuſſantes, & in priſma illud incidentes ad K, & L; ſintque eorum refracti KH, & LI, ac denuo HP, & IT. Et cùm refractiones ad latus AC, æquales eſſe refractionibus ad latus BC, quàm proximè ſuppoſuerim; ſi AC, & BC, ſtatuantur æqualia, ſimilis erit radiorum KH, & LI ad AB, baſin priſmatis, inclinatio; adeoque ang. CLI = ang. CHK; & ang. CIL = ang. CKH. Quare etiam refractiones in K, & I, æquales erunt; ut & in L, & H; atque adeo ang. PHA = ang. FLB; & ang. TIA = ang. BKF; & proinde refractorum HP, & IT, eadem erit ad invicem inclinatio, ac eſt incidentium radiorum FL, & FK. Sit ergo angulus KFL, 30 min. æqualis nempe ſolari diametro, & erit angulus, quem HI, & *mn*, comprehendunt, etiam 30 min. ſi modo radii FL, & FK, æqualiter refrangibiles ſtatuantur. At mihi experienti prodiit angulus ille circiter 2 gr. 49 min. quem radius HP,

* Vide Opt. Book I. Par. I. Tab. III. fig. 13.

extremum

extremum violaceum colorem, & *mn*, cæruleum exhibens, conſtituere; ac proinde radios illos diverſimode refrangibiles eſſe, ſive refractiones ſecundum diſparem ſinuum incidentiæ & refractionis rationem peragi, neceſſario concedendum eſt.

Addit præterea R. P. quòd non ſufficit ad obeundum ritè calculum, ex longitudine imaginis impactæ in chartam ſubtrahere magnitudinem foraminis feneſtræ; quandoquidem etiam poſito foramine indiviſibili, adhuc fieret aliud veluti foramen latum in poſteriori ſuperficie priſmatis. Mihi tamen videtur, his non obſtantibus, quòd refractiones radiorum, in anteriori æquè ac poſteriori ſuperficie priſmatis decuſſantium, ex adhibitis principiis poſſint ritè computari. Sed ſi res ſecus eſſet, latitudo hiatûs in poſteriori ſuperficie, quod ad inſtar foraminis eſt, haud efficeret errorem duorum minutorum ſecundorum; & in rebus practicis non operæ pretium duco ad minutias iſtas attendere.

The unequal refractions of the different colours in the *Experimentum Crucis* not cauſed by unequal incidence.

Illi inſuper experimento, quod *Crucis* vocaveram, nihil adverſatur R. P. dum contendit, inæquales radiorum, diverſis coloribus imbutorum, refractiones, ex inæqualibus incidentiis effectas fuiſſe. Nam radiis per duo admodum parva, ab invicem diſtantia, & immota foramina tranſeuntibus, incidentiæ illæ, prout ego experimentum inſtitui, omnino æquales erant, & tamen refractiones liquido inæquales. Sin ille de experimentis noſtris dubitet, oro, ut radiorum diverſis coloribus præditorum refractiones ex incidentiis paribus menſuret, & ſentiet inæquales eſſe. Si modus ille, quem ego ad hoc negotium adhibui, minùs placeat (quo tamen nullus poteſt eſſe luculentior) facile eſt alios excogitare, ſicut & alios ipſe haud paucos cum fructu expertus ſum.

Objections from the mixture of coloured powders anſwered.

Contra Theoriam de coloribus objicitur, quòd pulveres diverſorum colorum permiſti non candidum ſed ſubobſcurum & fuſcum colorem exhibent. Mihi vero albus, niger & omnes intermedii fuſci, qui ab albo & nigro permiſtis componi poſſunt, non ſpecie coloris ſed quantitate lucis tantùm differre videntur. Et cùm in miſtione pigmentorum, ſingula corpuſcula non niſi proprium colorem reflectant, adeoque maxima pars lucis incidentis ſupprimatur & retineatur: lux reflexa ſubobſcura evadet, & quaſi cum tenebris permiſta; adeo ut non intenſum alborem, ſed qualem nigredinis permiſtio conficit, hoc eſt fuſcum, exhibere debeat.

Objection from the mixture of coloured fluids in the ſame veſſel anſwered.

Objicitur deinde, quòd à liquoribus quibuſcunque diverſi coloris in eodem vaſe commiſtis, æquè ac in diverſis vaſis contentis, opacitas oriri debet; quod tamen, ait, verum non eſſe. Sed non video conſequentiam. Nam plurimi liquores agunt in ſe invicem, & novam ſibi mutuò partium contexturam ſecreto inducunt; unde opaci, diaphani, vel variis coloribus, ex coloribus permiſtorum nullo modo oriundis, præditi evadere poſſunt. Et hâc de cauſâ experimenta hujuſmodi minùs apta ſemper exiſtimavi, à quibus concluſiones deduci poſſint. Subnoto tamen, quòd ad hoc experimentum requiruntur liquores faturis & intenſis coloribus præditi, qui perpaucos niſi proprii coloris radios tranſmittant; quales rarò occurrunt, ut videbitur illuminando liquores cum diverſis coloribus priſmatis in obſcurato cubiculo. Nam pauci reperientur, qui in propriis coloribus ſatis diaphani appareant, inque alienis opaci. Convenit præterea, ut adhibiti colores ſint inter ſe oppoſiti, quales exiſtimo fore rubrum & cæruleum, vel flavum & violaceum, vel etiam viridem & purpureum illum qui coccineo affinis eſt. Et ex hujuſmodi liquoribus nonnulli, quorum partes tingentes non congrediuntur, fortaſſe permiſti evadunt opaciores. Sed de eventu nihil ſum ſollicitus, tum quòd luculentius eſt experimentum in liquoribus ſeorſim exiſtentibus, tum quòd experimentum illud, ſicut & iridis, tincturæ nephriticæ, & aliorum corporum naturalium phænomena, non ad probandam ſed ad illuſtrandam tantùm doctrinam propoſui.

Quod R. P. Theoriam noſtram *hypotheſin* vocat, amicè habeo, ſiquidem ipſi nondum conſtet. Sed alio tamen conſilio propoſueram, & nihil aliud continere videtur quàm proprietates quaſdam lucis, quas jam inventas probare haud difficile exiſtimo, & quas ſi non veras eſſe cognoveram, pro futili & inani ſpeculatione mallem repudiare, quàm pro meâ *hypotheſi* agnoſcere.

Quid verò cenſeri mereatur, ex reſponſionibus ad animadverſiones Domini——fortaſſe ſtatim proditurus clariùs patebit. Interim vale, et perge amare tibi devinctiſſimum

III.

To Mr. OLDENBURG.

SIR, Cambridge, April , 1672.

I herewith * ſend you an anſwer to the Jeſuit *Pardie's* conſiderations, in the concluſion of which, you may poſſibly apprehend me a little too poſitive; but I ſpeak only for myſelf. I am highly ſenſible of your good will, in communicating to me ſuch obſervations as occur concerning my theories, or cata-dioptrical inſtruments, and I deſire you to continue that favour to me. Mr. *Hugens* has very well obſerved the confuſion of refractions near the edges of a lens, where its two ſuperficies are inclined much like the planes of a priſm; whoſe refractions are in like manner confuſed. But it is not from the inclination of thoſe ſuperficies, ſo much as from the heterogeneity of light, that that confuſion is cauſed. For by illuminating an object with homogeneal light, I have ſeen it far diſtincter through a priſm, than I could by light that was heterogeneal.

I ſuppoſe the deſign of Sir *Robert Moray's* experiments is, to have their events expreſſed with ſuch obſervations as may occur concerning them.

Touching the firſt, I have obſerved, that the ſolar image falling on a paper, placed at the focus of the lens, was, by the interpoſed priſm, drawn out in length proportional to the priſm's refraction, or diſtance from that focus. And the chief obſervable here, which I remember, was, that the ſtreight edges of the oblong image were diſtincter than they would have been without the lens.

Experiment on the rays coming from Venus.

Conſidering that the rays, coming from the planet *Venus*, are much leſs inclined one to another, than thoſe which come from the oppoſite parts of the ſun's diſk; I once tried an experiment or two with her light. And to make it ſufficiently ſtrong, I found it neceſſary to collect it firſt by a broad lens; and then, interpoſing a priſm between the lens and its focus, at ſuch diſtance

* This encloſed the preceding Letter.

that

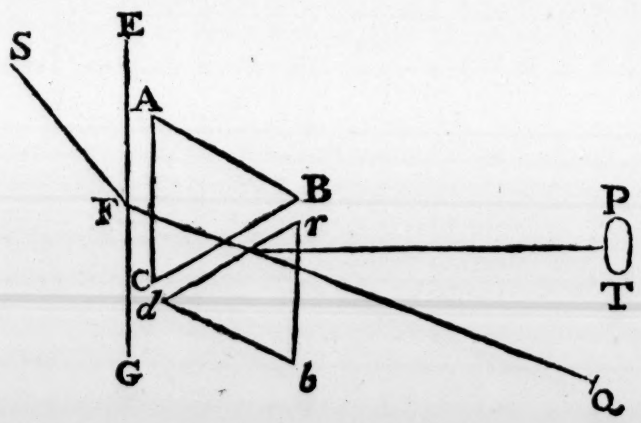
that all the light might paſs through the priſm, I found the focus, which before appeared like a lucid point, to be drawn out into a long ſplendid line by the priſm's refraction.

I have ſometimes deſigned to try how a fixed ſtar, ſeen through a long teleſcope, would appear, by interpoſing a priſm between the teleſcope and my eye. But by the appearance of *Venus*, viewed with my naked eye through a priſm, I preſage the event.

Concerning the ſecond experiment, I have occaſionally obſerved, that by covering both ends of the priſm with paper at ſeveral diſtances from the middle, the breadth of the ſolar image will be increaſed or diminiſhed as much, as is the aperture of the priſm, without any variation of the length: or, if the aperture be augmented on all ſides, the image on all ſides will be ſo much and no more augmented.

Of the third experiment, I have occaſion to ſpeak in my anſwer to another perſon; where you will find the effects of two priſms, in all croſs poſitions of one to another, deſcribed. But if one priſm alone be turned about, the coloured image will only be tranſlated from place to place, deſcribing a circle, or ſome other conick ſection on the wall, on which it is projected, without ſuffering any alteration in its ſhape, unleſs ſuch as may ariſe from the obliquity of the wall, or caſual change of the priſm's obliquity to the ſun's rays.

The effect of the fourth experiment I have already inſinuated, telling you, that light, paſſing through parts of the priſm of divers thickneſſes, did ſtill exhibit the ſame phænomena.



Note, the long axes of the two priſms in the above-mentioned experiment are parallel to one another. Other circumſtances reſpecting their poſition, you will better underſtand from the annexed ſcheme. In which, let EG deſign the window; F, the hole in it through which the light arrives at the priſms; ABC, the firſt priſm, which refracts the light towards PT, painting there the colours

colours in an oblong; and *dbr* the ſecond priſm, which refracts back again the rays to Q, where the long image, PT, is contracted into a round one. I ſuppoſe the plane *dr* parallel to BC, and *br* to AC, that no rays may be equally refracted contrary ways in both priſms. The priſms muſt alſo be placed very near to one another; for if their diſtance be ſo great, that colours begin to appear in the light, before its incidence on the ſecond priſm, thoſe colours will not be deſtroyed by the contrary refractions of that priſm.

Theſe things being obſerved, the round image, Q, will appear of the ſame bigneſs, which it doth when both the priſms are taken away, that the light may paſs directly towards Q from the hole without any refraction at all: and its diameter will equal the breadth of the long image PT, if thoſe images be equally diſtant from the priſms.

If an accurate conſideration of theſe refractions be deſigned, it is convenient that a lens be placed in the hole F, or immediately after the priſms, ſo that its focus be at the image Q, or PT. For thereby the perimeter of the image Q, and the ſtreight ſides of the image PT, will become much better defined than otherwiſe.

Thus far concerning Sir *Robert Moray's* propoſals. I have nothing more at preſent, unleſs to deſire you, that, in the letter wherein I ſent you of the table of apertures and charges, you would change an expreſſion concerning the ſix-foot tube, where I intimated, that it was none of the beſt in its kind. For leſt the friend, of whom it was borrowed, ſhould think I depreciate it, I had rather that the expreſſion ſhould be a little intimated after this manner; that I am not very well aſſured of its goodneſs, and therefore deſire, that the other experiment of reading at 100 foot diſtance, ſhall rather be confided in. You will do me a favour to peruſe the reſt of that letter alſo, before you commit it to the preſs. For I writ it in ſo much haſte, that I had no time to peruſe it: and by rendering my expreſſions more perſpicuous, or leſs ambiguous, you will ſtill oblige, yours, &c.

IV.

NEWTONUS OLDENBURGO S.

Reply to further objections of Ignatius Pardies.

IN obſervationibus R. P. Ignatii Pardies, quas ad te denuo conſcripſit, an majus ſit humanitatis argumentum, quòd meis reſponſionibus vim omnem attribuit, an ingenii, quòd objectiones proponit, quæ ſi non probè tollantur, doctrinam meam fruſtrari poſſunt, vix dixerim. Utrumque ſanè ad determinandam veritatem optimè conducit; efficitque, ut acceptis quàm lubentiſſimè reſpondeam.

Objection. That the length of the coloured image may be explained by Grimaldi's hypotheſis, or by Hook's.

Ait R. P. quòd abſque variâ diverſorum radiorum refrangibilitate, poſſibile ſit explicare longitudinem colorum; puta ex hypotheſi *P. Grimaldi*, per diffuſionem luminis, quod ſupponitur eſſe ſubſtantia quædam rapidiſſimè mota; vel ex hypotheſi *Hookii* noſtri, per diffuſionem vel expanſionem undulationum, quas ſtatuit in æthere à lucidis corporibus excitatas, quaquaverſum propagari. Addo, quòd ex hypotheſi *Carteſianâ* poteſt etiam effingi conſimilis diffuſio conatûs vel preſſionis globulorum; perinde ut in explicatione caudæ cometæ ſupponitur. Et eadem diffuſio vel expanſio juxta aliam quamvis hypotheſin, in quâ lumen ſtatuitur eſſe vis, actio, qualitas, vel ſubſtantia quælibet, à luminoſis corporibus undique emiſſa, effingi poteſt.

Ut his reſpondeam, animadvertendum eſt, quòd doctrina illa, quam de refractione & coloribus explicui, in quibuſdam lucis proprietatibus ſolummodo conſtitit, neglectis hypotheſibus per quas proprietates illæ explicari debent.

Optimus enim et tutiſſimus philoſophandi modus videtur, ut imprimis rerum proprietatis diligenter inquiramus, & per experimenta ſtabiliamus; ac dein tardiùs contendamus ad hypotheſes pro rerum explicatione. Nam hypotheſes ad explicandas rerum proprietates tantùm accommodari debent, & non ad determinandas uſurpari, niſi quatenus experimenta ſubminiſtrare poſſint. Et ſi quis ex ſolâ hypotheſium poſſibilitate de veritate rerum conjecturam faciat, non video quo pacto quicquam certi in ullâ ſcientiâ determinare poſſit; ſiquidem alias atque alias hypotheſes ſemper licet excogitari, quæ novas difficultates ſuppeditare videbuntur.

Quamobrem

Quamobrem ab hypotheſium contemplatione, tanquam improprio argumentandi loco, hic abſtinendum eſſe cenſui, & vim objectionis abſtrahendam, ut pleniorem & magis generalem reſponſionem accipiat.

Reply. Definition of Light, and Rays of Light.

Itaque per lumen intelligo quodlibet ens vel entis poteſtatem (ſive ſit ſubſtantia, ſive quævis ejus vis, actio, vel qualitas) quod à corpore lucido rectâ pergens aptum ſit ad excitandam viſionem; & per radios luminis intelligo minimas vel quaſlibet indefinitè parvas ejus partes, quæ ab invicem non dependent, quales ſunt illi omnes radii, quos lucentia corpora, vel ſimul vel ſucceſſivè, ſecundum rectas lineas emittunt. Nam illæ tum collaterales tum ſucceſſivæ partes luminis ſunt independentes; ſiquidem unæ abſque aliis intercipi poſſint, & in quaſlibet plagas ſeorſim reflecti vel refringi. Et hoc præcognito, objectionis vis omnis in eo ſita erit; quòd colores per aliquam luminis ultra foramen diffuſionem, quæ non oritur ab inæquali diverſorum radiorum ſeu luminis independentium partium refrangibilitate, in longum diduci poſſint.

Quod autem non aliunde oblongentur ſuperiùs monſtravi, in literis relatis in Philoſoph. Tranſact. Numb. 80. Et ut rationes faciliùs perſpiciantur, non gravabor jam fuſiùs explicare.

The fact proved, that Rays are unequally refracted in equal incidences.

Scilicet ex obſervatione, quòd Radii poſt Refractionem non incurvabantur, ſed rectâ ad parietem progreſſi fuerint, patuit, eandem fuiſſe eorum ad ſe mutuò inclinationem cùm modò exierunt priſmate, atque cùm impegerunt in parietem; & proinde longitudo colorum ex inclinatione radiorum emerſit, quam inter refringendum obtinuere; hoc eſt ex quantitate refractionis, quam ſinguli radii in priſmate patiebantur. Adeoque cum colorum longitudo latitudinem aliquot vicibus ex obſervatione ſuperavit; ſequitur majorem fuiſſe inæqualitatem refractionum, quàm potuit oriri ex inæqualitate incidentiarum. Quinimo ex figurâ Imaginis coloratæ, quòd nempe non fuit Ovalis, ſed latera duabus parallelis rectis lineis terminata, patuit eam ex indefinitè multis imaginibus Solis, per inæqualem refractionem in longam diſtractis & ſerie continuâ diſpoſitis, conſtitui; adeoque radios, à ſingulis partibus ſolaris diſci provenientes, per totam ferè longitudinem colorum diſpergi; & proinde ſimiliter incidentium inæquales eſſe refractiones. Id quod aliis etiam indiciis oſtendi poſſet.

Of the cauſe of that fact.

Conſtat itaque diverſas eſſe refractiones, ubi pares ſunt incidentiæ. Sed ampliùs inquirendum eſt, unde oriatur illa diverſitas? An ſit à causâ aliquâ incertâ & irregulari, vel certâ lege, ſecundum quam radius quilibet aptus eſt determinatam aliquam refractionem pati. Per incertas & irregulares cauſas intellige aſperitates in ſuperficie; vel venas diverſæ denſitatis in interiori parte Vitri, ex quo Priſma conflatur; item irregularem ſitum pororum, quos nonnulli ob luminis tranſmiſſionem per vitrum omnifariam trajici ſtatuunt; necnon tremores & inæquales commotiones partium Ætheris, Aeris vel Vitri; radiorum, in refringente ſuperficie ſe mutuò fortaſſe comprimentium, reſulturam ab invicem; ejuſdem cujuſque radii diviſionem ac diſſipationem in partes divergentes, quas vel numero finitas vel indefinitè multas, in ſuperficie aliquâ continuatim jacentes, imaginari liceat; vel quamvis aliam diffuſionem et dilatationem luminis, quam poſſumus excogitare, non ortam ex diversâ prædiſpoſitione cujuſque radii ad refractionem, in certo aliquo & conſtanti gradu, patiendam.

That it is no irregular uncertain cauſe.

Quòd autem diverſa refractio non orta ſit ex ullis ejuſmodi cauſis incertis & irregularibus, probavi per experimentum duorum conſimilium priſmatum in contrario ſitu juxtà poſitorum, ita ut poſterius contrariâ ſuâ refractione retroflecteret radios, & ſic regulares effectus prioris deſtrueret, ſed per iteratas refractiones, augeret irregulares. Utpote ſi prius priſma diffunderet, ac divergere faceret parallelos radios, ex. gr. per aſperam polituram, inæquabilem denſitatem, aut irregularem ſitum pororum in priſmatis, vel per tremulos motus partium Ætheris, Aeris aut Vitri; vel per dilatationem luminis propter partium ejus (*i. e.* radiorum) ſe mutuò comprimentium relaxationem verſus adjacentia ſpatia, quæ vel nullo vel minus conſtipato lumine irradiantur; vel denique per cujuſque radii dilatationem aut diffractionem in complures divergentes radios: tum ſane poſterius priſma magis diffunderet ac diſſiparet radios per dictas irregularitates Ætheris, Aeris aut Vitri; vel per iteratam dilatationem luminis, à refringentis ſuperficiei reſiſtentiâ denuò conſtipati ac diffuſi; vel etiam per cujuſque radii, à priori diffractione orti, iteratam diffractionem ac diviſionem in longè plures divergentes radios. Et ſic Lumen magis diſpergeretur per refractionem ſecundi Priſmatis, & in parietem

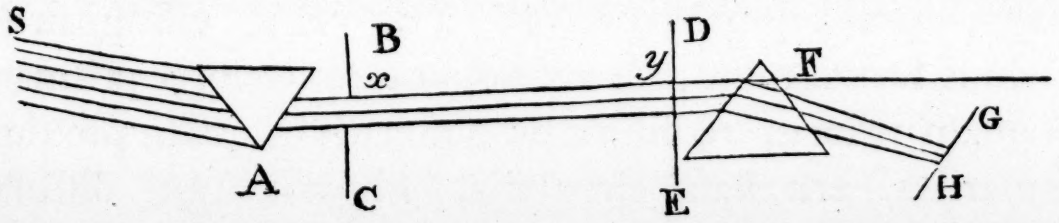
projectam

projectam imaginem duplo longiorem minimum exhiberet, quàm per ſolam refractionem prioris Priſmatis exhiberi potuiſſet. Quamobrem cùm, experientiâ teſte, refractio ſecundi Priſmatis adeo non diſpergat Lumen, ut contrahat, & in priſtinum ſtatum reducat, efficiatque ut in formâ Coni poſtea progrediatur, perinde ac ſi nullam omnino Refractionem paſſum fuiſſet; concedendum eſt, diffuſionem Luminis, à refractione anterioris Priſmatis effectam, non oriri ab aliquâ præfatarum cauſarum, aut aliâ quâvis irregularitate, ſed diverſæ Refrangibilitati radiorum ſolummodo tribuendam eſſe; utpote quâ radius unuſquiſque, ex inſitâ diſpoſitione tantam Refractionem in poſteriori, ac in priori paſſus, reducitur in paralleliſmum cum ſe ipſo; & ſic omnes radii ad ſe mutuò eaſdem inclinationes reſumunt, quas ante refractiones habuere.

Nor any other than the unequal refrangibility of the Rays.

Demum ut hæc omnia ſummè confirmarem, adjeci experimentum illud, quod jam nomine *crucis* paſſim inſignitur; de cujus conditionibus cùm R. P. dubitaverit, placuit jam deſignare Schemate. Sit BC anterior tabula, cui priſma A immediatè præfigi-

Experimentum Crucis deſcribed.



tur; ſitque DE altera tabula, quaſi 12 pedibus abinde diſtans, cui ſuffigitur alterum priſma F. Tabulæ autem ad *x* & *y*, ita preforentur, ut aliquantulum lucis, ab anteriori priſmate refractæ, trajici poſſit per utrumque foramen ad ſecundum priſma, inque eo denuo refringi. Jam priſma anterius circa axem reciproco motu convertatur, & colores in tabulam poſteriorem DE procidentes, per vices attollentur ac deprimentur; eoque pacto alius atque alius color ſucceſſivè pro arbitrio trajici poteſt per foramen ejus *y*, ad poſterius priſma, dum cæteri colores in tabulam impingunt: et videbis, radios diverſis coloribus præditos diverſam pati refractionem in illo poſteriori priſmate, ex eo quòd ad diverſa loca parietis vel cujuſvis obſtaculi GH, pedibus aliquot ulterius remoti, allabentur; puta violacei radii ad H, rubri ad G, & intermedii ad loca intermedia: & tamen, propter determinatam poſitionem foraminum, neceſſe eſt ut ſimilis ſit incidentia radiorum cujuſque coloris

coloris per utrumque trajecti. Atque ita ex menſurâ conſtat radios, diverſis coloribus affectos, habere diverſas leges refractionum.

Cauſe of Pardies miſtake.

Sed ſuſpicor unde adductus ſit R. P. in dubitationem; nempe videtur collocâſſe primum priſma, A, poſt tabulam BC, atque ita convertendo circa axem, veriſimile eſt inclinationem radiorum, qui interjacent foramina, propter intermediam refractionem fuiſſe mutatam. At, ex deſcriptione prius expoſitâ, debuit tabula illa collocari poſt priſma, ut radii inter foramina in directum jacerent; quemadmodum, ex verbis, *I took two boards, and placed one of them cloſe behind the priſm at the window*, conſtare poteſt. Et uſus experimenti idem innuit.

Ex abundanti placet obſervare, quòd in hoc experimento colorata lux, ob refractionem ſecundi priſmatis, longè minùs diffunditur ac divaricat, quàm cùm alba exiſtit, adeo ut imago ad G, vel H, ſit pene circularis; præſertim ſi priſmata ſtatuantur parallella, & in contrario ſitu angulorum, prout in ſchemate deſignantur. Quinetiam, ſi præterea diameter foraminis *y* adæquet latitudinem colorum, nulla erit ejuſdem coloratæ lucis in longum diffuſio; ſed imago, quæ à quopiam colore ad G, vel H, effingitur (poſitis circularibus foraminibus, & refractione poſterioris priſmatis non majori quàm prioris, radiiſque ad obſtaculum quàm proximè perpendicularibus) erit planè circularis. Id quod arguit diffuſionem, de quâ ſuprà egimus, non ex contagione vel continuitate materiæ undulantis, aut celerrimè motæ, vel ſimilibus cauſis ortam eſſe, ſed ex certâ refractionum cujuſque generis radiorum lege. Cur autem imago illa in uno caſu ſit circularis, & in aliis nonnihil oblongata, & quomodo diffuſio lucis in longitudinem in quolibet caſu pro arbitrio minui poſſit, à geometris determinandum, & cum experientiâ conferendum relinquo.

Poſtquam proprietates lucis his & ſimilibus experimentis ſatis exploratæ fuerint, ſpectando radios tanquam ejus ſive collaterales ſive ſucceſſivas partes, de quibus experti ſumus per independentiam quòd ſint ab invicem diſtinctæ; hypotheſes exinde dijudicandæ ſunt, & quæ non poſſunt conciliari rejiciendæ. Sed leviſſimi negotii eſt accommodare hypotheſes ad hanc doctrinam. Nam ſi quis hypotheſin Carteſianam defendere velit, dicendum eſt, globulos eſſe inæquales: vel preſſiones globulorum eſſe alias aliis fortiores,

fortiores, & inde diverſimodè refrangibiles, & aptas ad excitandam ſenſationem diverſorum colorum. Et ſic, juxta hypotheſin Cl. *Hookii* dicendum eſt, undulationes ætheris eſſe alias majores, ſive craſſiores aliis. Atque ita in cæteris. Hæc enim videtur eſſe ſummè neceſſaria lex & conditio hypotheſium, in quibus naturalia corpora ponuntur conſtare ex quamplurimis corpuſculis acervatim contextis, ut à diverſis lucentium corpuſculis, vel ejuſdem corpuſculi diverſis partibus (prout motu, figurâ, mole, aut aliis qualitatibus differunt) inæquales preſſiones, motiones aut mota corpuſcula per æthera quaquaverſum trajiciantur, ex quibus confuſè miſtis lux conſtitui ſupponetur. Et nihil durius eſſe poteſt in iſtis hypotheſibus quàm contraria ſuppoſitio.

Ex aperturâ ſive dilatatione lucis in poſteriori facie priſmatis, quam R. P. dixit eſſe veluti foramen, ſufficit, quòd error non emerget ſenſibilis, ſi modo aliquis emerget. Quòd ſi calculus juxta obſervationes præcisè ineatur, error erit nullus. Nam diametro foraminis à longitudine imaginis ſubductâ; reſtabit longitudo, quam imago haberet, ſi modo foramen ante priſma eſſet indiviſibile, idque non obſtante præfatâ lucis dilatatione in poſteriori facie priſmatis; ut facilè oſtenditur. Deinde, ex datâ illâ longitudine imaginis, ac diſtantiâ à foramine indiviſibili, ut & poſitione & formâ priſmatis, & ad id inclinatione incidentium radiorum, ac angulo, quem refracti radii ad medium imaginis tendentes, cum à centro ſolis incidentibus conſtituunt, cætera omnia determinantur. Et quæ determinant refractiones & poſitiones radiorum, ſufficiunt ad calculum iſtarum refractionum ritè ineundum. Sed res non tanti eſſe videtur, ut moram inferat.

Quòd R. P. doctrinam noſtram hypotheſin vocaverit, non aliunde factum eſſe credo, quàm quòd vocabulum uſurpavit, quod primum occurrit; ſiquidem mos obtinuit, ut quicquid exponitur in Philoſophiâ dicatur hypotheſis. Et ego ſane non alio conſilio vocabulum iſtud reprehendi; quàm ut ne invaleſceret appellatio, quæ rectè philoſophantibus præjudicio eſſe poſſet. Reverend. verò Patris candor in omnibus conſpicitur; indeque modus efferendi benevolentiam, qui mihi minimè convenit. Quòd tamen noſtra non diſplicent vehementer gaudeo Vale. Dabam Cantabrigiæ II. Junii 1672.

V.

V.

To Mr. OLDENBURG.

SIR, July , 1672.

Of the true method of examining this Theory.

IN the mean while give me leave, Sir, to inſinuate, that I cannot think it effectual for determining truth, to examine the ſeveral ways by which phænomena may be explained, unleſs where there can be a perfect enumeration of all thoſe ways. You know, the proper method for enquiring after the properties of things, is to deduce them from experiments. And I told you, that the theory which I propounded, was evinced to me, not by inferring, *it is thus*, *becauſe it is not otherwiſe*; that is, not by deducing it only from a confutation of contrary ſuppoſitions, but by deriving it from experiments concluding poſitively and directly. The way therefore to examine it, is, by conſidering whether the experiments, which I propound, do prove thoſe parts of the theory to which they are applied; or by proſecuting other experiments which the theory may ſuggeſt for its examination. And this I would have done in a due method; the laws of refraction being thoroughly enquired into and determined, before the nature of colours be taken into conſideration. It may not be amiſs to proceed according to the ſeries of theſe Queries; which I could wiſh were determined by the event of proper experiments, declared by thoſe that may have the curioſity to examine them.

1. Whether rays, that are alike incident on the ſame medium, have unequal refractions? And how great are the inequalities of their refractions at any incidence?

2. What is the law, according to which each ray is more or leſs refracted; whether it be, that the ſame ray is ever refracted according to the ſame ratio of the ſines of incidence and refraction; and divers rays, according to divers ratios; or that the refraction of each ray is greater or leſs without any certain rule? That is, whether each ray have a certain degree of refrangibility, according to which its refraction is performed; or is refracted without that regularity?

3. Whether

3. Whether rays, which are endued with particular degrees of refrangibility, when they are by any means ſeparated, have particular colours conſtantly belonging to them; *viz.* the leaſt refrangible, Scarlet; the moſt refrangible, deep Violet; the middle, Sea-Green; and others, other colours? And on the contrary?

4. Whether the colour of any ſort of rays apart may be changed by refraction?

5. Whether colours by coaleſcing do really change one another to produce a new colour, or produce it by mixing only?

6. Whether a due mixture of rays, indued with all variety of colours, produces light perfectly like that of the ſun, and which hath all the ſame properties, and exhibits the ſame phænomena?

7. Whether the component colours of each mixture be really changed; or be only ſeparated, when from that mixture various colours are produced again by refraction?

8. Whether there be any other colours produced by refraction, than ſuch as ought to reſult from the colours belonging to the diverſly refrangible rays, by their being ſeparated or mixed by that refraction?

To determine by experiments theſe and ſuch like queries, which involve the propounded theory, ſeems the moſt proper and direct way to a concluſion. And therefore I could wiſh all objections were ſuſpended taken from hypotheſes, or any other heads than theſe two: of ſhewing the inſufficiency of experiments to determine theſe queries, or prove any other parts of my theory, by aſſigning the flaws and defects in my concluſions drawn from them; or of producing other experiments, which directly contradict me, if any ſuch may ſeem to occur. For if the experiments, which I urge, be defective, it cannot be difficult to ſhew the defects; but if valid, then by proving the theory, they muſt render all objections invalid.

VI.

To Mr. OLDENBURG.

SIR, Cambridge, July 11, 1672.

I HAVE already told you, that at the peruſal of Mr. *Hook's Conſiderations* on my letter concerning Refractions and Colours, I found nothing that, as I conceived, might not without difficulty be anſwered. But, I muſt confeſs, at the firſt receipt of thoſe conſiderations I was a little troubled to find a perſon ſo much concerned for an *hypotheſis*, from whom in particular I moſt expected an unconcerned and indifferent examination of what I propounded. But yet I doubt not we have one common deſign; a ſincere endeavour after knowledge, without valuing uncertain ſpeculations for their ſubtleties, or deſpiſing certainties for their plainneſs. And on confidence of this it is, that I make this return to his Diſcourſe.

Of the improvement of Teleſcopes.

The firſt thing that offers itſelf is leſs agreeable to me, and I begin with it becauſe it is ſo. Mr. *Hook* thinks himſelf concerned to reprehend me for laying aſide the thoughts of improving optics by refractions. But he knows well it is not for one man to preſcribe rules to the ſtudies of another, eſpecially not without underſtanding the grounds on which he proceeds. Had he obliged me by a private letter on this occaſion, I would have acquainted him with my ſucceſſes in the trials that I have made in that kind; which I ſhall now ſay have been leſs than I ſometimes expected, and perhaps leſs than he at preſent hopes for. But ſince he is pleaſed to take it for granted, that I have let this ſubject paſs without due examination, I muſt refer him to my former letter, by which that conjecture will appear to be ungrounded; for what I ſaid there, was in reſpect of teleſcopes of the ordinary conſtruction, ſignifying, that their improvement is not to be expected from the well figuring of glaſſes, as opticians have imagined. But I deſpaired not of their improvement by other conſtructions, which made me cautious to inſert nothing that might intimate the contrary. For although ſucceſſive refractions, which are all made the ſame way, do neceſſarily more and more augment the errors of the firſt refraction; yet it ſeemed

not impoſſible for contrary refractions ſo to correct each other's inequalities, as to make their difference regular; and if that could be conveniently effected, there would be no further difficulty. Now to this end I examined what may be done not only by glaſſes alone, but more eſpecially by a complication of divers ſucceſſive mediums; as by two or more glaſſes or cryſtals with water, or ſome other fluid between them; all which together may perform the office of one glaſs, eſpecially of the object-glaſs on whoſe conſtruction the perfection of the inſtrument chiefly depends. But what the reſults in theory, or by trials, have been, I may poſſibly find a more proper occaſion to declare.

Truth of Reflection.

To the aſſertion, that rays are leſs true reflected to a point by a concave than refracted by a convex, I cannot aſſent; nor do I underſtand, that the focus of the latter is leſs a line than that of the former. The truth of the contrary you will rather perceive by this following Table, computed for ſuch a reflecting concave and refracting convex; on ſuppoſition that they have equal apertures, and collect parallel rays at an equal diſtance from their vertex; which diſtance being divided into 15000 parts, the diameter of the concave ſphere will be 60000 of thoſe parts, and of the convex 10000; ſuppoſing the ſines of incidence and refraction to be in round numbers, as 2 to 3. And this Table ſhews how much the exterior rays, at ſeveral apertures, fall ſhort of their principal focus.

The diameter of the Aperture.	The parts of the Axis intercepted between the vertex and the rays		The error by	
	reflected.	refracted.	Reflection.	Refraction.
2000	$14991\frac{2}{3}$	14865	$8\frac{1}{3}$	135
4000	14966	14449	33	551
6000	14924	13699	76	1301
8000	14865	12475	135	2525
10000	14787	9472	213	5528

By this you may perceive, that the errors of the refracting convex are ſo far from being leſs, that they are more than ſixteen times greater than the like errors of the reflecting concave, eſpecially in great apertures, and that without reſpect to the heterogeneous conſtitution of light: ſo that however the contrary ſuppoſition might make Mr. *Hook* reject reflexions, as uſeleſs for the promoting of opticks, yet I muſt for this, as well as other con-

conſiderations, prefer them in the theory before refractions. Whether the parabola be more difficult to deſcribe than the hyperbola or ellipſis, may be a query; but I ſee no abſolute neceſſity of endeavouring after any of their deſcriptions. For if metals can be ground truly ſpherical, they will bear as great apertures as, I believe, men will be well able to communicate an exact poliſh to. And for dioptrick teleſcopes I told you, that the difficulty conſiſted not in the figure of the glaſs, but in the difformity of refractions; which, if it did not, I could tell you a better, and more eaſy remedy, than the uſe of the conic ſections.

Reply to Mr. Hook's objections to the Theory.

Thus much concerning the practique part of opticks. I ſhall now take a view of Mr. *Hook's* conſiderations on my theories. And thoſe conſiſt in aſcribing an hypotheſis to me, which is not mine; in aſſerting an hypotheſis, which, as to the practical parts of it, is not againſt me; in granting the greateſt part of my diſcourſe, if explicated by that hypotheſis; and in denying ſome things, the truth of which would have appeared by an experimental examination.

The hypotheſis of the corporeity of Light no neceſſary foundation of the Newtonian Theory.

Of theſe particulars I ſhall diſcourſe in order; and firſt of the *hypotheſis*, which Mr. *Hook* hath aſſigned me in theſe words: *But grant his firſt ſuppoſition, that light is a body, and that as many colours or degrees thereof as there may be, ſo many bodies there may be, all which compounded together would make white, &c.* This, it ſeems, Mr. *Hook* takes for my hypotheſis. It is true, that from my theory I argue the corporeity of light, but I do it without any abſolute poſitiveneſs, as the word *perhaps* intimates, and make at moſt but a very plauſible conſequence of the doctrine, and not a fundamental ſuppoſition, nor ſo much as any part of it, which was wholly comprehended in the precedent propoſitions. And I wonder how Mr. *Hook* could imagine, that when I had aſſerted the theory with the greateſt rigor, I ſhould be ſo forgetful, as afterwards to aſſert the fundamental ſuppoſition itſelf with no more than a *perhaps*. Had I intended any ſuch hypotheſis, I ſhould ſomewhere have explained it. But I knew that the properties, which I declared of light, were in ſome meaſure capable of being explicated not only by that, but by many other mechanical hypotheſes; and therefore I choſe to decline

decline them all, and ſpeak of light in general terms, conſidering it abſtractedly as ſomething or other propagated every way in ſtreight lines from luminous bodies, without determining what that thing is; whether a confuſed mixture of difform qualities, or modes of bodies, or of bodies themſelves; or of any virtues, powers or beings whatſoever. And for the ſame reaſon I choſe to ſpeak of colours according to the information of our ſenſes, as if they were qualities of light without us. Whereas by that hypotheſis, I muſt have conſidered them rather as modes of ſenſation, excited in the mind by various motions, figures, or ſizes of the corpuſcles of the light, making various mechanical impreſſions on the organs of ſenſe, as I expreſſed it in that place where I ſpake of the corporeity of light.

Its near affinity with Hook's hypotheſis.

But ſuppoſing I had propounded this hypotheſis, I underſtand not why Mr. *Hook* ſhould ſo much endeavour to oppoſe it. For certainly it hath a much greater affinity with his own hypotheſis, than he ſeems to be aware of; the vibrations of æther being as uſeful and neceſſary in this, as in his own. For aſſuming the rays of light to be ſmall bodies, emitted every way from ſhining ſubſtances, thoſe, when they impinge on any refracting or reflecting ſuperficies, muſt as neceſſarily excite vibrations in the æther, as ſtones do in water, when thrown into it. And ſuppoſing theſe vibrations to be of ſeveral depths or thickneſſes, accordingly as they are excited by the ſaid corpuſcular rays of various ſizes and velocities; of what uſe they will be for explicating the manner of reflexion and refraction, the production of heat by the ſun's beams, the emiſſion of light from burning, putrifying, or other ſubſtances, whoſe parts are vehemently agitated, the phænomena of thin tranſparent plates and bubbles, and of all natural bodies, the manner of viſion, and the difference of colours, as alſo their harmony and diſcord; I ſhall leave to their conſideration, who may think it worth their endeavour to apply this hypotheſis to the ſolution of phænomena.

Of Hook's hypotheſis. Conformity of that and of every mechanical hypotheſis with the Newtonian doctrine.

In the ſecond place I told you, that Mr. *Hook's* hypotheſis, as to the fundamental part of it, is not againſt me. The fundamental ſuppoſition is, that the parts of bodies, when briſkly agitated, do excite vibrations in the æther, which are propagated every way from thoſe bodies in ſtreight lines, and cauſe a ſenſation.

tion of light, by beating and daſhing againſt the bottom of the eye; ſomething after the manner that vibrations in the air cauſe a ſenſation of ſound, by beating againſt the organs of hearing. Now the moſt free and natural application of this hypotheſis to the ſolution of phænomena I take to be this: That the agitated parts of bodies, according to their ſeveral ſizes, figures, and motions, excite vibrations in the æther of various depths or bigneſſes; which, being promiſcuouſly propagated through that medium to our eyes, effect in us a ſenſation of light of a white colour: but if by any means thoſe of unequal bigneſſes be ſeparated from one another, the largeſt, a ſenſation of a Red colour; the leaſt, or ſhorteſt, of a deep Violet; and the intermediate ones, of intermediate colours. Much after the manner that bodies, according to their ſeveral ſizes, ſhapes, and motions, excite vibrations in the air of various bigneſſes, which, according to thoſe bigneſſes, make ſeveral tones in ſound. That the largeſt vibrations are beſt able to overcome the reſiſtance of a refracting ſuperficies, and ſo break through it with leaſt refraction: whence the vibrations of ſeveral bigneſſes, that is, the rays of ſeveral colours, which are blended together in light, muſt be parted from one another by refraction; and ſo cauſe the phænomena of priſms, and other refracting ſubſtances. And that it depends on the thickneſs of a thin tranſparent plate or bubble, whether a vibration ſhall be reflected at its further ſuperficies, or tranſmitted; ſo that according to the number of vibrations interceding the two ſuperficies, they may be reflected or tranſmitted for many ſucceſſive thickneſſes. And ſince the vibrations, which make Blue and Violet, are ſuppoſed ſhorter than thoſe that make Red and Yellow; they muſt be reflected at a leſs thickneſs of the plate, which is ſufficient to explicate all the ordinary phænomena of thoſe plates or bubbles; and alſo of all natural bodies, whoſe parts are like ſo many fragments of ſuch plates. Theſe ſeem to be the moſt plain, genuine, and neceſſary conditions of this hypotheſis; and they agree ſo juſtly with my theories, that, if Mr. *Hook* think fit to apply them, he need not on that account fear a divorce from it. But yet how he will defend it from other difficulties, I know hot; for to me the fundamental ſuppoſition it-

ſelf

ſelf ſeems impoſſible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in ſtreight lines, without a continual and very extravagant ſpreading and bending every way into the quieſcent medium, where they are terminated by it. I am miſtaken if there be not both experiment and demonſtration to the contrary: and as to the other two or three hypotheſes which he mentions, I had rather believe them ſubject to the like difficulties, than ſuſpect that Mr. *Hook* ſhould ſelect the worſt for his own.

What I have ſaid of this, may be eaſily applied to all other mechanical hypotheſes, in which light is ſuppoſed to be cauſed by any preſſion or motion whatſoever, excited in the æther by the agitated parts of luminous bodies. For it ſeems impoſſible, that any of thoſe motions or preſſions can be propagated in ſtreight lines, without the like ſpreading every way into the ſhadowed medium, on which they border. But yet, if any man can think it poſſible, he muſt at leaſt allow, that thoſe motions, or endeavours to motion, cauſed in the æther by the ſeveral parts of any lucid bodies, which differ in ſize, figure, and agitation, muſt neceſſarily be unequal; which is enough to denominate light an aggregate of difform rays, according to any of thoſe hypotheſes. And if thoſe original inequalities may ſuffice to difference the rays in colour and refrangibility, I ſee no reaſon why they, that adhere to any of thoſe hypotheſes, ſhould ſeek for other cauſes of theſe effects, unleſs (to uſe Mr. *Hook's* argument) they will multiply entities without neceſſity.

Of Hook's conceſſions, and the limitations of them to his hypotheſis.

The third thing to be conſidered, is, the condition of Mr. *Hook's* conceſſions, which is, that I would explicate my theories by his hypotheſis: and if I could but comply with him in that point, there would be little or no difference between us: for he grants, that without any reſpect to a different incidence of rays, there are different refractions; but he would have it explicated, not by the different refrangibility of ſeveral rays, but by the ſplitting and rarifying of æthereal pulſes. He grants my third, fourth, and ſixth Propoſitions; the ſenſe of which is, that uncompounded colours are unchangeable, and that compounded colours are changeable, only by reſolving them into the colours of which they are compounded; and that all the changes,

changes, which can be wrought in colours, are effected only by variouſly mixing or parting them: but he grants them, on condition that I will explicate colours by the two ſides of a ſplit pulſe, and ſo make but two ſpecies of them, accounting all other colours in the world to be but various degrees and dilutings of thoſe two. And he further grants, that whiteneſs is produced by the convention of all colours; but then I muſt allow it to be not only by mixture of thoſe colours, but by a further uniting of the parts of the ray ſuppoſed to be formerly ſplit. If I would proceed to examine theſe his explications, I think it would be no difficult matter to ſhew, that they are not only inſufficient, but in ſome reſpects unintelligible. For though it be eaſy to conceive how motion may be dilated and ſpread, or how parallel motions may become diverging; yet I underſtand not by what artifice any linear motion can, by a refracting ſuperficies, be infinitely dilated and rarified ſo as to become ſuperficial: or if that be ſuppoſed, yet I underſtand as little why it ſhould be ſplit at ſo ſmall an angle only, and not rather ſpread, and diſperſed through the whole angle of refraction. And, further, though I can eaſily imagine how unlike motions may croſs one another, yet I cannot well conceive how they ſhould coaleſce into one uniform motion, and then part again, and recover their former unlikeneſs; notwithſtanding that I conjecture the ways by which Mr. *Hook* may endeavour to explain it. So that the direct, uniform, and undiſturbed pulſes, ſhould be ſplit and diſturbed by refraction; and yet the oblique and diſturbed pulſes perſiſt without ſplitting, or further diſturbance, by following refractions, is as unintelligible. And there is as great difficulty in the number of colours, as you will ſee hereafter.

The Newtonian doctrine independent of every hypotheſis.

But whatever be the advantages or diſadvantages of this hypotheſis, I hope Mr. *Hook* will excuſe me from taking it up, ſince I do not think it needful to explicate my doctrine by any hypotheſis at all; for if light be conſidered abſtractedly without reſpect to any hypotheſis, I can as eaſily conceive, that the ſeveral parts of a ſhining body may emit rays of different colours, and other qualities, of all which light is conſtituted, as that the ſeveral parts of a falſe or uneven ſtring, or of agitated water in a brook

brook or a cataract, or the ſeveral pipes of an organ inſpired all at once, or all the variety of ſounding bodies in the world together, ſhould produce ſounds of ſeveral tones, and propagate them through the air confuſedly intermixed. And if there were any natural bodies, which could reflect ſounds of one tone, and ſtifle or tranſmit thoſe of another; then, as the echo of a confuſed aggregate of all tones would be that particular tone, which the echoing body is diſpoſed to reflect: ſo ſince even by Mr. *Hook's* conceſſions there are bodies apt to reflect rays of one colour, and ſtifle or tranſmit thoſe of another; I can as eaſily conceive, that thoſe bodies, when illuminated by mixture of all colours, muſt appear of that colour only which they reflect.

But when Mr. *Hook* would inſinuate a difficulty in theſe things, by alluding to ſounds in the ſtring of a muſical inſtrument before percuſſion, or in the air of an organ-bellows before its arrival to the pipes: I muſt confeſs I underſtand it as little, as if he had ſpoke of light in a piece of a wood, before it be ſet on fire; or in the oil of a lamp, before it aſcend up the match to feed the flame.

Difficulties of Hook's diſcourſe.

You ſee therefore how much it is beſides the buſineſs in hand, to diſpute about hypotheſis. For which reaſon I ſhall now, in the laſt place, proceed to abſtract the difficulties involved in Mr. *Hook's* diſcourſe, and, without having regard to any hypotheſis, conſider them in general terms. And they may be reduced to theſe queries: Whether the unequal refractions, made without reſpect to any inequality of incidence, be cauſed by the different refrangibility of ſeveral rays, or by the ſplitting, breaking, or diſſipating the ſame ray into diverging parts? Whether there be more than two ſorts of colours; and whether whiteneſs be a mixture of all colours?

The firſt of theſe queries you may find already determined by an experiment in my former letter: the deſign of which was to ſhew, that the length of the coloured image proceeded not from any unevenneſs in the glaſs, or any other contingent irregularity in the refractions. Amongſt other irregularities, I know not what is more obvious to ſuſpect, than a fortuitous dilating and ſpreading of light, after ſome ſuch manner as *Des Cartes* hath

deſcribed in his æthereal refractions for explicating the tail of a comet; or as Mr. *Hook* now ſuppoſeth to be effected by the ſplitting and rarifying of his æthereal pulſes. And to prevent the ſuſpicion of any ſuch irregularities, I told you, that I refracted the light contrary ways with two priſms ſucceſſively; to deſtroy thereby the regular effects of the firſt priſm by the ſecond; and to diſcover the irregular effects, by augmenting them with iterated refractions. Now, amongſt other irregularities, if the firſt priſm had ſpread and diſſipated every ray into an indefinite number of diverging parts; the ſecond ſhould, in like manner, have ſpread and diſſipated every one of thoſe parts into a further indefinite number: whereby the image would ſtill be more dilated, contrary to the event. And this ought to have happened, becauſe thoſe linear diverging parts depend not on one another for the manner of their refraction, but are every one of them as truly and compleatly rays, as the whole was before its incidence, as may appear by intercepting them ſeverally.

The reaſonableneſs of this proceeding will, perhaps, better appear, by acquainting you with this further circumſtance. I ſometimes placed the ſecond priſm in a poſition tranſverſe to the firſt, on deſign to try if it would make the long image become four-ſquare, by refractions croſſing thoſe which had drawn the round image into a long one. For if amongſt other irregularities the refraction of the firſt priſm did, by ſplitting, dilate a linear ray into a ſuperficial, the croſs refractions of that ſecond priſm ought, by further ſplitting, to dilate, and draw that ſuperficial ray into a pyramidal ſolid. But upon trial I found it otherwiſe; the image being as regularly oblong as before, and inclined to both the priſms at an angle of 45 degrees.

I tried alſo all other poſitions of the ſecond priſm, by turning the ends about its middle part, and in no caſe could obſerve any ſuch irregularity. The image was ever alike inclined to both priſms, its breadth anſwering to the ſun's diameter, and its length being greater or leſs, accordingly as the refractions more or leſs agreed or contradicted one another. And by theſe obſervations, ſince the breadth of the image was not augmented by the croſs refraction of the ſecond priſm, that refraction muſt have been performed

performed without any ſplitting and dilating of the ray: and therefore at leaſt the light incident on that priſm muſt be granted an aggregate of rays unequally refrangible in my ſenſe. And ſince the image was equally inclined to both priſms, and conſequently the refractions alike in both, it argues, that they were performed according to ſome conſtant law without any irregularity.

To determine the ſecond query, Mr. *Hook* refers to an experiment made with two wedge-like boxes; the deſign of which was to produce all colours out of a mixture of two. But there is a double defect in this inſtance; for it appears not that by this experiment all colours can be produced out of two; and if they could, yet the inference would not follow. That all colours cannot, by the experiment, be produced out of two, will appear by conſidering, that the tincture of *aloes*, which afforded one of thoſe colours, was not all over of one uniform colour, but appeared Yellow near the edge of the box, and Red at other places where it was thicker, affording all variety of colours from a pale Yellow to a deep Red or Scarlet, according to the various thickneſs of the liquor. And ſo the ſolution of copper, which afforded the other colour, was of various Blues and Indicos: ſo that inſtead of two colours, here is a great variety made uſe of, for the production of all colours. Thus, for inſtance, to produce all ſorts of Greens, the ſeveral degrees of Yellow and pale Blue muſt be mixed; but to compound Purples, the Scarlet and deep Blue or Indico are to be the ingredients. Original colours more than two.

Now if Mr. *Hook* contend, that all the Reds and Yellows of the one liquor, or Blues and Indicos of the other, are only various degrees and dilutings of the ſame colour, and not divers colours; that is a begging of the queſtion: and I ſhould as ſoon grant, that the two-thirds or ſixths in muſic are but ſeveral degrees of the ſame ſound, and not divers ſounds. Certainly it is much better to believe our ſenſes informing us, that Red and Yellow are divers colours; and to make it a philoſophical query, why the ſame liquor doth, according to its various thickneſs, appear of thoſe divers colours; than to ſuppoſe them to be the ſame colour, becauſe exhibited by the ſame liquor. For if that were

the ſufficient reaſon, then Blue and Yellow muſt alſo be the ſame colour, ſince they are both exhibited by the ſame tincture of *nephritick* wood.

But that they are divers colours, you will more fully underſtand by the reaſon of them, which is this: The tincture of aloes is qualified to tranſmit moſt eaſily the rays indued with Red; moſt difficulty, the rays indued with Violet; and with intermediate degrees of facility, the rays indued with intermediate colours. So that where the liquor is very thin, it may ſuffice to intercept moſt of the Violet, and yet tranſmit moſt of the other colours; all which together muſt compound a middle colour, that is, a faint Yellow. And where it is ſo much thicker, as alſo to intercept moſt of the Blue and Green; the remaining Green, Yellow, and Red, muſt compound an Orange. And where the thickneſs is ſo great, that ſcarce any rays can paſs through it, beſides thoſe indued with Red; it muſt appear of that colour, and that ſo much the deeper and obſcurer, by how much the liquor is thicker. And the ſame may be underſtood of the various degrees of Blue exhibited by the ſolution of copper, by reaſon of its diſpoſition to intercept Red moſt eaſily, and tranſmit a deep Blue or Indico colour moſt freely.

But ſuppoſing that all colours might, according to this experiment, be produced out of two by mixture; yet it follows not, that thoſe two are the only original colours, and that for a double reaſon. Firſt, becauſe thoſe two are not themſelves original colours, but compounded of others; there being no liquor, nor any other body in nature, whoſe colour in day-light is wholly uncompounded. And then, becauſe though thoſe two were original, and all others might be compounded of them; yet it follows not that they cannot be otherwiſe produced. For I ſaid, that they had a double origin, the ſame colours to ſenſe being in ſome caſe compounded, and in others uncompounded; and ſufficiently declared in my third and fourth Propoſitions, and in the concluſion, by what properties the one might be known and diſtinguiſhed from the other. But becauſe I ſuſpect, by ſome circumſtances, that the diſtinction might not be rightly apprehended; I ſhall once more declare it, and further explain it by examples.

That

That colour is *primary, or original, which cannot by any art be changed, and whoſe rays are all alike refrangible*; *and that compounded, which is changeable into other colours, and whoſe rays are not alike refrangible.* For inſtance, to know whether the colour of any Green object be compounded or not, view it through a priſm; and if it appear confuſed, and the edges tinged with Blue, Yellow, or any variety of other colours, then is that Green compounded of ſuch colours, as at its edges emerge out of it. But if it appear diſtinct and well defined, and intirely Green to the very edges, without any other colours emerging; it is of an original and uncompounded Green. In like manner, if a refracted beam of light, being caſt on a white wall, exhibit a Green colour; to know whether that be compounded, refract the beam with an interpoſed priſm: and if you find any difformity in the refractions, and the Green be transformed into Blue, Yellow, or any variety of other colours; you may conclude, that it was compounded of thoſe colours which emerge. But if the refraction be uniform, and the Green perſiſt without any change of colour; then it is original and uncompounded. And the reaſon why I call it ſo, is, becauſe a Green, indued with ſuch properties, cannot be produced by any mixing of other colours.

Rule to diſtinguiſh compounded and uncompounded colours.

Now if two green objects may, to the naked eye, appear of the ſame colour; and yet one of them, through a priſm, ſeem confuſed, and variegated with other colours at the edges; and the other diſtinct and intirely Green: or, if there may be two beams of light which, falling on a white wall, do, to the naked eye, exhibit the ſame Green colour; and yet one of them, when tranſmitted through a priſm, be uniformly and regularly refracted, and retain its colour unchanged; and the other be irregularly refracted, and made to divaricate into a multitude of other colours: I ſuppoſe theſe two Greens will, in both caſes, be granted of a different origin and conſtitution. And if by mixing colours a Green cannot be compounded with the properties of the unchangeable Green, I think I may call that an uncompounded colour; eſpecially ſince its rays are all alike refrangible, and uniform in all reſpects.

The

The ſame rule is to be obſerved in examining whether Red, Orange, Yellow, Blue, or any other colour, be compounded or not. And, by the way, ſince all White objects through the priſm appeared confuſed, and terminated with colours; Whiteneſs muſt, according to this diſtinction, be ever compounded; and that the moſt of all colours, becauſe it is the moſt confuſed and changed by refractions.

A way of improving Microſcopes.

From hence I may take occaſion to communicate a way for the improvement of microſcopes by refraction; which I do the more willingly, becauſe Mr. *Hook* hath made ſuch excellent uſe of that inſtrument; and I ſhall be glad if it will contribute any thing to your promotion of thoſe his ingenious endeavours, or add to his inventions of that kind. The way is, by illuminating the object in a darkened room with light of any convenient colour, not too much compounded. For by that means the microſcope will, with diſtinctneſs, bear a deeper charge and larger aperture; eſpecially if its conſtruction be ſuch as I may hereafter deſcribe: for the advantage in ordinary microſcopes will not be ſo ſenſible.

Whiteneſs a mixture of all colours.

There remains now the third query to be conſidered; and that is, whether Whiteneſs be an uniform colour, or a diſſimilar mixture of all colours. The experiment which I brought to decide it, Mr. *Hook* thinks may be otherwiſe explained, aud ſo concludes nothing. But he might eaſily have ſatisfied himſelf, by trying what would be the reſult of a mixture of all colours. And that very experiment might have ſatisfied him, if he had examined it by the various circumſtances. One circumſtance I there declared, of which I ſee no notice taken; and it is, that if any colour at the lens be intercepted, the whiteneſs will be changed into the other colours. If all the colours but Red be intercepted, the Red alone, in the concourſe or croſſing of the rays, will not conſtitute whiteneſs, but continues as much Red as before; and ſo of the other colours. So that the buſineſs is not only to ſhew how rays, which before the concourſe exhibit colours, do in the concourſe exhibit White: but to ſhew how, in the ſame place where the ſeveral ſorts of rays apart exhibit ſeveral colours, a confuſion of all together make White. For inſtance, if Red alone be firſt tranſmitted to the paper at the place

of

of concourſe, and then the other colours be let fall on that Red: the queſtion will be, whether they convert it into White, by mixing with it only; as Blue, falling upon Yellow light, is ſuppoſed to compound Green: or whether there be ſome further change wrought in the colours by their mutual acting on one another, until, like contrary peripatetic qualities, they become aſſimilated. And he that ſhall explicate this caſe mechanically, muſt conquer a double impoſſibility. He muſt firſt ſhew, that many unlike motions in a fluid can, by claſhing, ſo act on one another, and change each other, as to become one uniform motion; and then, that an uniform motion can of itſelf, without any new unequal impreſſions, depart into a great variety of motions regularly unequal. And after this he muſt further tell me, why all objects appear not of the ſame colour; that is, why their colours in the air, where the rays that convey them every way are confuſedly mixed, do not affimilate one another, and become uniform, before they arrive at the ſpectator's eye.

But if there be yet any doubting, it is better to put the event on further circumſtances of the experiment, than to acquieſce in the poſſibility of any hypothetical explication. As, for inſtance, by trying what will be the apparition of theſe colours in a very quick conſecution of one another. And this may be eaſily performed by the rapid gyration of a wheel with many ſpokes, or coggs in its perimeter, whoſe interſtices and thickneſſes may be equal; and of ſuch a largeneſs, that if the wheel be interpoſed between the priſm and the White concourſe of the colours, one half of the colours may be intercepted by a ſpoke or cogg, and the other half paſs through an interſtice. The wheel being in this poſture, you may firſt turn it ſlowly about, to ſee all the colours fall ſucceſſively on the ſame place of the paper, held at the aforeſaid concourſe: and if you then accelerate its gyration, until the conſecution of thoſe colours be ſo quick, that you cannot diſtinguiſh them ſeverally; the reſulting colour will be a whiteneſs perfectly like that which an unrefracted beam of light exhibiteth, when in like manner ſucceſſively interrupted by the ſpokes or coggs of that circulating wheel. And that this whiteneſs is produced only by a ſucceſſive intermixture of the colours,

without

without their being aſſimilated or reduced to any uniformity, is certainly beyond all poſſibility of doubting; unleſs things that exiſt not at the ſame time, may notwithſtanding act on one another.

There are yet other circumſtances, by which the truth might have been decided; as by viewing the White concourſe of the colours through another priſm, placed cloſe to the eye, by whoſe refraction that whiteneſs may appear again transformed into colours. And then, to examine their origin, if an aſſiſtant intercept any of the colours at the lens before their arrival at the whiteneſs, the ſame colours will vaniſh from amongſt thoſe, into which that whiteneſs is converted by the ſecond priſm. Now if the rays which diſappear be the ſame with thoſe that are intercepted; then it muſt be acknowledged, that the ſecond priſm makes no new colour in any rays which were not in them before their concourſe at the paper: which is a plain indication, that the rays of ſeveral colours remain diſtinct from one another in the whiteneſs, and that from their previous diſpoſitions are derived the colours of the ſecond priſm. And, by the way, what is ſaid of their colours, may be applied to their refrangibility. The aforeſaid wheel may be alſo here made uſe of. And if its gyration be neither too quick nor too ſlow, the ſucceſſion of the colours may be diſcerned through the priſm, whilſt, to the naked eye of a by-ſtander, they exhibit whiteneſs.

There is ſomething ſtill remaining to be ſaid of this experiment. But this I conceive is enough to enforce it, and ſo to decide the controverſy. However, I ſhall now proceed to ſhew ſome other ways of producing whiteneſs by mixtures; ſince I perſuade myſelf, that this aſſertion above the reſt appears paradoxical, and is with moſt difficulty admitted. And becauſe Mr. *Hook* deſires an inſtance of it in bodies of divers colours, I ſhall begin with that. But in order thereto, it muſt be conſidered, that ſuch coloured bodies reflect but ſome part of the light incident on them, as is evident by the 13th Propoſition; and therefore the light, reflected from an aggregate of them, will be much weakened by the loſs of many rays. Whence a perfect and intenſe whiteneſs is not to be expected, but rather a colour

between thoſe of light and ſhadow, or ſuch a Grey or dirty colour as may be made by mixing White and Black together. And yet ſuch a colour will reſult, may be collected from the colour of duſt found in every corner of a houſe, which hath been obſerved to conſiſt of many coloured particles. There may be alſo produced the like dirty colour, by mixing ſeveral painter's colours together. And the ſame may be effected by painting a top (ſuch as boys play with) of divers colours; for when it is made to circulate by whipping, it will appear of ſuch a dirty colour.

Greys not ſpecifically different from White.

Now the compounding of theſe colours is proper to my purpoſe, becauſe they differ not from whiteneſs in the ſpecies of colours, but only in degrees of luminouſneſs, which, did not Mr. *Hook* concede it, I might thus evince. A beam of the ſun's light being tranſmitted into a darkened room, if you illuminate a ſheet of white paper by that light reflected from a body of any colour; the paper will always appear of the colour of that body, by whoſe reflected light it is illuminated. If it be a Red body, the paper will be Red; if a Green, it will be Green; and ſo of the other colours. And the reaſon is, that the fibres or threads, of which the paper conſiſts, are all tranſparent and ſpecular; and ſuch ſubſtances are known to reflect colours without changing them. To know, therefore, to what ſeries of colours a Grey belongeth, place any Grey body, ſuppoſe a mixture of painted colours, in the ſaid light; and the paper, being illuminated by its reflection, ſhall appear White; and the ſame thing will happen, if it be illuminated by reflection from a Black ſubſtance.

Greys and Black by what diſtinguiſhed from White.

Theſe, therefore, are all of one ſpecies; but yet they ſeem to be diſtinguiſhed not only by degrees of luminouſneſs, but alſo by ſome other inequalities, whereby they become more harſh or pleaſant: and the diſtinction ſeems to be, that Grays, and perhaps Black, are made by an uneven defect of light, conſiſting as it were of many little veins or ſtreams, which differ either in luminouſneſs, or in the unequal diſtribution of diverſly coloured rays, ſuch as ought to be cauſed by reflection from a mixture of White and Black, or of diverſly coloured corpuſcles. But when ſuch imperfectly mixed light is, by a ſecond reflection from the paper, more evenly and uniformly blended, it becomes more

pleaſant, and exhibits a faint or ſhaddowed whiteneſs. And that ſuch little irregularities as theſe may cauſe theſe differences is not improbable, if we conſider how much variety may be cauſed in ſounds of the ſame tone by irregular and uneven jarrings. And, beſides, theſe differences are ſo little, that I have ſometimes doubted whether they be any at all; when I have conſidered, that a Black and a White body being placed together, the one in a ſtrong light, and the other in a very faint light ſo proportioned, that they might appear equally luminous; it hath been difficult to diſtinguiſh them, when viewed at a diſtance, unleſs when the Black ſeemed more Blueiſh, and the White body in a light ſtill fainter, hath, in compariſon of the Black body itſelf, appeared Black.

Another way of compounding Whiteneſs.

This leads me to another way of compounding Whiteneſs, which is this: that if four or five bodies of the more eminent colours, or a paper painted all over in ſeveral parts of it with thoſe ſeveral colours in a due proportion, be placed in the ſaid beam of light; the light reflected from thoſe colours to another white paper, held at a convenient diſtance, ſhall make that paper appear White. If it be held too near the colours, its parts will ſeem of thoſe colours which are neareſt them; but by removing it further, that all its parts may be equally illuminated by all the colours, they will be more and more diluted, until they become perfectly White. And you may further obſerve, that if any of the colours be intercepted, the paper will no longer appear White, but of the other colours which are not intercepted. Now that this Whiteneſs is a mixture of the ſeverally coloured rays, falling confuſedly on the paper, I ſee no reaſon to doubt of; becauſe if the light became uniform and ſimilar before it fell on the paper, it muſt much more be uniform, when at a greater diſtance it falls on the ſpectator's eye; and ſo the rays which come from ſeveral colours would, in no qualities, differ from one another, but all of them exhibit the ſame colour to the ſpectator, contrary to what he ſees. Not much unlike this inſtance it is, that if a poliſhed piece of metal be ſo placed, that the colours appear in it as a looking-glaſs, and then the metal be made rough, that by a confuſed reflection thoſe apparent colours may be blended together,

gether, they ſhall diſappear, and by their mixture cauſe the metal to look White.

Of the Whiteneſs of Froth.

But further to enforce this experiment, if inſtead of the paper any white froth, conſiſting of ſmall bubbles, be illuminated by reflection from the aforeſaid colours, it ſhall to the naked eye ſeem White; and yet through a good microſcope the ſeveral colours will appear diſtinct on the bubbles, as if ſeen by reflection from ſo many ſpherical ſurfaces; with my naked eye being very near, I have alſo diſcerned the ſeveral colours on each bubble; and yet at a greater diſtance, where I could not diſtinguiſh them apart, the froth hath appeared entirely White. And at the ſame diſtance, when I looked intently, I have ſeen the colours diſtinctly on each bubble; and yet by ſtraining my eyes, as if I would look at ſomething far off beyond them, thereby to render the viſion confuſed, the froth hath appeared without any other colour than Whiteneſs. And what is here ſaid of froth, may be eaſily underſtood of the paper or metal in the foregoing experiments; for their parts are ſpecular bodies, like thoſe bubbles, and perhaps with an excellent microſcope the colours may be alſo ſeen intermixedly reflected from them.

In proportioning the ſeverally coloured bodies to produce theſe effects, there may be ſome niceneſs; and it will be more convenient to make uſe of the colours of the priſm caſt on a wall, by whoſe reflection the paper, metal, froth, and other white ſubſtances may be illuminated: and I uſually made my trials this way, becauſe I could better exclude any ſcattering light from mixing with the colours to dilute them.

To this way of compounding Whiteneſs may be referred that other, by mixing light after it hath been trajected through tranſparently coloured ſubſtances. For inſtance, if no light be admitted into a room but only through coloured glaſs, whoſe ſeveral parts are of ſeveral colours in a pretty equal proportion, all white things in the room ſhall appear White, if they be not held too near the glaſs: and yet this light with which they are illuminated cannot poſſibly be uniform, becauſe if the rays, which at their entrance are of divers colours, do, in their progreſs through the room, ſuffer any alteration to be reduced to an

uniformity, the glaſs would not, in the remoteſt parts of the room, appear of the very ſame colour which it doth when the ſpectator's eye is very near it. Nor would the rays, when tranſmitted into another dark room through a little hole in an oppoſite door or partition-wall, project on a paper the ſpecies or repreſentation of the glaſs in its proper colours.

And, by the way, this ſeems a very fit and cogent inſtance of ſome other parts of my theory, and particularly of the 13th Propoſition. For in this room all natural bodies whatever appear of their proper colours; and all the phænomena of colours in nature, made either by refraction or without it, are here the ſame as in the open air. Now the light in this room being ſuch a diſſimilar mixture as I have deſcribed in my Theory, the cauſes of all theſe phænomena muſt be the ſame that I have there aſſigned. And I can ſee no reaſon to ſuſpect, that the ſame phænomena ſhould have other cauſes in the open air. The ſucceſs of this experiment may be eaſily conjectured by the appearance of things in a church or chapel, whoſe windows are of coloured glaſs; or in the open air, when it is illuſtrated with clouds of various colours.

There are yet other ways by which I have produced Whiteneſs: as by caſting ſeveral colours from two or more priſms upon the ſame place, by refracting a beam of light with two or three priſms ſucceſſively, to make the diverging colours converge again; by reflecting one colour upon another; and by looking through a priſm on an object of many colours; and, which is equivalent to Mr. *Hook's* way of mixing colours, by concave-wedges filled with coloured liquors, I have obſerved the ſhadows of a painted glaſs-window to become white, where thoſe of many colours have at a great diſtance interfered. But yet, for further ſatisfaction, he may try, if he pleaſe, the effects of four or five ſuch wedges filled with liquors of as many colours.

Beſides all theſe, the colours of water-bubbles, and other thin pellucid ſubſtances, afford ſeveral inſtances of Whiteneſs, produced by their mixture; with one of which I ſhall conclude this particular. Let ſome water, in which a convenient quantity of ſoap or waſh-bal is diſſolved, be agitated into a froth; and after that froth hath

hath ſtood a while without further agitation, till you ſee the bubbles of which it conſiſts begin to break, there will appear a great variety of colours all over the top of every bubble, if you view them near at hand; but if you view them at ſo great a diſtance, that you cannot diſtinguiſh the colours one from another, the froth will appear perfectly White.

Experimentum Crucis affirmed.

Thus much concerning the deſign and ſubſtance of Mr. *Hook's* conſiderations. There are yet ſome particulars to be taken notice of; as the denial of the *Experimentum Crucis*. On this I choſe to lay the whole ſtreſs of my diſcourſe, which therefore was the principal thing to have been objected againſt. But I cannot be convinced of its inſufficiency by a ſhort bare denial, without aſſigning a reaſon for it. I am apt to believe it hath been miſunderſtood. For otherwiſe it would have prevented the diſcourſes about rarefying and ſplitting of rays; becauſe the deſign of it is to ſhew, that rays of divers colours, conſidered apart, do at equal incidences ſuffer unequal refractions, without being ſplit, rarefied, or any way dilated.

Hook's repreſentation of the Newtonian doctrine very imperfect.

In the conſiderations on my firſt and ſecond Propoſitions, Mr. *Hook* hath rendered my doctrine of unequal refrangibility very imperfect and maimed, by explicating it wholly by the ſplitting of rays; whereas I chiefly intended it in thoſe refractions, which are performed without that ſuppoſed irregularity, ſuch as the *Experimentum Crucis* might have informed him of. And in general I find, that whilſt he hath endeavoured to explicate my Propoſitions hypothetically, the more material ſuggeſtions, by which I deſigned to recommend them, have eſcaped his conſideration; ſuch as are the unchangeableneſs of the degree of refrangibility peculiar to any ſort of rays; the ſtrict analogy between the degrees of refrangibility and colours; the diſtinction between compounded and uncompounded colours; the unchangeableneſs of uncompounded colours; and the aſſertion, that if any one of the priſmatic colours be wholly intercepted, that colour cannot be new produced out of the remaining light by any further refraction or reflection whatſoever; and of what length and efficacy theſe particulars are for enforcing the theory, I deſire, therefore, may be now conſidered.

In

Degree of certainty in the Science of Colours.

In the laſt place, I ſhould take notice of a caſual expreſſion, which intimates a greater certainty in theſe things, than I ever promiſed, *viz. the certainty of Mathematical Demonſtrations.* I ſaid, indeed, that the ſcience of colours was mathematical, and as certain as any other part of Optics; but who knows not that Optics, and many other mathematical ſciences, depend as well on phyſical ſciences, as on mathematical demonſtrations? And the abſolute certainty of a ſcience cannot exceed the certainty of its principles. Now the evidence, by which I aſſerted the propoſitions of colours, is in the next words expreſſed to be from experiments, and ſo but phyſical: whence the Propoſitions themſelves can be eſteemed no more than phyſical principles of a ſcience. And if thoſe principles be ſuch, that on them a mathematician may determine all the phænomena of colours, that can be cauſed by refractions, and that by diſputing or demonſtrating after what manner, and how much, thoſe refractions do ſeparate or mingle the rays, in which ſeveral colours are originally inherent; I ſuppoſe the ſcience of colours will be granted mathematical, and as certain as any part of Optics. And that this may be done, I have good reaſon to believe, becauſe ever ſince I became firſt acquainted with theſe principles, I have, with conſtant ſucceſs in the events, made uſe of them for this purpoſe.

Thus much I have thought fit to return to Mr. *Hook's* conſiderations, which, that it may bring ſatisfaction in this part of Optics to the honourable members of the Royal Society, hath been the rule of my intentions. Yours, &c.

VII.

To Mr. OLDENBURG.

SIR, Cambridge.

I Received you letter, with Mr. *Hugen's* kind preſent, which I have viewed with great ſatisfaction, finding it full of very ſubtle and uſeful ſpeculation very worthy of the author. I am glad that we are to expect another diſcourſe of the *vis centrifuga*, which ſpeculation may prove of good uſe in natural philoſophy and aſtronomy, as well as mechanics.

Thus, for inſtance, if the reaſon why the ſame ſide of the moon is ever towards the earth, be the greater *conatus* of the other ſide to recede from it; it will follow, upon ſuppoſition of the earth's motion about the ſun, that the greateſt diſtance of the ſun from the earth is to the greateſt diſtance of the moon from the earth not greater than 10000 to 56: and therefore the parallax of the ſun will not be leſs than $\frac{56}{10000}$ of the parallax of the moon. Becauſe were the ſun's diſtance leſs in proportion to that of the moon, ſhe would have a greater *conatus* from the ſun than from the earth. I thought alſo ſometimes, that the moon's libration might depend upon her *conatus* from the ſun and earth compared together, till I apprehended a better cauſe.

Eſtimation of the ſun's parallax by the moon's.

In the demonſtration of the 18th Propoſition, *De deſcenſu gravium*, there ſeems to be an illegitimate ſuppoſition; namely, that the flexures at B and C do not hinder the motion of the deſcending body. For in reality they will hinder it; ſo that the body, which deſcends from A, ſhall not acquire ſo great velocity when arrived at D, as one which deſcends from E. If this ſuppoſition be made, becauſe a body deſcending by a curve line meets with no ſuch oppoſition, and this propoſition is laid down in order to the contemplation of motion in curve lines; then it ſhould have been ſhewn, that though rectilinear flexures do hinder, yet the infinitely little flexures, which are in curves, though infinite in number, do not at all hinder the motion.

An illegitimate ſuppoſition in Mr. Hugen's demonſtration of his 18th Prop. *de deſcenſu gravium*.

The rectifying curve lines by that way, which Mr. *Hugens* calls evolution, I have been ſome time conſidering alſo; and have met with a way of reſolving it, which ſeems more ready and free from the trouble of calculation, than that of Mr. *Hugens*. If he pleaſe, I will ſend it him. The Problem alſo is capable of being improved, by being propounded thus more generally.

Evolution.

Curvas invenire qualeſcunque, quarum longitudines cum propoſitæ alicujus Curvæ longitudine, vel cum areâ ejus, ad datam lineam applicatâ, comparari poſſunt.

Concerning the buſineſs of colours, when Mr. *Hugens* hath ſhewn how White may *be produced out of two uncompounded colours*, *I will tell him, why he can conclude nothing from that*; my meaning

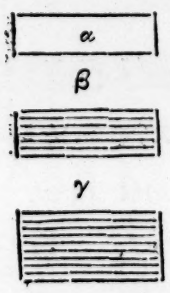
White not to be produced out of two uncompounded Colours.

meaning was, that ſuch a White, were there any ſuch, would have different properties from the White which I had reſpect to, when I deſcribed my Theory; that is, from the White of the ſun's immediate light, of the ordinary objects of our ſenſes, and of all white phænomena that have hitherto fallen under my obſervation. And thoſe different properties would evince it to be of a different conſtitution: inſomuch, that ſuch a production of White would be ſo far from contradicting, that it would rather illuſtrate and confirm my Theory; becauſe by the difference of that from other Whites, it would appear, that other Whites are not compounded of only two colours like that. And therefore if Monſ. *H.* would prove any thing, it is requiſite that he do not only produce out of two primitive colours a White, which to the naked eye ſhall appear like other Whites, but alſo ſhall agree with them in all other properties.

But to let you underſtand wherein ſuch a White would differ from other Whites, and why from thence it would follow, that other Whites are otherwiſe compounded; I ſhall lay down this poſition.

That a compounded colour can be reſolved into no more ſimple colours, than thoſe of which it is compounded.

This ſeems to be ſelf evident; and I have alſo tried it ſeveral ways, and particularly by this which follows. Let α repreſent an



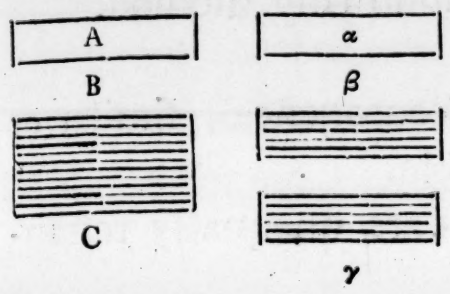
oblong piece of White paper, about $\frac{1}{3}$ or $\frac{1}{4}$ of an inch broad, and illuminated in a dark room, with a mixture of two colours caſt upon it from two priſms; ſuppoſe a deep Blue and Scarlet, which muſt ſeverally be as uncompounded as they can conveniently be made. Then, at a convenient diſtance, ſuppoſe of ſix or eight yards, view it through a clear triangular glaſs or cryſtal priſm, held parallel to the paper; and you ſhall ſee the two colours parted from one another in the faſhion of two images of the paper, as they are repreſented at β and γ; where ſuppoſe β the Scarlet, and γ the Blue, without Green, or any other colour between them.

Now from the aforeſaid poſition I deduce theſe two concluſions.

1. That if there were found out a way to compound White of two ſimple colours, that White would be again reſolvable into no more

more than two. 2. That if other Whites, as that of the ſun's light, &c. be reſolvable into more than two ſimple colours, as I find by experiment that they are, then they muſt be compounded of more than two.

To make this plainer, ſuppoſe that A repreſents a White body, illuminated by a direct beam of the ſun, tranſmitted through a ſmall hole into a dark room; and α, ſuch another body, illuminated by a mixture of two ſimple colours; which, if poſſible, may make it alſo appear of a White colour exactly like A. Then, at a convenient diſtance, view theſe two Whites through a priſm; and A will be changed into a ſeries of all colours, Red, Yellow, Green, Blue, Purple, with their intermediate degrees ſucceeding in order from B to C. But α, according to the aforeſaid experiment, will only yield thoſe two colours of which it was compounded; and thoſe not conterminate like the colours at BC, but ſeparate from one another, as at β and γ, by means of the different refrangibility of the rays, to which they belong. And thus by comparing theſe two Whites, they would appear to be of a different conſtitution, and A to conſiſt of more colours than α. So that what Monſ. *H.* contends for, would rather advance my Theory by the acceſs of a new kind of White, than conclude againſt it. But I ſee no hopes of compounding ſuch a White.



As for Monſ. *H.* his expreſſion, *That I maintain my doctrine with ſome concern*; I confeſs it was a little ungrateful to me, to meet with objections which had been anſwered before, without having the leaſt reaſon given me, why thoſe anſwers were inſufficient. Thoſe anſwers were to ſhew, that there are other ſimple colours beſides Blue and Yellow; I inſtanced in a ſimple or homogeneal Green, ſuch as cannot be made by mixing Blue and Yellow, or any other colours. And I alſo ſhewed why, ſuppoſing that all colours might be produced out of two, yet it would not follow that thoſe two are the only original colours. The reaſons I deſire you would compare with what hath now been ſaid of White. And ſo the neceſſity of all colours to produce White,

might have appeared by that experiment, where I ſay, That if any colour at the lens be intercepted, the Whiteneſs (which is compounded of them all) will be changed into (the reſult of) the other colours.

However, ſince there ſeems to have happened ſome miſunderſtanding between us, I ſhall endeavour to explain myſelf a little further in theſe things, according to the following method.

DEFINITIONS.] I. I call that Light homogeneal, ſimilar or uniform, whoſe rays are equally refrangible.

II. And that heterogeneal, whoſe rays are unequally refrangible.

Note. There are but three affections of light, in which I have obſerved its rays to differ, *viz. refrangibility*, *reflexibility* and *colour*; and thoſe rays which agree in refrangibility, agree alſo in the other two, and therefore may well be defined homogeneal; eſpecially ſince men uſually call thoſe things homogeneal, which are ſo in all qualities that come under their knowledge, though in other qualities, that their knowledge extends not to, there may poſſibly be ſome heterogeneity.

III. Thoſe colours I call ſimple, or homogeneal, which are exhibited by homogeneal light.

IV. And thoſe compounded or heterogeneal, which are exhibited by heterogeneal light.

V. Different colours, I call not only the more eminent ſpecies, Red, Yellow, Green, Blue, Purple, but all other the minuteſt gradations; much after the ſame manner that not only the more eminent degrees of muſick, but all the leaſt gradations are eſteemed different ſounds.

PROPOSITIONS.] I. The ſun's light conſiſts of rays differing by indefinite degrees of refrangibility.

II. Rays which differ in refrangibility when parted from one another, do proportionally differ in the colours which they exhibit. Theſe two Propoſitions are matter of fact.

III. There are as many ſimple or homogeneal colours, as degrees of refrangibility: for to every degree of refrangibility belongs

longs a different colour, by Prop. II. and that colour is ſimple, by Def. I. and III.

IV. Whiteneſs, in all reſpects like that of the ſun's immediate light, and of all the uſual objects of our ſenſes, cannot be compounded of two ſimple colours alone: for ſuch a compoſition muſt be made by rays that have only two degrees of refrangibility, by Def. I. and III; and therefore it cannot be like that of the ſun's light, by Prop. I; nor, for the ſame reaſon, like that of ordinary White objects.

V. Whiteneſs, in all reſpects like that of the ſun's immediate light, cannot be compounded of ſimple colours, without an indefinite variety of them: for to ſuch a compoſition there are requiſite rays endued with all the indefinite degrees of refrangibility, by Prop. I. And thoſe infer as many ſimple colours, by Def. I. and III. and Prop. II. and III.

To make theſe a little plainer, I have added alſo the Propoſitions that follow.

VI. The rays of light do not act on one another in paſſing through the ſame medium. This appears by ſeveral former paſſages, and is capable of further proof.

VII. The rays of light ſuffer not any change of their qualities from refraction.

VIII. Nor afterwards from the adjacent quiet medium. Theſe two Propoſitions are manifeſt *de facto* in homogeneal light, whoſe colour and refrangibility is not at all changeable, either by refraction, or by the contermination of a quiet medium. And as for heterogeneal light, it is but an aggregate of ſeveral ſorts of homogeneal light; no one ſort of which ſuffers any more alteration than if it were alone; becauſe the rays act not on one another, by Prop. VI. and therefore the aggregate can ſuffer none. Theſe two Propoſitions alſo might be further proved apart by experiments, too long to be here deſcribed.

IX. There can no homogeneal colours be educed out of light by refraction, which were not commixt in it before; becauſe by Prop. VII. and VIII. refraction changeth not the qualities of the rays, but only ſeparates thoſe which have diverſe qualities, by means of the different refrangibility.

X. The ſun's light is an aggregate of an indefinite variety of homogeneal colours; by Prop. I. III. and IX. And hence it is, that I call homogeneal colours alſo primitive or original.

And thus much concerning colours.

Monſ. *H.* has thought fit to inſinuate, that the aberration of rays, by their different refrangibility, is not ſo conſiderable a diſadvantage in glaſſes, as I ſeemed to be willing to make men believe, when I propounded concave mirrors as the only hopes of perfecting teleſcopes. But if he pleaſe to take his pen, and compute the errors of a glaſs and ſpeculum, that collect rays at equal diſtances, he will find how much he is miſtaken; and that I have not been extravagant, as he imagines, in preferring reflexions. And as for what he ſays of the difficulty of the *praxis*, I know it is very difficult; and by thoſe ways which he attempted it, I believe unpracticable. But there is a way inſinuated in the Tranſactions, p. 3080, by which it is not improbable, but that as much may be done in large teleſcopes, as I have thereby done in ſhort ones; but yet not without more than ordinary diligence and curioſity.

Pray with theſe notes return my thanks to Mr. *Hugens* for his book.

Of Sluſius's method of Tangents.

By a former letter of your's, I was a little dubious whether Mr. *Sluſius* might not apprehend, by what you wrote to him concerning me, that I pretended to his method of drawing tangents, until I underſtood by Mr. *Collins* that you ſignified to him, that you thought it here of a later date. For it ſeems to me, that he was acquainted with it ſome years before he printed his *Meſolabum*, and conſequently before I underſtood it. But if it had been otherwiſe, yet ſince he firſt imparted it to his friends and the world, it ought deſervedly to be accounted his. As for the methods, they are the ſame; though I believe derived from different principles. But I know not whether his principles afford it ſo general as mine; which extends to equations affected with ſurd terms, without reducing them to another form. But, if you pleaſe, let this paſs.

The incongruities you ſpeak of, I paſs by. But I muſt, as formerly, ſignify to you, that I intend to be no farther ſollicitous about

about matters of philoſophy. And therefore I hope you will not take it ill, if you find me never doing any thing more in that kind: or rather that you will favour me in my determination, by preventing, ſo far as you can conveniently, any objections, or other philoſophical letters, that may concern me. For your proffer about my quarterly payments, I thank you. But I would not have you trouble yourſelf to get them excuſed, if you have not done it already. And now being tired with this long letter, I muſt, in haſte, write myſelf your's, &c.

VIII.

Mr. NEWTON'S anſwer to Monſ. HUGENIUS'S letter of Jan. 14, $16\frac{72}{73}$.

SIR,

IT ſeems to me, that M. *Hugens* takes an improper way of examining the nature of colours, whilſt he proceeds upon compounding thoſe that are already compounded, as he doth in the former part of his letter. Perhaps he would ſooner ſatisfy himſelf, by reſolving light into colours, as far as may be done by art, and then by examining the properties of thoſe colours apart; and afterwards by trying the effects of reconjoining two or more, or all of thoſe: and, laſtly, by ſeparating them again, to examine what changes that reconjunction had wrought in them. This will prove a tedious and difficult taſk, to do it as it ought to be done; but I could not be ſatisfied, till I had gone through it. However, I only propound it, and leave every man to his own method.

The fallacy of Mr. Hugens's method of examining Colours.

As to the contents of his letter, I conceive my former anſwer to the *quære* about the number of colours is ſufficient, which was to this effect. That all colours cannot practically be derived out of Yellow and Blue, and conſequently that thoſe hypotheſes are falſe, which imply they may. If you aſk, what colours cannot be derived out of Yellow and Blue, I anſwer, none of thoſe which I defined to be original; and if he can ſhew by experiment how they may, I will acknowledge myſelf in an error.

All colours not to be derived from Yellow and Blue.

Nor

Nor is it eaſier to frame an hypotheſis, by aſſuming only two original colours, rather than an indefinite variety; unleſs it be eaſier to ſuppoſe, that there are but two figures, ſizes, and degrees of velocity, or force of the æthereal corpuſcles or pulſes, rather than an indefinite variety, which certainly would be a very harſh ſuppoſition. No man wonders at the indefinite variety of waves of the ſea, or of ſands on the ſhore; but were they all of but two ſizes, it would be a very puzzling phænomenon. And I ſhould think it as unaccountable, if the ſeveral parts or corpuſcles of which a ſhining body conſiſts, which muſt be ſuppoſed of various figures, ſizes, and motions, ſhould impreſs but two ſorts of motion on the adjacent æthereal medium; or any other way beget but two ſorts of rays. But to examine how colours may be thus explained hypothetically, is, beſides, my purpoſe. I never intended to ſhew wherein conſiſts the nature and difference of colours, but only to ſhew, that *de facto* they are original and immutable qualities of the rays which exhibit them; and to leave it to others to explicate, by mechanical hypotheſes, the nature and difference of the qualities; which I take to be no very difficult matter. But I would not be underſtood, as if their difference conſiſted in the different refrangibility of thoſe rays; for that different refrangibility conduces to their production no otherwiſe, than by ſeparating the rays whoſe qualities they are. Whence it is, that the ſame rays exhibit the ſame colours, when ſeparated by any other means; as by their different reflexibility; a quality not yet diſcourſed of.

White not to be produced from Yellow, Green and Blue.

In the next particular, where M. *Hugens* would ſhew, that it is not neceſſary to mix all colours for the production of White; the mixture of Yellow, Green, and Blue, without Red or Violet, which he propounds for that end, will not produce White, but Green; and the brighteſt part of the Yellow will afford no other colour but Yellow, if the experiment be made in a room well darkened, as it ought, becauſe the coloured light is much weakened by the reflexion, and ſo apt to be diluted by the mixing of the other ſcattering light. But yet there is an experiment or two mentioned in my letter in the Tranſactions, Numb. 88, by which I have produced White out of two colours alone, and that variouſly,

ouſly, as out of Orange and a full Blue, and out of Red and pale Blue, and out of Yellow and Violet; as alſo out of other pairs of intermediate colours. The moſt convenient experiment for performing this was, that of caſting the colours of one priſm upon thoſe of another, after a due manner. But what M. *Hugens* can deduce from hence, I ſee not; for the two colours were compounded of all others, and ſo the reſulting White, to ſpeak properly, was compounded of them all, and only decompounded of thoſe two. For inſtance, the Orange was compounded of Red, Orange, Yellow, and ſome Green; and the Blue and Violet, full Blue, light Blue, and ſome Green, with all their intermediate degrees; and conſequently the Orange and Blue together made an aggregate of all colours, to conſtitute the White. Thus if one mix Red, Orange and Yellow powders, to make an Orange; and Green, Blue, and Violet powders, to make a Blue: and, laſtly, the two mixtures to make a Grey: that Grey, though decompounded of no more than two mixtures, is yet compounded of all the ſix powders as truly, as if the powders had been all mixed at once. This is ſo plain, that I conceive there can be no further ſcruple; eſpecially to them who know how to examine whether a colour be ſimple or compounded, and of what colours it is compounded: which having explained in another place, I need not now repeat. If therefore M. *Hugens* would conclude any thing, he muſt ſhew how White may be produced out of two uncompounded colours; which, when he hath done, I will further tell him why he can conclude nothing from that. But I believe there cannot be found an experiment of that kind; becauſe, as I remember, I once tried by gradual ſucceſſion the mixture of all pairs of uncompounded colours, and though ſome of them were paler and nearer to White than others, yet none could be truly called White. But it being ſome years ſince this trial was made, I remember not well the circumſtances; and therefore recommend it to others to be tried again.

Of the aberrant rays in Teleſcopes.

In the laſt place, had I thought the diſtinctneſs of the picture which (for inſtance) a twelve-foot object-glaſs caſts into a darkened room, to be ſo contrary to me as M. *Hugens* is pleaſed to affirm, I ſhould have mended my Theory in that point, before I propounded

propounded it. For that I had thought on that difficulty, you may eaſily gueſs by an expreſſion ſomewhere in my firſt letter to this purpoſe, that I wondered how teleſcopes could be brought to ſo great perfection by refractions, which were ſo irregular. But to take away the difficulty, I muſt acquaint you, firſt, That though I put the greateſt lateral error of the rays from one anther to be about $\frac{1}{50}$ of the glaſs's diameter, yet their greateſt error from the points, on which they ought to fall, will be but $\frac{1}{100}$ of that diameter: and then, that the rays, whoſe error is ſo great, are but very few, in compariſon to thoſe which are refracted more juſtly. For the rays, which fall upon the middle parts of the glaſs, are refracted with ſufficient exactneſs; as alſo are thoſe that fall near the perimeter, and have a mean degree of refrangibility. So that there remain only the rays which fall near the perimeter, and are moſt or leaſt refrangible, to cauſe any ſenſible confuſion in the picture. And theſe are yet ſo much further weakened by the greater ſpace, through which they are ſcattered; that the light which falls on the due point is infinitely more denſe, than that which falls on any other point round about it. Which, though it may ſeem a paradox, yet is eaſily demonſtrable. Yea, although the light, which paſſeth through the middle parts of the glaſs, were wholly intercepted, yet would the remaining light convene infinitely more denſe at the due points, than at other places. And by this axis of denſity, the light which falls in, or inſenſibly near, the juſt point, may, I conceive, ſtrike the ſenſorium ſo vigorouſly, that the impreſs of the weak light, which errs round about it, ſhall, in compariſon, not be ſtrong enough to be animadverted, or to cauſe any more ſenſible confuſion in the picture, than is found by experience. This I conceive is enough to ſhew why the picture appears ſo diſtinct, notwithſtanding the irregular refraction. But if this ſatisfy not, M. *Hugens* may try, if he pleaſe, how diſtinct the picture will appear, when all the lens is covered, excepting a little hole next its edge on one ſide only. And if in this caſe he pleaſe to meaſure the breadth of the colours, thus made at the edge of the ſun's picture, he will perhaps find it approach nearer to my proportion than he expects.

IX.

To Mr. OLDENBURG.

SIR,

WHEN you ſhewed me Mr. *Line's* ſecond letter, I remember I told you, that I thought an anſwer in writing would be inſignificant; becauſe the diſpute was not about any ratiocination, but my veracity in relating an experiment, which he denies will ſucceed, as it is deſcribed in my printed letter: for this is to be decided not by diſcourſe, but new trial of the experiment.

What it is that impoſes upon Mr. *Line*, I cannot imagine; but I ſuſpect he has not tried the experiment ſince he acquainted himſelf with my Theory; but depends upon his old notions, taken up before he had any hint given to obſerve the figure of the coloured image. I ſhall deſire him, therefore, before he returns any anſwer, to try it once more for his ſatisfaction, and according to this manner.

Directions for making the experiment with the Priſm.

Let him take any priſm, and hold it ſo that its axis may be perpendicular to the ſun's rays; and in this poſture let it be placed as cloſe as may be to the hole, through which the ſun ſhines into a dark room; which hole may be about the bigneſs of a pea. Then let him turn the priſm ſlowly about its axis, and he ſhall ſee the colours move upon the oppoſite wall; firſt, towards that place to which the ſun's direct light would paſs, if the priſm were taken away; and then back again. When they are in the middle of theſe two contrary motions, that is, when they are neareſt that place to which the ſun's direct ray tends, there let him ſtop; for then are the rays equally refracted on both ſides the priſm. In this poſture of this priſm, let him obſerve the figure of the colours; and he ſhall find it not round, as he contends, but oblong; and ſo much the more oblong as the angle of the priſm, comprehended by the refracting planes, is bigger; and the wall, on which the colours are caſt, more diſtant from the priſm; the colours, Red, Yellow, Green, Blue, Purple, ſucceeding in order, not from one ſide of the figure to the other, as in Mr. *Line's* con-

jecture, but from one end to the other; and the length of the figure being not parallel, but tranſverſe to the axis of the priſm. After this manner I uſed to try the experiment; and it will not ſucceed well, if the day be not clear, and the priſm placed cloſe to the hole; or ſo near at leaſt, that all the ſun's light, that comes from the hole, may paſs through the priſm alſo, ſo as to appear in a round form, if intercepted by a paper immediately after it has paſſed the priſm.

When Mr. *Line* has tried this, I could wiſh he would proceed a little further, to try that which I called the *experimentum crucis*. For when he has tried them (which, by his denying them, I know he has not done yet as they ſhould be tried) I preſume he will reſt ſatisfied.

Three or four days after you gave me a ſight of Mr. *Line's* ſecond letter, I remember I thereupon ſhewed the firſt of theſe two experiments to that gentleman, whom you found with me when you gave me that viſit. And whilſt I was ſhewing it to him, *A. H.* a member of the Royal Society, came in; and I ſhewed it to him alſo. And you may remember, that *R. H.* two or three years ago, in a letter read before the Royal Society, and tranſmitted to me, gave teſtimony not only to the experiments queſtioned by Mr. *Line*, but to all thoſe ſet down in my firſt letter about colours, as having tried them himſelf: and when you read Mr. *Line's* letter at a meeting of the ſaid Society, and was pleaſed to do me the favour to propound the experiment to be tried in their preſence, *R. H.* ſpoke of it to them as a thing not to be queſtioned. But if it have not yet been tried before them, and any of them, upon Mr. *Line's* confidence, doubt of it; I promiſe, when I ſhall have the happineſs to be at any more of their aſſemblies, upon the leaſt hint, to ſhew them the trial of it; and I hope I ſhall not be troubleſome, becauſe it may be tried, though not ſo perfectly, even without darkening a room, or the expence of any more time than half a quarter of an hour; although, if Mr. *Line* perſiſt in his denial of it, I could wiſh it might be tried ſooner there, than I ſhall have an opportunity to be among them.

I have

I have returned you Mr. *Line's* letter. It came to my hands but this week; the gentleman by whom you ſent it having not yet been at *Cambridge*, but tranſmitting it to me from *Oxford*.

I had ſome thoughts of writing a further diſcourſe about colours to be read at one of your Aſſemblies; but find it yet againſt the grain, to put pen to paper any more on that ſubject. But however, I have one diſcourſe by me of that ſubject, written when I ſent my firſt letters to you about colours, and of which I then gave you notice. This you may command, when you think it will be convenient, if the cuſtom of reading weekly diſcourſes ſtill continue. In the mean while, I am, &c.

X.

To Mr. OLDENBURG.

SIR, Jan. 10, 167⅞.

CONCERNING the experiment of the glaſs and papers, I ſhould add theſe two to the former directions. One, that the glaſs be rubbed with a full handful of ſtuff, which may cover and rub all the glaſs at once: for thus its electric virtue will be more eaſily and vigorouſly excited, than if rubbed with a little only, doubled up but once or twice. This rubbing with the ſtuff, I ſuppoſe, rarifies and diffuſes the electric *effluvia* from the glaſs into the air; and the knocking or rubbing with the finger-ends puts the diffuſed *effluvia* into irregular motions. The other thing I would note is, that the papers may perhaps be too little, as well as too great. Too ſmall ones will be apter to ſtick to the glaſs or table. If the experiment be tried with a glaſs three or four inches broad, ſet about $\frac{1}{6}$ of an inch from the table, and the papers of a thin ſort of paper cut into triangular pieces, the ſides of thoſe triangles may not unfitly be about the 20th or 25th part of an inch, more or leſs. It may be beſt tried with bits of ſeveral ſizes put in at once; and if there be put in a piece or two of the wing of a fly, thoſe, I find, will move more eaſily, though ſcarce ſo variouſly. Theſe and the former directions obſerved, I cannot imagine how you ſhould miſs; though I cannot promiſe

Directions about an experiment of Electricity.

all things will appear juſtly to you as they do to me, there being unaccountable circumſtances, which may make a difference.

Reply to inſinuations of Mr. Hook's.

I am obliged to you, Sir, for your candour, in acquainting me with Mr. *Hook's* inſinuations. It is but a reaſonable piece of juſtice, I ſhould have an opportunity to vindicate myſelf from what may be undeſervedly caſt on me; and therefore, ſince you have been pleaſed to be my repreſentative there, and I have no means of knowing what is done but by you, I hope you will continue that equitable candour; though I think the preſent buſineſs of no great moment as to me, not imagining that the Royal Society are to be impoſed on in a thing ſo plain, or that Mr. *Hook* himſelf will perſiſt in a miſtake, when he hears the difference ſtated. The only thing I ſaid he could pretend taken from his hypotheſis, was the diſpoſition of æther to vibrate; and yet whilſt he graſps at all, he is likely to fall ſhort of this too. That æthereal vibrations are light, is his; but that æther may vibrate (which is all, I ſuppoſe) is to be had from a higher fountain: for that æther is a finer degree of air, and air a vibrating medium, are old notions, and the principles I go upon. I deſire Mr. *Hook* to ſhew me, therefore, I ſay not only the ſum of the hypotheſis I wrote, which is his inſinuation, but only part of it taken out of his *Micrographia*; but then I expect too, that he inſtance in what is his own. It is moſt likely he will pretend, I had from him the application of vibrations to the ſolution of the phænomena of thin plates: and yet all the uſe I make of vibrations, is, to ſtrengthen or weaken the reflecting power of the æthereal ſuperficies; which is ſo far from being in his *Micrographia*, that the laſt ſpring, when I told him of the reflecting power of the æthereal ſuperficies, he took it for a new notion; having till then ſuppoſed light to be reflected by the parts of groſs bodies. To the things that he has from *Des Cartes*, pray add this, *that the parts of ſolid bodies have a vibrating motion*, leaſt he ſhould ſay I had from him what I ſay about heat. And his having from *Des Cartes* the reduction of all colours to two, you may, if need be, explain further for me thus. That as *Des Cartes* puts every *globulus* to be urged forward on one ſide by the illuminated medium, and impeded on the other by the dark one; ſo Mr. *Hook* puts

puts every vibration to be promoted at one end, and retarded at the other by thoſe mediums; and thence both alike derive two modifications of light, on the two ſides of the refracted beam, for the production of all colours.

Directions about the proper management of the priſm, in anſwer to Mr. Line.

By Mr. *Gaſcoin's* letter, one might ſuſpect that Mr. *Linus* tried the experiment ſome other way than I did; and therefore I ſhall expect, till his friends have tried it according to my late diſcoveries: in which trial it may poſſibly be a further guidance to them to acquaint them, that the priſm caſts from it ſeveral images. One is that oblong one of colours which I mean, and this is made by two refractions only. Another there is, made by two refractions and an intervening reflexion; and this is round and colourleſs, if the angles of the priſm be exactly equal; but if the angles at the reflecting baſe be not equal, it will be coloured; and that ſo much the more, by how much unequaller the angles are; but yet not much unround, unleſs the angles be very unequal. A third image there is made by one ſimple reflexion; and this is always round and colourleſs. The only danger is in miſtaking the ſecond for the firſt. But they are diſagreeable not only by the length and lively colours of the firſt, but by its different motion too: for whilſt the priſm is turned continually the ſame way about its axis, the ſecond and third move ſwiftly, and go always on the ſame way till they diſappear; but the firſt moves ſlow; and grows continually ſlower, till it be ſtationary; and then turns back again, and goes back faſter and faſter, till it vaniſh in the place where it began to appear. If without darkening their room, they hold the priſm at their window in the ſun's open light; in ſuch a poſture, that its axis be perpendicular to the ſunbeams; and then turn it about its axis, they cannot miſs of ſeeing the firſt image: which having found, they may double up a paper once or twice, and make a round hole in the middle of it about $\frac{1}{2}$ or $\frac{3}{4}$ of an inch broad, and hold the paper immediately before the priſm, that the ſun may ſhine on the priſm through that hole; and the priſm being ſtayed and held ſteady, in that poſture which makes the image ſtationary; if the image then fall directly on an oppoſite wall, or on a ſheet of paper placed at the wall, ſuppoſe 15 or 20 foot from the priſm,

or

or further off, they will ſee that image in ſuch an oblong figure as I have deſcribed, with the Red at one end, the Violet at the other, and a blueiſh Green in the middle; and if they obſcure their room as much as they can by drawing curtains or otherwiſe, it will make the colours the more conſpicuous.

This direction I have ſet down, that no body, into whoſe hands a priſm ſhall happen, may find difficulty or trouble in trying it. But when Mr. *Linus's* friends have tried it thus, they may proceed to repeat it in a dark room, with a leſs hole made in their window-ſhut. And then I ſhall deſire, that they will ſend you a full and clear deſcription how they tried it, expreſſing the length, breadth, and angles of the priſm; its poſition to the incident rays, and to the window-ſhut; the bigneſs of the hole in that ſhut, through which the ſun ſhined on their priſm; what ſide of the priſm the ſun ſhined on; and at what ſide the light came out of it again; the diſtance of the priſm from the oppoſite paper or wall, on which the refracted light was caſt perpendicularly; and the length, breadth, and figure of the ſpace there illuminated by that light; and the ſituation of each colour within that figure: and if they pleaſe to illuſtrate their deſcription with a ſcheme or two, it will make the buſineſs plainer. By this means, if there be any difference in our way of experimenting, I ſhall be the better enabled to diſcern it, and give them notice where the failure is, and how to rectify it. I ſhould be glad too, if they would favour me with a deſcription of the experiment, as it has been hitherto tried by Mr. *Linus*; that I may have an opportunity to conſider, what there is in that which makes againſt me. And becauſe Mr. *Gaſcoin* ſeems to ſuſpect, that my directions, ſent Mr. *Linus*, differ from what I have printed; I deſire alſo that he would ſignify wherein he thinks they may differ, ſo as to need reconciling. Fuller they are, but not different, nor any other than I have followed above theſe ſeven years.

History of the diſpute with Mr. Line.

As for my ſuſpicion, that Mr. *Linus* might poſſibly rely on old experiments; his quoting Sir *Kenelm Digby* for a by-ſtander, might have made any other ſtranger to his way, as well as me, ſuſpect it. But I wonder moſt at Mr. *Gaſcoin's* inſinuation, as if I influenced the preſs in what concerns Mr. *Linus* and me. You know, Sir,

I never

I never ſpoke or hinted a ſyllable to you, concerning printing or not printing any thing of Mr. *Linus*; nor ſo much as knew of the printing his firſt letter, till it was out in the Tranſactions. When you ſent it to me, I, out of a great deſire to avoid controverſies (which, as you know, I had entertained long before) wrote back to you, that I had no mind to meddle with it: but, as I was ready to ſeal that letter, I added a poſtſcript to this purpoſe: That ſeeing Mr. *Linus* was deſigning ſomething about light for the preſs, to prevent publiſhing his miſtake, you might, if you thought fit, ſignify to him (but not from me) that the experiment was tried otherwiſe than he ſuggeſted; and that in ſuch and ſuch reſpects, which I there named. And the ſubſtance of this poſtſcript was that you publiſhed at the end of his firſt letter, on which Mr. *Gaſcoin* here animadverts; but was ſo far from being deſigned for the preſs by me, that the firſt ſight of it, together with his letter in the Tranſactions, made me ſay to one, that I wiſhed they had been ſuppreſſed; for I doubted the printing them would make Mr. *Linus* unquiet, and ſo in the end create me trouble. As for his ſecond letter which you ſhewed me at *London*, I returned it again to you ſo ſoon as I had read it, and never ſaw it ſince, perſiſting in my deſire to avoid the controverſy. And at my returning it, you moved me for an anſwer, with this argument, that if I waved it, Mr. *Linus* was like to make the more ſtir: to which I replied, that the buſineſs, being about matter of fact, was not proper to be decided by writing, but by trying it before competent witneſſes. Whereupon, at your motion, I told you what was requiſite; and by your procurements, preparations were accordingly made for its trial at the next aſſembly of the Royal Society, as I underſtood by Mr. *Hook*: but the day proved cloudy, and before another aſſembly I returned to *Cambridge*, and from that time never enquired after nor regarded the matter further, till you ſent me Mr. *Linus's* third letter. This is the hiſtory of Mr. *Linus's* buſineſs, ſo far as I know it: which I have ſet down, that his friends may ſee he has not been dealt with obliquely, as they ſeem to apprehend. All, I think, that they can object to you is, that you were at a ſtand, becauſe you could not engage me in the controverſy; and to me, that I had no mind

mind to be engaged: a liberty every body has a right to, and may gladly make uſe of ſometimes at leaſt, and eſpecially if he want leiſure, or meet with prejudice or groundleſs inſinuations. But I hope to find none of this in Mr. *Gaſcoin*. The handſome genius of his preſent letter, makes me hope it for the future. In the mean time I deſire with him, that you would publiſh Mr. *Linus's* letters as ſoon as you can conveniently, to prevent further miſapprehenſions. I am, &c.

XI.

To Mr. OLDENBURG.

SIR, Cambridge, Feb. 29, 167⅞.

BY reading Mr. *Linus's* letter, when you ſhewed it to me at *London*, I retained only a general remembrance, that Mr. *Linus* denied what I affirmed, and ſo could ſay nothing in particular to it. But having the opportunity to read it again in Numb. CXXI. of the Tranſactions, I perceive he would perſuade you, that the information you gave him about the experiment is as inconſiſtent with my printed letters, as with experience. And therefore, leſt any, who have not read thoſe letters, ſhould take my ſilence in this point for an acknowledgment, I thought it not amiſs to ſend you ſomething in anſwer to this alſo.

He tells you, that "whereas you aſſure him, Firſt, that the "experiment was made in clear days; ſecondly, that the priſm "was placed cloſe to the hole, ſo that the light had no room to "diverge; and, thirdly, that the image was not parallel, but "tranſverſe to the axis of the priſm: if theſe aſſertions be com-"pared with my relation of the experiment in the Phil. Tranſ. "Numb. LXXX. p. 3076. it will evidently appear they cannot be "admitted, as being directly contrary to what is there delivered." His reaſons are theſe:

Firſt, that I ſaid the ends of the long image ſeemed ſemicircular, which, ſays he, "never happens in any of the three caſes "above-ſaid." But this is not to ſet me at odds with myſelf, but with the experiment. For it is there deſcribed to happen in them all;

all; and I ſtill ſay it doth happen in them. Let others try the experiment, and judge.

Further he ſays, that the priſm is placed at a diſtance from the hole in the ſcheme of the experiment in Numb. LXXXIV. p. 4091 (a). But what if it were ſo there? For that is the ſcheme of a demonſtration, not of the experiment; and would have ſerved for the demonſtration, had the diſtance been put twenty times greater than it is. In the ſchemes of the experiment, Numb. LXXX. p. 3086 (b), and Numb. LXXXIII. p. 4061 (c), it is repreſented cloſe, and cloſe enough the ſcheme, Numb. LXXXIII. p. 4061 (c). But Mr. *Linus* thought fit to wink at thoſe, and pitch upon the ſcheme of a demonſtration; and ſuch a ſcheme too, as hath no hole at all repreſented in it. For the ſcheme, Numb. LXXXIV. p. 4091, is this (a). In which the rays are not ſo far diſtant from one another at G, L; but that the hole, had I expreſſed it, might have been put there, and yet have comprehended them. But if we ſhould put the hole at *x*, their decuſſation, yet it will not be any thing to his purpoſe; the diſtance *x*G, or *x*L, being but about half the breadth of a ſide of the priſm ($\frac{1}{2}$AC); which I conceive is not a twentieth part of the diſtance requiſite in his conjecture.

Thirdly, he ſays, that "more might be ſaid out of my rela-"tion to ſhew, that the image was not tranſverſe; for if it had "been tranſverſe, I could not have been ſurprized (as I ſaid I "was) to ſee the length thereof ſo much exceed the breadth; it "being a thing ſo obvious and eaſy to be explicated by the ordi-"nary rules of refraction." But, on the contrary, it may rather be ſaid, that if the image had been parallel, I could not have been ſurprized to ſee the length thereof ſo much exceed the breadth, it being a thing ſo extremely obvious, as not to need any explication. For who, that had but common ſenſe, and ſaw the whole priſm, or a good part of it illuminated, could not expect that the light ſhould have the ſame long figure upon the wall, that it had when it came out of the priſm? Mr. *Linus*, therefore, while he would ſtrengthen his argument, by repreſenting me well ſkilled in optics, does but overthrow it. But where he ſays, "I could not have been ſurprized at the length, had the "image been *tranſverſe*, it being a thing ſo obvious, and eaſy to

(a) See p. 308, lin. 20. (b) See p. 306, lin. 30. (c) See p. 312.

" be explicated by the ordinary rules of refraction." Let any man take the experiment entire, as I have there delivered it, that is with this condition, that the refractions on both ſides the priſm were equal, and try if he can reconcile it with the ordinary rules of refraction. On the contrary, he may find the impoſſibility of ſuch a reconciliation demonſtrated, in my anſwer to *P. Pardies*. Num. LXXXIV. p. 4091 (a).

In the laſt place he objects, that my ſaying, in Num. LXXX. p. 3077 (b), that the incident refractions were, in the experiment, equal to the emergent, proves again that the long image was parallel. And yet that very ſaying is a ſufficient argument that I meant the contrary; becauſe it becomes wholly impertinent, if applied to a parallel image; but in the other caſe is a very neceſſary circumſtance. What is added, therefore, of *P. Pardies*, might have been ſpared; eſpecially ſince that learned perſon underſtood my diſcourſe to be meant of a tranſverſe image, and acquieſced in my anſwers.

This in anſwer to Mr. *Linus's* letter. And now to take away the like ſuſpicion from his friends, if my declaration of my meaning ſatisfy not; I ſhall note ſome further paſſages in my letters, whereby they may ſee how I was to be underſtood from the beginning, as to the aforeſaid circumſtances.

For the day; I expreſs every where that the experiment was tried in the ſun's light; and in Num. LXXX. p. 3077 (c), that the breadth of the image by meaſure anſwered to the ſun's diameter. But becauſe it is pretended I was impoſed upon; I would aſk, what the experiment, as it is advanced to that which I called the *experimentum crucis*, can have to do with a cloudy day. For if the *experimentum crucis*, which is that which I depend on, can have nothing to do with a cloudy day, then it is to no purpoſe to talk of a cloudy day in the firſt experiment, which does but lead on to that. But if this ſatisfy not, let the Tranſactions, in Num. LXXXIII. p. 4060, be conſulted (d). For there I tell you, how, by applying a lens to the priſm, the ſtreight edges of the oblong image became diſtincter than they would have been without the lens: a circumſtance which cannot happen in Mr. *Linus's* caſe of a bright cloud.

(a) See p. 308. (b) See p. 297. (c) See p. 297, lin. 4 and 5. (d) See p. 311, lin. 25—27.

For

For the poſition of the priſm; I tell you, Num. LXXX. p. 3076 (a), that it was placed at the ſun's entrance into the chamber: and in page 3085 (b), I bad to make a hole in the ſhut, and there place the priſm: and in the next page I ſay again, that the priſm ABC is to be ſet cloſe by the hole F of the window EG; and accordingly repreſent it cloſe in the figure. Alſo in page 3077 (c) I tell you, that the diſtance of the image from the hole, or priſm, was 22 feet: which is as much as to ſay, that the priſm, ſuppoſe that ſide of it next the hole, was as far from the image as the hole itſelf was; and conſequently that the priſm and the hole were contiguous. Alſo in p. 3078 (d), where inſtead of the window-ſhut I made uſe of a hole in a looſe board, I tell you expreſsly, that I placed the board behind the priſm. All theſe paſſages are in my very firſt letter about colours. And who, therefore, would imagine, that any one, that had read that letter, ſhould ſo much as ſuſpect, that I placed the priſm, I ſay not at ſo great a diſtance as Mr. *Linus* ſuppoſes, but at any diſtance worth conſidering.

Laſtly, for the poſition of the image, it is repreſented tranſverſe to the axis of the priſm in the figures Num. LXXX. p. 3086 (e), Num. LXXXIII. p. 4061 (f), and Num. LXXXV. p. 5016 (g). And in Num. LXXXVIII. p. 5093 (h), where I made uſe of two croſs priſms, I tell you expreſsly, that the image was croſs to both of them, at an angle of 45 degrees. The calculation alſo, Num. LXXX. p. 3077 (i), are not to be underſtood without ſuppoſing the image croſs. Nor are my notions about different refrangibility otherwiſe intelligible. For in Mr. *Linus's* ſuppoſition, the rays that go to the two ends of the image are equally refracted. So for colours: The Red, according to my deſcription, falls at one end of the image; and the Blue, at the other; which cannot happen but in a tranſverſe image. The ſame poſition is alſo demonſtrable from what I ſaid in Num. LXXX. p. 3076 (k), about turning the long image into a round one, by the contrary refractionof a ſecond priſm, further explained in Num. LXXXIII. p. 4061 (l). For this is not to be done in Mr. *Linus's* ſurmiſe

(a) See p. 295, lin. 10. (b) See p. 306, lin. 5—8. (c) See p. 296, lin. 28.
(d) See p. 298, lin. 18. (e) See p. 306, lin. 30. (f) See p. 312. (g) See p. 317.
(h) See p. 330, lin. 29. (i) See p. 296. (k) See p. 296, lin. 20. (l) See p. 312.

of a parallel image; and therefore had Mr. *Linus* conſidered it, he could never have run into that ſurmiſe.

This I ſuppoſe is enough to manifeſt the three particulars: any one of which, being evidenced, is ſufficient to take away the ſcruple. And therefore Mr. *Linus's* friends need not fear, but that the further directions I ſent them lately for trying the experiment, are the ſame with thoſe I have followed from the beginning; nor trouble themſelves about any thing but to try the experiment right. But yet, becauſe Mr. *Gaſcoin* has been pleaſed to inſinuate his ſuſpicion, that I do differ from himſelf in thoſe directions; I ſhall not ſcruple here to reduce them into particulars, and ſhew where each particular is to be found.

Firſt, then, he is to get a priſm with an angle about 60 or 65 degrees (Num. LXXX. p. 3077 ([a]), and p. 3086 ([b]). If the angle be about 63 degrees, as that was which I made uſe of (Num. LXXX. p. 3077 ([a]),) he will find all things ſucceed exactly as I deſcribed them there. But if it be bigger or leſs, as 30, 40, 50, or 70 degrees, the refraction will be accordingly bigger or leſs; and conſequently the image longer or ſhorter. If his priſm be pretty nearly equilateral, ſuch as I ſuppoſe are uſually ſold in other places as well as in *England*, he may make uſe of the biggeſt angle. But he muſt be ſure to place the priſm ſo, that the refraction be made by the two planes which comprehend this angle. I could almoſt ſuſpect, by conſidering ſome circumſtances in Mr. *Linus's* letter, that his error was in this point; he expecting the image ſhould become as long by a little refraction, as by a great one; which yet being too groſs an error to be ſuſpected of any optician, I ſay nothing of it; but only hint this to Mr. *Gaſcoin*, that he may examine all things.

Secondly, having ſuch a priſm, he muſt place it ſo, that the axis be perpendicular to the rays (Num. LXXXIV. p. 4091, lin. 18, 19 ([c]).) A little error in this point makes no ſenſible variation of the effect.

Thirdly, the priſm muſt be ſo placed, that the refractions on both ſides be equal (Num. LXXX. p. 3077 ([d]),) which ſhew it was

([a]) See p. 296, lin. 33. ([b]) See p. 306, lin. 3. ([c]) See p. 308, lin. 20—22.
([d]) See p. 296, lin. 33—35.

to

to be readily done, by turning it about its axis, and ſtaying it when you let the image reſt between two contrary motions, as I explained in my late deſcriptions, ſo I hinted before (Num. LXXX. p. 3077, lin. 34, 35, 36 (a).) If there ſhould be an error in this point alſo, it can do no hurt.

Fourthly, the diameter of the hole I put $\frac{1}{4}$ of an inch (Num. LXXX. p. 3077 (b),) and placed the priſm cloſe to it, even ſo cloſe as to be contiguous (Num. LXXX. p. 3077, lin. 4, 5(c).) But yet there needs no curioſity in theſe circumſtances. The hole may be of any other bigneſs, and the priſm at a diſtance from the hole; provided things be ſo ordered, that the light appear of a round form, if intercepted perpendicularly at its coming out of the priſm. Nor needs there any curioſity in the day. The clearer it is, the better; but if it be a little cloudy, that cannot much prejudice the experiment, ſo the ſun do but ſhine diſtinctly through the cloud.

Theſe things being thus ordered, if the refracted light fall perpendicularly on a wall or paper, at 20 feet or more from the priſm; it will appear in an oblong form, croſs to the axis of the priſm, Red at one end, and Violet at the other; the length five times the breadth, more or leſs, according to the quantity of the refraction; the ſides, ſtreight lines parallel to one another; and the ends confuſed, but yet ſeeming ſemicircular.

I hope, therefore, Mr. *Linus*'s friends will not entertain themſelves any further about incongruous ſurmiſes, but try the experiment, as Mr. *Gaſcoin* has promiſed. And then, ſince Mr. *Gaſcoin* tells you, that "the experiment being of itſelf extraordinary and ſurprizing, and beſides uſhering in new principles into optics, quite contrary to the common and received; it will be hard to perſuade it at a truth, till it made ſo viſible to all, as it were a ſhame to deny it;" if he eſteem it ſo extraordinary, he may have the privilege of making it ſo viſible to all, that it will be a ſhame to deny it. For I dare ſay, after his teſtimony, nobody elſe will ſcruple it. And I make no queſtion but he will hit of it, it being ſo plain and eaſy, that I am very much at a loſs to imagine what way Mr. *Linus* took to miſs.

(a) See p. 297, lin. 20—23. (b) See p. 296, lin. 29. (c) See p. 296, lin. 28.

XII.

To Mr. OLDENBURG.

SIR,

THE things oppoſed by Mr. *Line* being upon trials found true and granted me, I begin with the new queſtion about the proportion of the length of the image to its breadth. I call it a new one. For though Mr. *Line* in his laſt letter ſpake againſt ſo great a length, as I aſſign; yet, as it ſeems to me, it was not to grant any tranſverſe length ſhorter than that aſſigned by me (for in his firſt letter he abſolutely denied that there would be any ſuch length) but to lay the greater emphaſis upon his diſcourſe, whilſt in defence of common optics, he was diſputing in general againſt a tranſverſe image. And therefore, in my anſwer, I did not preſcribe the juſt quantity of the refracting angle, with which I would have the experiment repeated: which would have been a neceſſary circumſtance, had the diſpute been about the juſt proportion of the length to the breadth. Yet I added * this note, that the bigger the angle of the priſm is, the greater will be the length in proportion to the breadth (a); not imagining, but that when he had found in any priſm the length of the image tranſverſe to the axis, he would eaſily thence conclude, that a priſm with a greater angle would make the image longer; and conſequently, that by uſing an angle great enough, he might bring it to equal, or exceed the length aſſigned by me; as indeed he might: for, by taking an angle of 70 or 75 degrees, or a little greater, he might have made the length not only five, but ſix or eight times the breadth, or more. No wonder, therefore, that Mr. *Lucas* found the image ſhorter than I did, ſeeing he tried the experiment with a leſs angle.

The angle, indeed, which I uſed was but about 63 deg. 12 min. and his is ſet to 60 degrees; the difference of which from mine being but 3 deg. 12 min. is too little to reconcile us; but yet it will bring us conſiderably nearer together. And if this angle was not exactly meaſured, but the round number of 60 degrees ſet down by gueſs, or by a leſs accurate meaſure (as I ſuſ-

* In my firſt letter in Phil. Tranſ. Num. CXXI. p. 500.

(a) See p. 353, lin. 30, 31

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pect by the conjectural meaſure of the refraction of his priſm, by the ratio of the ſines as 2 to 3, ſet down at the ſame time, inſtead of an experimental one), then might it be 2 or 3 degrees leſs than 60 deg. if not ſtill leſs: and all this, if it ſhould be ſo, would take away the greateſt part of the difference between us.

But however it be, I am well aſſured my own obſervation was exact enough. For I have repeated it divers times ſince the receipt of Mr. *Lucas's* letter, and that without any conſiderable difference of my obſervations, either from one another, or from what I wrote before: and that it might appear experimentally, how the increaſe of the angle increaſes the length of the image, and alſo that nobody, who has a mind to try the experiment exactly, might be troubled to procure a priſm which has an angle juſt of the bigneſs aſſigned by me, I tried the experiment with divers angles, and have ſet down my trials in the following Table; where the firſt column expreſſes the angles of two priſms which I uſed, which are meaſured as exactly as I could, by applying them to the angle of a ſector; and the ſecond column expreſſes, in inches, the length of the image made by each of thoſe angles; its breadth being two inches, its diſtance from the priſm 18 feet and 4 inches, and the breadth of the hole in the window-ſhut $\frac{1}{4}$ of an inch.

	Angles.		Lengths.
The firſt priſm	56°	10′	$7\frac{1}{4}$
	60	24	$9\frac{1}{2}$
	63	26	$10\frac{1}{3}$

	Angles.		Lengths.
The ſecond priſm	54°	0′	$7\frac{3}{5}$
	62	12	$10\frac{1}{3}$
	63	48	$10\frac{3}{4}$

You may perceive, that the length of the images, in reſpect of the angles that made them, are ſomething greater in the ſecond priſm than in the firſt; but that was becauſe the glaſs, of which the ſecond priſm was made, had the greater refractive power.

The days in which I made theſe trials were pretty clear, but not ſo clear as I deſired; and therefore, afterwards meeting with a day as clear as I deſired, I repeated the experiment with the ſecond priſm, and found the lengths of the image made by its ſeveral angles, to be about $\frac{1}{4}$ of an inch greater than before; the meaſures being thoſe ſet down in the Table.

The

	Angles.		Lengths.
The ſecond priſm	54°	0′	$7\frac{1}{3}$
	62	12	10
	63	48	11

The reaſon of this difference, I apprehend, was, that in the cleareſt days the light of the white ſkies, which dilutes and renders inviſible the fainteſt colours at the ends of the image, is a little diminiſhed in a clear day, and ſo gives leave to the colours to appear to a great length; the ſun's light at the ſame time becoming briſker, and ſo ſtrengthening the colours, and making the faint ones at the two ends more conſpicuous: for I have obſerved, that in days ſomething cloudy, whilſt the priſm has ſtood unmoved at the window, the image would grow a little longer or a little ſhorter, accordingly as the ſun was more or leſs obſcured by thin clouds, which paſſed over it; the image being ſhorteſt while the cloud was brighteſt, and the ſun's light fainteſt. Whence it is eaſy to apprehend, that if the light of the clouds could be quite taken away, ſo that the ſun might appear ſurrounded with darkneſs, or if the ſun's light were much ſtronger than it is, the colours would ſtill appear to a greater length.

In all theſe obſervations the breadth of the image was juſt two inches. But obſerving that the ſides of the two priſms I uſed were not exactly plain, but a little convex (the convexity being about ſo much as that of a double convex-glaſs of a ſixteen or eighteen-foot teleſcope) I took a third priſm, whoſe ſides were as much concave as thoſe of the other were convex: and this made the breadth of the image to be two inches and a third part of an inch; the angles of this priſm, and the lengths of the image made by each of thoſe angles, being thoſe expreſſed in this Table.

Angles.	Lengths.
58°	$8\frac{1}{2}$
$59\frac{1}{2}$	9
$62\frac{1}{2}$	$10\frac{1}{3}$

In this caſe you ſee the concave figure of the ſides of the priſm by making the rays diverge a little, cauſes the breadth of

the image to be greater in proportion to its length than it would be otherwiſe. And this I thought fit to give you notice of, that Mr. *Lucas* may examine, whether his priſm hath not this fault. If a priſm may be had with ſides exactly plain, it may do well to try the experiment with that; but it is better if the ſides be about ſo much convex as thoſe of mine are, becauſe the image will thereby become much better defined: for this convexity of the ſides does the ſame effect, as if you ſhould uſe a priſm with ſides exactly plain, and between it and the hole in the window-ſhut, place an object-glaſs of an 18 foot teleſcope, to make the round image of the ſun appear diſtinctly defined on the wall, when the priſm is taken away, and conſequently the long image made by the priſm to be much more diſtinctly defined, eſpecially at its ſtreight ſides, than it would be otherwiſe.

One thing more I ſhall add: That the utmoſt length of the image, from the fainteſt Red at one end, to the fainteſt Blue at the other, muſt be meaſured. For in my firſt letter about colours, where I ſet down the length to be five times the breadth, I called that length the utmoſt length of the image; and I meaſured the utmoſt length, becauſe I account all that length to be cauſed by the immediate light of the ſun; ſeeing the colours, as I noted above, become viſible to the greateſt length in the cleareſt days, that is, when the light of the ſun tranſcends moſt the light of the clouds. Sometimes there will happen to ſhoot out from both ends of the image a glaring light a good way beyond theſe colours; but this is not to be regarded, as not appertaining to the image. If the meaſures be taken right, the whole length will exceed the length of the ſtreight ſides by about the breadth of the image.

By theſe things ſet down thus circumſtantially, I preſume Mr. *Lucas* will be enabled to accord his trials of the experiment with mine; ſo nearly at leaſt, that there ſhall not remain any very conſiderable difference between us. For if ſome little difference ſhould ſtill remain, that need not trouble us any further, ſeeing there may be many various circumſtances which may conduce to it; ſuch as are not only the different figures of priſms, but alſo the different refractive power of glaſſes, the different diameters

of the ſun at divers times of the year, and the little errors that may happen in meaſuring lines and angles, or in placing the priſm at the window; though, for my part, I took care to do theſe things as exactly as I could. However, Mr. *Lucas* may make ſure to find the image as long or longer than I have ſet down, if he take a priſm whoſe ſides are not hollow ground, but plain, or (which is better) a very little convex, and whoſe refracting angle is as much greater than that I uſed, as that he hath hitherto tried it with is leſs; that is, whoſe angle is about 66 or 67 degrees, or, if he will, a little greater.

Concerning Mr. *Lucas's* other experiments, I am much obliged to him that he would take theſe things ſo far into conſideration, and be at ſo much pains for examining them; and I thank him ſo much the more, becauſe he is the firſt that hath ſent me an experimental examination of them. But yet it will conduce to his more ſpeedy and full ſatisfaction, if he a little change the method which he has propounded, and, inſtead of a multitude of things, try only the *Experimentum Crucis*: for it is not number of experiments, but weight to be regarded; and where one will do, what need many? Had I thought more requiſite, I could have added more. For before I wrote my firſt letter to you about colours; I had then taken much pains in trying experiments about them, and written a tractate on that ſubject; wherein I had ſet down at large the principal of the experiments I had tried: amongſt which there happened to be the principal of thoſe experiments which Mr. *Lucas* has now ſent me. And as for the experiments ſet down in my firſt letter to you, they were only ſuch as I thought convenient to ſelect out of that tractate.

But ſuppoſe thoſe had been my whole ſtore; yet Mr. *Lucas* ſhould not have grounded his diſcourſe upon a ſuppoſition of any want of experiment, till he had examined thoſe few. For if any of thoſe be demonſtrative, they will need no aſſiſtants, nor leave room for further diſputing about what they demonſtrate.

The main thing he goes about to examine is, the different refrangibility of Light; and this I demonſtrated by the *Experimentum Crucis*. Now if this demonſtration be good, there needs no further

ther examination of the thing; if not good, the fault of it is to be ſhewn: for the only way to examine a demonſtrated propoſition is to examine the demonſtration. Let that experiment therefore be determined in the firſt place, and that which it proves be acknowledged; and then, if Mr. *Lucas* wants my aſſiſtance to unfold the difficulties which he fancies to be in the experiments he has propounded, he ſhall freely have it. At preſent I ſhall ſay nothing in anſwer to his experimental diſcourſe, but this in general; That it has proceeded partly from ſome miſunderſtanding of what he writes againſt, and partly for want of due caution in trying experiments; and that amongſt his experiments, there is one, which, when duly tried, is, next to the *Experimentum Crucis*, the moſt conſpicuous experiment, I know, for proving the different refrangibility of light, which he brings it to prove againſt.

By the poſtſcript of Mr. *Lucas's* letter, one not acquainted with what has paſſed, might think that he quotes the obſervation of the Royal Society againſt me; whereas the relation of their obſervation, which you ſent to *Liege*, contained nothing at all about the juſt proportion of the length of the image to its breadth according to the angle of the priſm, nor any thing more (ſo far as I can perceive by your laſt) than what was pertinent to the things then in diſpute, *viz.* that they found them ſucceed as I had affirmed. And therefore, ſince Mr. *Lucas* has found the ſame ſucceſs, I ſuppoſe, that when he expreſſeth, That *he much rejoiced to ſee the trials of the* R. Society *agree ſo exactly with his*, he meant only ſo far as his agreed with mine.

And becauſe I am again upon this firſt experiment, I ſhall deſire that Mr. *Lucas* will repeat it with all the exactneſs and caution that may be, regard being had to the information about it, ſet down in this letter. And then I deſire to have the length and breadth of the image, with its diſtance from the priſm, ſet down exactly in feet and inches, and parts of an inch; that I may have an opportunity to conſider what relation its length and breadth have to the ſun's diameter. For I know that Mr. *Lucas'* obſervation cannot hold, where the refracting angle of the priſm is full 60 degrees, and the day is clear, and the full length of the

colours is meaſured, and the breadth of the image anſwers to the ſun's diameter. And ſeeing I am well aſſured of the truth and exactneſs of my own obſervations, I ſhall be unwilling to be diverted by any other experiments, to have a fair end made of this in the firſt place.

P. S. I had like to have forgotten to adviſe, that the *Experimentum Crucis*, and ſuch others as ſhall be made for knowing the nature of colours, be made with priſms that refract ſo much, as to make the length of the image five times its breadth, and rather more than leſs; for otherwiſe, experiments will not ſucceed ſo plainly with others, as they have done with me.

LETTERS

RELATING TO THE

EXCITATION OF ELECTRICITY IN GLASS.

I.

EXTRACT FROM THE MINUTES

OF THE

ROYAL SOCIETY.

Dec. 9, 1675.

THE company taking particular notice, among other things, of an experiment mentioned in this hypotheſis, deſired it might be tried, *viz.* That having laid upon a table a round piece of glaſs, about two inches broad, in a braſs ring, ſo that the glaſs might be about one-third of an inch from the table, and the air between them incloſed on all ſides after the manner as if he had whelved a little ſieve upon the table: and then rubbing the glaſs briſkly, till ſome little fragments of paper, laid on the table under the glaſs, began to be attracted, and move nimbly to and fro; after he had done rubbing the glaſs, the papers would continue a pretty while in various motions; ſometimes leaping up to the glaſs, and reſting there a-while; then leaping down and reſting there, and then leaping up and down again: and this ſometimes in lines ſeeming perpendicular to the table, ſometimes in obliique ones; ſometimes alſo leaping up in one arch, and down in another, divers times together, without ſenſible reſting between; ſometimes ſkip in a bow from one part of the glaſs to another, without touching the table; and ſometimes hang by a corner, and turn often about very nimbly, as if they had been carried about in the midſt of a whirldwind; and be otherwiſe variouſly moved, every paper with a divers motion. And upon ſliding his finger on the upper ſide of the glaſs, though neither the glaſs nor incloſed air below were moved thereby, yet would the papers, as they hung under the glaſs, receive ſome new motion,

motion, inclining this or that way, according as he moved his finger.

The experiment he propoſes to be varied with a larger glaſs placed further from the table, and to make uſe of bits of leaf-gold inſtead of papers, eſteeming that this will ſucceed much better, ſo as perhaps to make the gold riſe and fall in ſpiral lines, or whirl for a time in the air, without touching the table of glaſs.

Ordered, That this experiment be tried the next meeting.

II.

To Mr. OLDENBURG.

SIR, Cambridge, Dec. 14, 1675

THE notice you gave me of the Royal Society's intending to ſee the experiment of glaſs rubbed, to cauſe various motions in bits of paper underneath, put me upon recollecting myſelf a little further about it. And then remembering, that if one edge of the braſs hoop was laid downward, the glaſs was as near again to the table, as it was when the other edge was laid downward; and that the papers played beſt when the glaſs was neareſt to the table; I began to ſuſpect, that I had ſet down a greater diſtance of the glaſs from the table, than I ſhould have done. For in ſetting down that experiment, I truſted to the idea I had of the bigneſs of the hoop; in which I might eaſily be miſtaken, having not ſeen it of a long time. And this ſuſpicion was increaſed by trying the experiment with an object-glaſs of a teleſcope placed about the third part of an inch from the table. For I could not ſee the papers play any thing near ſo well, as I had ſeen them formerly. Whereupon I looked for the old hoop with its glaſs; and at length found the hoop, the glaſs being gone. But by the hoop I perceived, that when one edge was turned down, the glaſs was almoſt the third part of an inch from the table: and when the other edge was down, which made the papers play ſo well, the glaſs was ſcarce the eighth part of an inch from the table. This I thought fit to ſignify to you, that if the experiment ſucceed not well at the diſtance I ſet down; it may be tried

at a leſs diſtance, and that you may alter my paper, and write in it an eighth part of an inch, inſtead of $\frac{1}{2}$ or $\frac{1}{3}$ of an inch. The bits of paper ought to be very little, and of thin paper; perhaps little bits of the wings of a fly, or other light ſubſtances, may do better than paper. Some of the motions, as that of hanging by a corner, and twirling about, and that of leaping from one part of the glaſs to another without touching the table, happen but ſeldom; but it made me take the more notice of them.

Pray preſent my humble ſervice to Mr. *Boyle* when you ſee him, and thanks for the favour of the converſe I had with him at Spring. My conceit at trepanning the common æther, as he was pleaſed to expreſs it, makes me begin to have the better thoughts on, that he was pleaſed to entertain it with a ſmile. I am apt to think, that when he has a ſett of experiments to try in his air-pump, he will make that one to ſee how the compreſſion or relaxation of a muſcle will ſhrink or ſwell, ſoften or harden, lengthen or ſhorten it.

As for regiſtering the two diſcourſes, you may do it; only I deſire you would ſuſpend till my next letter, in which I intend to ſet down ſomething to be altered, and ſomething to be added in the hypotheſis, being in the mean while, Sir, &c.

III.

Extract from the Minutes of the ROYAL SOCIETY.

Dec. 16, 1675.

MR. NEWTON'S experiment of glaſs rubbed to cauſe various motions in bits of paper underneath was tried, but ſucceeded not in theſe circumſtances with which it was tried. This trial was made upon the reading of a letter of his dated the 14th of Dec. 1675, at *Cambridge*, wherein he gives ſome particular directions about the experiment.

Ordered, That the Secretary ſhould write again to the ſaid Mr. *Newton*, and acquaint him with the want of ſucceſs of his experiment; and deſire him, that he would ſend his own apparatus wherewith he had made it: as alſo to enquire whether he had

ſecured the papers, being moved by the air, that might ſomewhere ſteal in.

IV.

To Mr. OLDENBURG.

SIR, Dec. 21, 1675.

UPON your letter, I took another glaſs, 4 inches broad and $\frac{1}{4}$ of an inch thick, of ſuch glaſs as teleſcopes are made of; and placed it a $\frac{1}{6}$ part of an inch from the table. It was ſet in ſuch a piece of wood, as the object-glaſſes of teleſcopes uſe to be ſet: and the experiment ſucceeded well. After the rubbing was ſtill, and all was ſtill, the motion of the papers would continue ſometimes while I counted a hundred; every paper leaping up about twenty times more or leſs, and down as often. I tried it alſo with two other glaſs that belong to a teleſcope, and it ſucceeded with both. And I make no queſtion but any glaſs will do, that ſhall be excited to electric virtue; as I think any may. If you have a mind to try any of theſe glaſſes, you may have them. But I ſuppoſe, if you cannot make it do in other glaſs, you will fail in any I can ſend you. I am apt to ſuſpect the failure was in the manner of rubbing: for I have obſerved, that the rubbing variouſly, or with various things, alters the caſe. At one time I rubbed the aforeſaid great glaſs with a napkin, twice as much as I uſed to do with my gown, and nothing would ſtir; and yet preſently rubbing it with ſomething elſe, the motions ſoon began. After the glaſs has been much rubbed too, the motions are not ſo laſting; and the next day I found the motions fainter and difficulter to excite than the firſt. If the Society have a mind to attempt it any more, I can give no better advice than this: To take a new glaſs not yet rubbed (perhaps one of the old ones may do well enough after it has lain ſtill a while) and let this be rubbed, not with linen nor ſoft nappy woollen, but with ſtuff whoſe threads may rake the ſurface of the glaſs, ſuppoſe tamerine or the like doubled up in the hand; and this with a briſk motion as may be, till 100 or 150 may be counted; the glaſs lying

lying all the while over the papers. Then if nothing ſtir, rub the glaſs with the finger-ends half a ſcore times to and fro, or knock your finger-ends as often upon the glaſs: for this rubbing or knocking with your finger-ends, after the former rubbing, conduces moſt to excite the papers. If nothing ſtir yet, rub again with the cloth till 60 or 80 may be counted; and then rub or knock again with your fingers, and repeat this till the electric virtue of the glaſs be ſo far excited as to take up the papers; and then a very little rubbing or knocking now and then will revive the motions. In doing all this, let the rubbing be always done as nimbly as may be; and if the motion be circular like that of glaſs-grinders, it may be better. But if you cannot make it yet ſucceed, it muſt be let alone till I have ſome opportunity of trying it before you. As for the ſuſpicion of the paper being moved by the air, I am ſecure from that: yet in the other of drawing the leaf-gold to above a foot diſtance, which I never went about to try myſelf till the laſt week, I ſuſpect the air might raiſe the gold, and then a ſmall attraction might determine it towards the glaſs: for I could not make it ſucceed.

As for Mr. *Hook's* inſinuation, that the ſum of the hypotheſis I ſent you, had been delivered by him in his Micrography, I need not be much concerned at the liberty he takes in that kind. Yet becauſe you think it may be well, if I ſtate the difference I take to be between them, I ſhall do it as briefly as I can; and that the rather, that I may avoid the ſavour of having done any thing unjuſtifiable or unhandſome towards Mr. *Hook*. But for this end, I muſt firſt, to ſee what is his, caſt out what he has borrowed from *Des Cartes*, or others, *viz.* That there is an æthereal medium: That light is the action of this medium: That this medium is leſs implicated in the parts of ſolid bodies, and ſo moves more freely in them, and tranſmits light more readily through them; and that after ſuch a manner as to accelerate the rays in a certain proportion: That refraction ariſes from this acceleration, and has ſines proportional: That light is at firſt uniform: That colours are ſome diſturbance or modification of its rays by refraction, or reflexion: That the colours of a priſm are made by means of the quieſcent medium accelerating ſome motion of

the rays on one ſide, where Red appears, and retarding it on the other ſide, where Blue appears: and that there are but theſe two original colours or colour making modifications of light, which by their various degrees, or, as Mr. *Hook* calls it, dilutings, produce all intermediate ones. This rejected, the remainder of his hypotheſis is, that he has changed *Des Cartes* preſſing or progreſſive motion of the medium to a vibrating one; the rotation of the *globuli* to the obliquation of the pulſes; and the accelerating their rotation on the one hand, and retarding it on the other by the quieſcent medium, to produce colours, to the like action of the medium on the two ends of his pulſe for the ſame end. And having thus far modified his by the Carteſian hypotheſis, he has extended it farther to explicate the phænomena of thin plates; and added another explication of the colours of natural bodies fluid and ſolid.

This, I think, is in ſhort the ſum of his hypotheſis. And in all this, I have nothing common with him, but the ſuppoſition that æther is a medium ſuſceptible of vibrations. Of which ſuppoſition I make a very different uſe: he ſuppoſing it light itſelf; which I ſuppoſe it is not. This is as great a difference, as is between him and *Des Cartes*. But beſides this, the manner of refraction and reflexion, and the nature and production of colours in all caſes, which take up the body of my diſcourſe, I explain very differently from him; and even in the colours of thin tranſparent ſubſtances, I explain every thing after a way ſo differing from him, that the experiments, I ground my diſcourſe on, deſtroy all he has ſaid about them. And the two main experiments, without which the manner of the production of thoſe colours is not to be found out, were not only unknown to him when he wrote his Micrography, but even laſt Spring; as I underſtood in mentioning them to him. This therefore is the ſum of what is common to us; That æther may vibrate. And ſo if he thinks fit to uſe that notion of colours ariſing from the various bigneſs of pulſes, without which his hypotheſis will do nothing, his will borrow as much from my anſwer to his objections, as that I ſend you does from his Micrography.

But it may be, he means, that I have made uſe of his obſervations. And of ſome I did: as that of the inflexion of rays, for which I quoted him: that of opacity ariſing from the interſtices of the parts of bodies, which I inſiſt not on: and that of plated bodies exhibiting colours; a phænomenon, for the notice of which I thank him. But he left me to find out and make ſuch experiments about it, as might inform me of the manner of the production of thoſe colours to ground an hypotheſis on: he having given no farther inſight into it than this, that the colour depended on ſome certain thickneſs of the plate. Though what that thickneſs was at every colour, he confeſſes in his Micrography he had attempted in vain to learn. And therefore ſeeing I was left to meaſure it myſelf, I ſuppoſe he will allow me to make uſe of what I took the pains to find out. And this I hope may vindicate me from what Mr. *Hook* has been pleaſed to charge me with.

Sir, I doubt I have already troubled you with too large a letter, and ſo break off abruptly, yours, &c.

V.

Extract from the Minutes of the ROYAL SOCIETY.

Dec. 30, 1675.

—READ a leter of Mr. *Newton's*, dated *Dec.* 21, 1675, in which he aſſures, that the experiment had been repeated by him again with good ſucceſs, and gives yet further directions for the making of it ſucceſsfully.

Ordered, That theſe directions ſhould be obſerved at the next trial to be made at the next meeting of the Society.

VI.

Jan. 13, $16\frac{75}{76}$.

MR. *NEWTON'S* experiment of glaſs rubbed to cauſe various motions in bits of paper underneath, being made according to his more particular directions, did ſucceed very well. The rubbing

rubbing was made both with a ſcrubbing-bruſh made of ſhort hog's-briſtles, with a knife, the haft of a knife made with whale-bone, and with the nail of one's finger. It appeared, that touching many parts at once with a hard and rough body, did produce the effect expected.

A LET-

A

LETTER

TO THE

HON. MR. BOYLE

ON THE

CAUSE OF GRAVITATION.

Copy of a Letter from SIR I. NEWTON TO THE HON. MR. BOYLE.

HON. SIR, Feb. 28, 167⅘.

I HAVE ſo long deferred to ſend you my thoughts about the phyſical qualities we ſpoke of, that, did I not eſteem myſelf obliged by promiſe, I think I ſhould be aſhamed to ſend them at all. The truth is, my notions about things of this kind are ſo indigeſted, that I am not well ſatisfied myſelf in them; and what I am not ſatisfied in, I can ſcarce eſteem fit to be communicated to others, eſpecially in natural philoſophy, where there is no end of fanſying. But becauſe I am indebted to you, and yeſterday met with a friend, Mr. *Maulyverer*, who told me, he was going to *London*, and intended to give you the trouble of a viſit, I could not forbear to take the opportunity of conveying this to you by him.

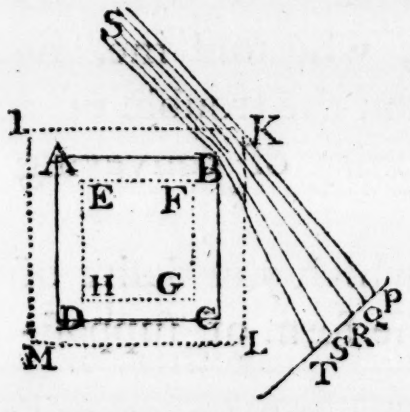
It being only an explication of qualities, which you deſire of me; I ſhall ſet down my apprehenſions in the form of ſuppoſitions, as follow.

And, firſt, I ſuppoſe that there is diffuſed through all places an æthereal ſubſtance, capable of contraction or dilatation, ſtrongly elaſtic; and, in a word, much like air in all reſpects, but far more ſubtile.

2. I ſuppoſe this Æther pervades all groſs bodies, but yet ſo as to ſtand rarer in their pores than in free ſpaces; and ſo much

the rarer, as their pores are leſs. And this I ſuppoſe (with others) to be the cauſe, why light, incident on thoſe bodies, is refracted towards the perpendicular; why two well-poliſhed metals cohere in a receiver exhauſted of air: why quick-ſilver ſtands ſometimes up to the top of a glaſs pipe, though much higher than 30 inches; and one of the main cauſes, why the parts of all bodies cohere; alſo the cauſe of philtration, and of the riſing of water in ſmall glaſs pipes, above the ſurface of the ſtagnating water they are dipped into: for I ſuſpect the æther may ſtand rarer not only in the inſenſible pores of bodies, but even in the very ſenſible cavities of thoſe pipes. And the ſame principle may cauſe menſtruums to pervade with violence the pores of the bodies they diſſolve; the ſurrounding æther as well as the atmoſphere preſſing them together.

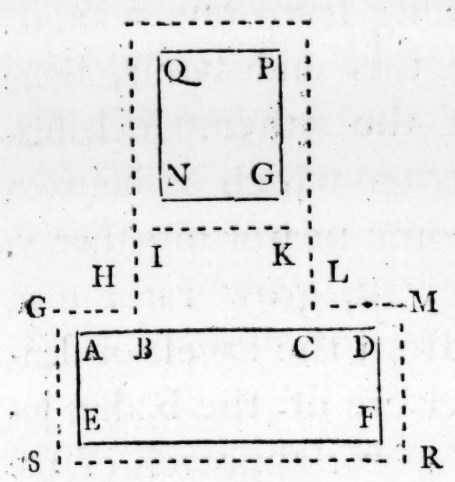
3. I ſuppoſe the rarer Æther within bodies, and the denſer without them, not to be terminated in a mathematical ſuperficies, but to grow gradually into one another: the external æther beginning to grow rarer, and the internal to grow denſer, at ſome little diſtance from the ſuperficies of the body, and running through all intermediate degrees of denſity in the intermediate ſpaces. And this may be the cauſe why light, in *Grimaldi's* experiment, paſſing by the edge of a knife, or other opake body, is turned aſide, and as it were refracted; and by that refraction makes ſeveral colours. Let ABCD be a denſe body, whether opake or tranſparent; EFGH, the outſide of the uniform æther, which is within it; IKLM, the inſide of the uniform æther, which is without it; and conceive the æther, which is between EFGH and IKLM, to run through all intermediate degrees of denſity, between that of the two uniform æthers on either ſide. This being ſuppoſed, the rays of the ſun, SB, SK, which paſs by the edge of this body, between B and K, ought, in their paſſage through the unequally denſe æther there, to receive a ply from the denſer æther, which is on that ſide towards K; and that the more, by how much they paſs nearer to the body; and thereby to be ſcattered through the



ſpace

ſpace PQRST, as by experience they are found to be. Now the ſpace between the limits EFGH and IKLM, I ſhall call the ſpace of the æther's *graduated rarity*.

4. When two bodies, moving towards one another, come near together, I ſuppoſe the æther between them to grow rarer than before; and the ſpaces of its graduated rarity to extend further from the ſuperficies of the bodies towards one another; and this by reaſon of the æther cannot move and play up and down ſo freely, in the ſtrait paſſage between the bodies, as it would before they came ſo near together.



Thus, if the ſpace of the æther's graduated rarity reach from the body ABCDEF, only to the diſtance GHLMRS, when no other body is near it; yet may it reach farther, as to IK, when another body, NGPQ, approaches: and as the other body approaches more and more, I ſuppoſe the æther between them will grow rarer and rarer.

Theſe ſuppoſitions I have ſo deſcribed, as, if I thought the ſpaces of gradual æther had preciſe limits, as is expreſſed at IKLM, in the firſt figure, and GMRS, in the ſecond: for thus I thought I could better expreſs myſelf. But really, I do not think they have ſuch preciſe limits, but rather decay inſenſibly; and, in ſo decaying, extend to a much greater diſtance, than can eaſily be believed, or need be ſuppoſed.

5. Now from the fourth ſuppoſition, it follows, that when two bodies, approaching one another, come ſo near together as to make the æther between them begin to rarify; they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another: which reluctance and endeavour will increaſe as they come nearer together, becauſe thereby they cauſe the interjacent æther to rarify more and more: but at length, when they come ſo near together, that the exceſs of preſſure of the external æther, which ſurrounds the bodies, above that of the rarified æther, which is between them, is ſo great, as to overcome the reluctance which the bodies have from

being brought together; then will that exceſs of preſſure drive them with violence together, and make them adhere ſtrongly to one another, as was ſaid in the ſecond ſuppoſition. For inſtance, in the ſecond figure, when the bodies ED and NP are ſo near together, that the ſpaces of the æther's graduated rarity begin to reach to one another, and meet in the line IK; the other between them will have ſuffered much rarification, which rarification requires much force, that is, much preſſing of the bodies together: and the endeavour, which the æther between them has to return to its former natural ſtate of condenſation, will cauſe the bodies to have an endeavour of receding from one another. But, on the other hand, to counterpoiſe this endeavour, there will not yet be any exceſs of denſity of the æther which ſurrounds the bodies, above that of the æther which is between them at the line IK. But if the bodies come nearer together, ſo as to make the æther in the mid-way-line, IK, grow rarer than the ſurrounding æther; there will ariſe, from the exceſs of denſity of the ſurrounding æther, a compreſſure of the bodies towards one another; which, when by the nearer approach of the bodies, it becomes ſo great as to overcome the aforeſaid endeavour the bodies have to recede from one other, they will then go towards one another, and adhere together. And, on the contrary, if any power force them aſunder, to that diſtance where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now, hence I conceive it is, chiefly, that a fly walks on water without wetting her feet, and conſequently without touching the water; that two poliſhed pieces of glaſs are not without preſſure brought to contact, no, not though the one be plain, the other a little convex; that the particles of duſt cannot by preſſing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging ſubſtances, and ſalts diſſolved in water, do not of their own accord concrete and fall to the bottom, but diffuſe themſelves all over the liquor, and expand ſtill more, if you add more liquor to them. Alſo, that the particles of vapours, exhalations, and air, do ſtand at a diſtance from one another, and endeavour to recede from one another, as the preſſure of the incumbent

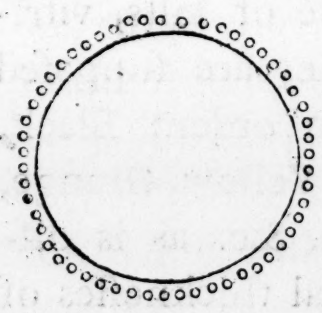
atmoſphere

atmoſphere will let them: for I conceive the confuſed maſs of vapours, air, and exhalations, which we call the atmoſphere, to be nothing elſe but the particles of all ſorts of bodies, of which the earth conſiſts, ſeparated from one another, and kept at a diſtance by the ſaid principle.

From theſe principles, the actions of menſtruums upon bodies may be thus explained. Suppoſe any tinging body, as cochineal or logwood, be put into water, ſo ſoon as the water ſinks into its pores, and wets on all ſides, any particle, which adheres to the body, only by the principle in ſecond ſuppoſition; it takes off, or at leaſt much diminiſhes the efficacy of that principle, to hold the particle to the body, becauſe it makes the æther on all ſides the particle to be of a more uniform denſity than before. And then the particle being ſhaken off by any little motion, floats in the water, and with many ſuch others, makes a tincture; which tincture will be of ſome lively colour, if the particles be all of the ſame ſize and denſity; otherwiſe of a dirty one. For the colours of all natural bodies whatever, ſeem to depend on nothing but the various ſizes and denſities of their particles: as I think you have ſeen deſcribed by me more at large in another paper. If the particles be very ſmall (as are thoſe of ſalts, vitriols, and gums) they are tranſparent; and as they are ſuppoſed bigger and bigger, they put on the ſecolours in order; Black, White, Yellow, Red; Violet, Blue, pale Green, Yellow, Orange, Red; Purple, Blue, Green, Yellow, Orange, Red, &c. as is diſcerned by the colours, which appear at the ſeveral thickneſſes of very thin plates of tranſparent bodies. Whence, to know the cauſes of the changes of colours, which are often made by the mixtures of ſeveral liquors, it is to be conſidered, how the particles of any tincture may have their ſize or denſity altered by the infuſion of another liquor.

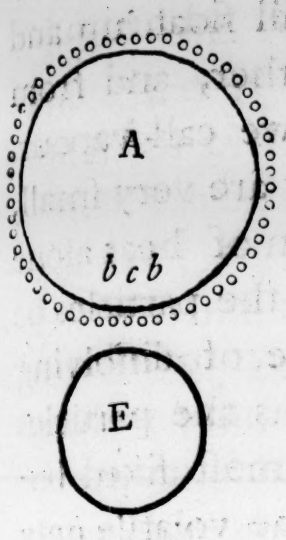
When any metal is put into common water, the water cannot enter into its pores to act on it and diſſolve it: not that water conſiſts of too groſs parts for this purpoſe, but becauſe it is unſociable to metal: for there is a certain ſecret principle in nature, by which liquors are ſociable to ſome things, and unſociable to others. Thus water will not mix with oil, but readily with ſpirit

rit of wine, or with ſalts; it ſinks alſo into wood, which quick-ſilver will not; but quick-ſilver ſinks into metals, which water will not; ſo *aqua fortis* diſſolves ſilver, not gold; *aqua regis* gold, and not ſilver, &c. But a liquor, which is of itſelf unſociable to a body, may, by the mixture of a convenient mediator, be made ſociable: ſo molten lead, which alone will not mix with copper, or with *regulus* of *mars*, by the addition of tin, is made to mix with either: and water, by the mediation of ſaline ſpirits, will mix with metal. Now when any metal is put in water, impregnated with ſuch ſpirits as into *aqua fortis*, *aqua regis*, *ſpirit* of *vitriol*, or the like; the particles of the ſpirits, as they in floating in the water ſtrike on the metal, will, by their ſociableneſs, enter into its pores, and gather round its outſide particles, and, by advantage of the continual tremor the particles of the metal are in, hitch themſelves in by degrees between thoſe particles and the body, and looſen them from it; and the water entering into the pores together with the ſaline ſpirits, the particles of the metal will be thereby ſtill more looſed, ſo as by that motion the ſolution puts them into to be eaſily ſhaken off, and made to float in the water: the ſaline particles ſtill encompaſſing the metallic ones, as a coat or ſhell does a kernell, after the manner expreſſed in the annexed figure; in which figure I have made the particles round, though they may be cubical, or of any other ſhape.



If into a ſolution of metal, thus made, be poured a liquor abounding with particles, to which the former ſaline particles are more ſociable than to the particles of the metal (ſuppoſe with particles of ſalt of tartar); then, ſo ſoon as they ſtrike on one another in the liquor, the ſaline will adhere to thoſe particles more firmly than to the metalline ones; and by degrees be wrought off from thoſe, to encloſe theſe. Suppoſe A, a metalline particle encloſed with ſaline ones of ſpirit of nitre; and E, a particle of ſalt of tartar contiguous to two of the particles of the ſpirit of nitre *b* and *c*: and ſuppoſe the particle E is impelled by any motion towards *d*, ſo as to roll about the particle *c*, till it touch the particle *d*; the particle *b* adhering more firmly to E than to A, will be forced off from

A.



A. And by the ſame means the particle E, as it rolls about it, will tear off the reſt of the ſaline particles from A, one after another, till it has got them all, or almoſt all, about itſelf. And when the metallic particles are thus diveſted of the nitrous ones, which, as a mediator between them and the water, held them floating in it; the alcalizate ones crouding for the room, the metallic ones took up before, will preſs theſe towards one another, and make them come more eaſily together: ſo that by the motion they continually have in the water, they ſhall be made to ſtrike on one another; and then, by means of the principle in the ſecond ſuppoſition, they will cohere, and grow into cluſters, and fall down by their weight to the bottom, which is called precipitation.

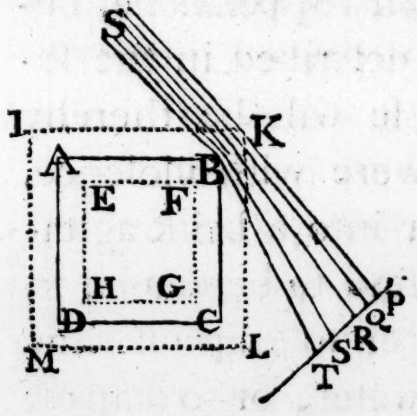
In the ſolution of metals, when a particle is looſing from the body, ſo ſoon as it gets to that diſtance from it, where the principle of receding, deſcribed in the 4th and 5th ſuppoſitions, begins to overcome the principle of acceding, deſcribed in the ſecond ſuppoſition; the receding of the particle will be thereby accelerated, ſo that the particle ſhall, as it were with violence, leap from the body, and, putting the liquor into a briſk agitation, beget and promote that heat, we often find to be cauſed in ſolutions of metal. And if any particle happen to leap off thus from the body, before it be ſurrounded with water, or to leap off with that ſmartneſs as to get looſe from the water, the water, by the principle, in the 4th and 5th ſuppoſitions, will be kept off from the particle, and ſtand round about it like a ſpherical hollow arch, not being able to come to a full contact with it any more. And ſeveral of theſe particles afterwards gathering into a cluſter, ſo as by the ſame principle to ſtand at a diſtance from one another without any water between them, will compoſe a bubble. Whence I ſuppoſe it is, that, in briſk ſolutions, there uſually happens an ebullition.

This is one way of tranſmuting groſs compact ſubſtances into aereal ones. Another way is by heat. For as faſt as the motion of heat can ſhake off the particles of water from the ſurface

of

of it; thoſe particles, by the ſaid principle, will float up and down in the air at a diſtance both from one another, and from the particles of air, and make that ſubſtance we call vapour. Thus I ſuppoſe it is, when the particles of a body are very ſmall, (as I ſuppoſe thoſe of water are) ſo that the action of heat alone may be ſufficient to ſhake them aſunder. But if the particles be much larger, they then require the greater force of diſſolving menſtruums to ſeparate them, unleſs by any means the particles can be firſt broken into ſmaller ones. For the moſt fixed bodies, even gold itſelf, ſome have ſaid, will become volatile only by breaking their parts ſmaller. Thus may the volability, and fixedneſs of bodies, depend on the different ſizes of their parts.

And on the ſame difference of ſize may depend the more or leſs permanency of aereal ſubſtances in their ſtate of rarification. To underſtand this, let us ſuppoſe ABCD to be a large piece of any metal; EFGH, the limit of the interior uniform æther; and B, a part of the metal at the ſuperficies AB. If this part, or particle B, be ſo little, that it reaches not to the limit EF; it is plain that the æther, at its center, muſt be leſs rare than if the particle were greater: for were it greater, its center would be further from the ſuperficies AB; that is, in a place where the æther, by ſuppoſition, is rarer. The leſs the particle B therefore, the denſer the æther at its center; becauſe its center comes nearer to the edge AB, where the æther is denſer than within the limit EFGH. And if the particle were divided from the body, and removed to a diſtance from it, where the æther is ſtill denſer, the æther within it muſt proportionally grow denſer. If you conſider this, you may apprehend how, by diminiſhing the particle, the rarity of the æther within it will be diminiſhed, till between the denſity of the æther without, and the denſity of the æther within it, there be little difference; that is, till the cauſe be almoſt taken away which ſhould keep this, and other ſuch particles, at a diſtance from one another. For that cauſe, explained in the 4th and 5th



ſuppoſitions, was the exceſs of denſity of the external æther above that of the internal. This may be the reaſon, then, why the ſmall particles of vapours eaſily come together, and are reduced back into water; unleſs the heat, which keeps them in agitation, be ſo great as to diſſipate them as faſt as they come together: but the groſſer particles of exhalations, raiſed by fermentation, keep their aereal form more obſtinately, becauſe the æther within them is rarer.

Nor does the ſize only, but the denſity of the particles alſo, conduce to the permanency of aereal ſubſtances. For the exceſs of denſity of the æther without ſuch particles, above that of the æther within them, is ſtill greater: which has made me ſometimes think, that the true permanent air may be of a metallic original: the particles of no ſubſtances being more denſe than thoſe of metals. This I think is alſo favoured by experience; for I remember I once read in the Philoſophical Tranſactions how Mr. *Hugens*, at *Paris*, found, that the air made by diſſolving ſalt of tartar would, in two or three days time, condenſe and fall down again; but the air made by diſſolving a metal continued without condenſing or relenting in the leaſt. If you conſider then how, by the continual fermentations made in the bowels of the earth, there are aereal ſubſtances raiſed out of all kinds of bodies; all which together make the atmoſphere, and that of all theſe, the metallic are the moſt permanent, you will not perhaps think it abſurd, that the moſt permanent part of the atmoſphere, which is the true air, ſhould be conſtituted of theſe: eſpecially ſince they are the heavieſt of all other, and ſo muſt ſubſide to the lower parts of the atmoſphere, and float upon the ſurface of the earth, and buoy the lighter exhalation and vapours, to float in greateſt plenty above them. Thus, I ſay, it ought to be with the metallic exhalations, raiſed in the bowels of the earth by the action of acid menſtruums; and thus it is with the true permanent air: for this, as in reaſon it ought to be eſteemed the moſt ponderous part of the atmoſphere, becauſe the loweſt; ſo it betrays its ponderoſity, by making vapours aſcend readily in it; by ſuſtaining miſts and clouds of ſnow; and by buoying up groſs and ponderous ſmoak. The air alſo is the moſt groſs, un-

active part of the atmoſphere, affording living things no nouriſhment, if deprived of the more tender exhalations and ſpirits that float in it; and what more unactive, or remote from nouriſhment, than metallic bodies.

I ſhall ſet down one conjecture more, which came into my mind now as I was writing this letter: it is about the cauſe of gravity. For this end, I will ſuppoſe æther to conſiſt of parts differing from one another in ſubtilty by indefinite degrees: that in the pores of bodies, there is leſs of the groſſer æther in proportion to the finer, than in open ſpaces; and conſequently, that in the great body of the earth there is much leſs of the groſſer æther, in proportion to the finer, than in the regions of the air: and that yet the groſſer æther in the air affects the upper regions of the earth, and the finer æther in the earth the lower regions of the air in ſuch a manner, that, from the top of the air to the ſurface of the earth, and again from the ſurface of the earth to the center thereof, the æther is inſenſibly finer and finer. Imagine, now, any body ſuſpended in the air, or lying on the earth; and the æther being, by the hypotheſis, groſſer in the pores which are in the upper parts of the body, than in thoſe which are in the lower parts; and that groſſer æther, being leſs apt to be lodged in thoſe pores, than the finer æther below; it will endeavour to get out, and give way to the finer æther below, which cannot be, without the bodies deſcending to make room above for it to go out into.

From this ſuppoſed gradual ſubtilty of the parts of the æther, ſome things above might be further illuſtrated, and made more intelligible; but by what has been ſaid, you will eaſily diſcern whether, in theſe conjectures, there be any degree of probability; which is all I aim at. For my own part, I have ſo little fancy to things of this nature, that, had not your encouragement moved me to it, I ſhould never, I think, have thus far ſet pen to paper about them. What is amiſs, therefore, I hope you will the more eaſily pardon in yours, &c.

DE

NATURA ACIDORUM.

DE

NATURA ACIDORUM.

ACIDORUM particulæ ſunt aqueis craſſiores, & propterea minùs volatiles; at terreſtribus multo ſubtiliores, & propterea multo minùs fixæ. Vi magnâ attractivâ pollent, & in hâc vi conſiſtit earum activitas; quâ & corpora diſſolvunt, & organa ſenſuum agitant & pungunt. Mediæ ſunt naturæ inter aquam & corpora, & utraque attrahunt. Per vim ſuam attractivam congregantur circum particulas corporum, ſeu lapideas ſeu metallicas, iiſque undique adhærent arctiſſimè, ut ab iiſdem deinceps per diſtillationem vel ſublimationem vix poſſint ſeparari; attractæ verò & undique congregatæ elevant disjungunt ac diſcutiunt particulas corporum ab invicem; id eſt, corpora diſſolvunt; & per vim attractionis, quâ ruunt in corpora, commovent fluidum, & calorem excitant; particulaſque nonnullas adeo diſcutiunt, ut in aërem convertant, & bullulas generant. Et hæc eſt ratio diſſolutionis & fermentationis violentæ. Acidum verò, attrahendo aquam æquè ac particulas corporum, efficit ut particulæ diſſolutæ promptè miſceantur cum aquâ, eique innatent ad modum ſalium. Et quemadmodum globus terræ per vim gravitatis, attrahendo aquam fortiùs quàm corpora leviora, efficit ut leviora aſcendant in aquâ & fugiant in terra; ſic particulæ ſalium, attrahendo aquam, fugant ſe mutuò, & ab invicem quàm maximè recedendo, per aquam totam expanduntur.

Particulæ

Particulæ ſalis Alkali ex terreis et acidis ſimiliter unitis conſtant; ſed hæ acidæ vi magnâ attractivâ pollent, ut per ignem non ſeparentur à ſale, utque metalla diſſoluta præcipitent, attrahendo ab ipſis particulas acidas quibus diſſolvebantur.

Si particulæ acidæ in minori proportione cum terreſtribus jungantur, tam arctè retinentur à terreſtribus, ut ab iis ſupprimi ac occultari videantur; neque enim ſenſum jam pungunt neque attrahunt aquam; ſed corpora dulcia, & quæ cum aquâ ægrè miſcentur, hoc eſt pinguia componunt: ut fit in Mercurio dulci, ſulphure communi, Lunâ Corneâ, & Cupro, quod Mercurius ſublimatus corroſit. Ab acidi verò ſic ſuppreſſi vi attractivâ fit, ut pinguia corporibus propè univerſis adhæreant, & flammam facilè concipiant; ſi modò acidum calefactum inveniat alias corporum in fumo accenſorum particulas, quas fortiùs attrahat quàm proprias: ſed & acidum in ſulphureis ſuppreſſum, fortiùs attrahendo particulas aliorum corporum, ſcilicet terreas, quàm proprias, fermentationem brutam & naturalem ciet, & fovet, uſque ad putrefactionem compoſiti; quæ putrefactio ſita eſt in eo, quòd Acidi *particulæ* fermentationem diu foventes, tandem in interſtitia minima et partes primæ compoſitionis interjacentia ſeſe inſinuant, intimèque iis partibus unitæ mixtionem novam efficiunt, non amovendam nec cum priore commutandam.

Cogitationes variæ.

Flamma eſt fumus candens, differtque à fumo, ut ferrum rubens ab ignito ſed non rubente.

Calor eſt agitatio partium quaquaverſum.

Nihil eſt abſolutè quieſcens ſecundum partes ſuas, & ideò frigidum, præter atomos, vacui ſcilicet expertes.

Terra augetur aquâ in eam converſâ, et omnia in aquam reduci poſſunt.

Nitrum abit diſtillatione magnam partem in ſpiritum acidum, relictâ terrâ; quia acidum nitri attrahit phlegma, et idcirco ſimul aſcendunt, conſtituuntque ſpiritum. At nitrum carbone accenſum magnam partem abit in Sal Tartari, quia ignis eo modo applicatus partes acidi & terræ in ſe impingit, fortiúſque unit.

Spiritus ardentes ſunt Olea cum phlegmate, per fermentationem, unita.

Tinctura Cochinellæ, cum ſpiritu Vini facta, in aquæ magnam molem immiſſa, parvâ licet doſi, totam tamen aquam inficit; ſcilicet quia particulæ Cochinellæ magis attrahuntur ab aquâ, quàm à ſe mutuò.

Aqua non habet magnam vim diſſolvendi, quia pauco acido gaudet; acidum enim dicimus, quod multum attrahit & attrahitur. Videmus nempe ea quæ in aqua ſolvuntur, lentè & ſine efferveſcentiâ ſolvi; at ubi eſt attractio fortis, & particulæ menſtrui undique attrahuntur à particulâ metalli; vel potius particula metalli undique attrahitur à particulis menſtrui, hæ illam abripiunt, & circumſiſtunt; hoc eſt, metallum corrodunt; hæ eædem particulæ ſenſorio applicatæ ejus partes eodem modo divellunt, doloremque inferunt. Acidæ appellantur relictâ ſcilicet terrâ ſubtili cui adhærebant, ob majorem attractionem ad liquidum linguæ, &c.

In omni ſolutione per menſtruum, particulæ ſolvendæ magis attrahuntur à partibus menſtrui quàm à ſe mutuò.

In omni fermentatione eſt acidum ſuppreſſum, quod coagulat præcipitando.

Oleum, cum nimis magnâ mole phlegmatis intimè mixtum, fit ſalinum quiddam, & ſic acetum conſtituit; hîc etiam Tartari, ſeu terræ admiſtæ, habenda eſt ratio.

Mercurius attrahitur, i. e. corroditur ab acidis; & ſicut pondere obſtructiones tollit, ita vi attractrice acida infringit.

Mercurius eſt volatilis, & facilè elevatur calore; quia ejus particulæ ultimæ compoſitionis ſunt parvæ, & facilè ſeparantur, ſeparatæque ſeſe fugant; ut fit in particulis vaporis fluidorumque rarefactorum.

Aqua comprimi non poteſt; quia ejus particulæ jamjam ſe tangunt: ut ſi ſe tangerent particulæ aëris (comprimi aer poteſt quia ipſius particulæ nondum ſe tangunt) aer evaderet in marmor. Sequitur ex Prop. XXIII. Lib. 2. Princip. Philoſ.

Aurum particulas habet ſe mutuò trahentes; harum ſumma vocentur primæ compoſitionis; harum ſummarum ſummæ, ſecundæ, &c.

Poteſt

Poteſt Mercurius, poteſt Aqua Regis poros pervadere, qui particulas ultimæ compoſitionis interjacent, at non alios. Si poſſet menſtruum alios illos pervadere, vel ſi auri partes primæ & ſecundæ compoſitionis poſſent ſeparari, fieret aurum fluidum & malleabile. Si aurum fermenteſcere poſſet, in aliud quodvis corpus poſſet transformari.

Viſciditas eſt, vel ſolùm defectus fluiditatis (quæ ſita eſt in partium parvitate & ſic ſeparabilitate, intellige partes ultimæ compoſitionis) vel defectus lubricitatis, ſeu lævoris, partes unas ſupra alias labi impediens. Hujus viſciditatis acidum ſæpe cauſa eſt, ſæpe ſpiritus alius lubricus terræ junctus; ut Oleum Terebinthinæ, capiti ſuo mortuo redditum, fit tenax.

Ratio cur charta oleo inuncta tranſitum oleo, non Aquæ, concedat, eſt, quia aqua Oleo non miſcetur, ſed fugatur ab eo.

Acidum primigenium videtur conſtare particulis ſphæricis, hoc eſt ſummè attractivis; & particulæ hæ ſunt majores aquæ, & minores terræ partibus, ſeu inter eas mediæ & utrarumque attractrices.

Cùm acidæ partes, minores ſcilicet, aliquod diſſolvunt, id faciunt, quia partem rei ſolvendæ includunt undique, utpote majorem quâlibet acidi partium.

TABULÆ DUÆ

CALORUM ALTERA

ALTERA REFRACTIONUM.

I.

TABULA QUANTITATUM

ET

GRADUUM CALORIS.

Colorum partes æquales.	Colorum gradus.	Calorum Deſcriptiones et Signa.
		CALOR aëris hyberni ubi aqua incipit gelu rigeſcere. Innoteſcit hic calor accuratè locando Thermometrum in nive compreſſâ, quo tempore gelu ſolvitur.
0, 1, 2.		Calores aëris hyberni.
2, 3, 4.		Calores aëris verni & autumnalis.
4, 5, 6.		Calores aëris æſtivi.
6.		Calor uſitatus aëris meridiani circa menſem Julium.
12.	1	Calor maximus quem Thermometer ad contactum corporis humani concipit. Idem circiter eſt calor avis ova incubantis.
$14\frac{3}{11}$	$1\frac{1}{4}$	Calor balnei propè maximus quem quis, manu immerſâ & conſtanter agitatâ, diutiùs perferre poteſt. Idem ferè eſt calor ſanguinis recens effuſi.
17	$1\frac{1}{4}$	Calor balnei maximus quem quis, manu immerſâ & immobili manente, diutiùs perferre poteſt.
$20\frac{2}{11}$	$1\frac{3}{4}$	Calor balnei quo cera innatans & liquefacta deferveſcendo rigeſcit, & diaphaneitatem amittit.

24	2	Calor balnei quo cera innatans incaleſcendo liqueſcit, & in continuo fluxu ſine ebullitione conſervatur.
28 $\frac{6}{11}$	$2\frac{1}{4}$	Calor mediocris inter calores quo cera liqueſcit, & aqua ebullit.
34	$2\frac{1}{2}$	Calor quo aqua vehementer ebullit; & miſtura duarum partium plumbi, trium partium ſtanni & quinque partium biſmuti deferveſcendo rigeſcit. Incipit aqua ebullire calore partium 33, & calorem partium pluſquam $34\frac{1}{2}$ ebulliendo vix concipit. Ferrum verò deferveſcens calore partium 35 vel 36, ubi aqua calida, & 37, ubi frigida in ipſum guttatim incidit, deſinit ebullitionem excitare.
40 $\frac{4}{11}$	$2\frac{3}{4}$	Calor minimus quo miſtura unius partis plumbi, quatuor partium ſtanni, & quinque partium biſmuti incaleſcendo liqueſcit, & in continuo fluxu conſervatur.
48	3	Calor minimus quo miſtura æqualium partium ſtanni & biſmuti liqueſcit. Hæc miſtura calore partium 47 deferveſcendo coagulatur.
57	$3\frac{1}{4}$	Calor quo miſtura duarum partium ſtanni, & unius partis biſmuti funditur, ut & miſtura trium partium ſtanni & duarum plumbi: ſed miſtura quinque partium ſtanni & duarum partium biſmuti hoc calore deferveſcendo rigeſcit. Et idem facit miſtura æqualium partium plumbi & biſmuti.
68	$3\frac{1}{2}$	Calor minimus quo miſtura unius partis biſmuti, & octo partium ſtanni funditur. Stannum per ſe funditur calore partium 72, & deferveſcendo rigeſcit calore partium 70.
81	$3\frac{3}{4}$	Calor quo biſmutum funditur; ut & miſtura quatuor partium plumbi, & unius partis ſtanni. Sed miſtura quinque partium plumbi, & unius partis ſtanni, ubi fuſa eſt & deferveſcit, in hoc calore rigeſcit.
96	4	Calor minimus quo plumbum funditur. Plumbum incaleſcendo funditur calore partium 96 vel 97, & deferveſcendo rigeſcit calore partium 95.

Calor

114	$4\frac{1}{4}$	Calor quo corpora ignita, deferveſcendo, penitus deſinunt in tenebris nocturnis lucere; & viciſſim, incaleſcendo, incipiunt in iiſdem tenebris lucere, ſed luce tenuiſſimâ quæ ſentiri vix poſſit. Hoc calore liqueſcit miſtura æqualium partium ſtanni & reguli martis; & miſtura ſeptem partium biſmuti & quatuor partium ejuſdem Reguli deferveſcendo rigeſcit.
136	$4\frac{1}{4}$	Calor quo corpora ignita in tenebris nocturnis candent, in crepuſculo verò neutiquam. Hoc calore tum miſtura duarum partium reguli martis & unius partis biſmuti, tum etiam miſtura quinque partium reguli martis & unius partis ſtanni deferveſcendo rigeſcit. Regulus per ſe rigeſcit calore partium 146.
161	$4\frac{3}{4}$	Calor quo corpora ignita in crepuſculo, proximè ante ortum ſolis, vel poſt occaſum ejus, manifeſtò candent; in clarâ verò diei luce neutiquam, aut non niſi perobſcurè.
192	5	Calor prunarum in igne parvo culinari, ex carbonibus foſſilibus bituminoſis conſtructo, & abſque uſu follium ardente. Idem eſt calor ferri, in tali igne quantum poteſt candentis. Ignis parvi culinaris, qui ex lignis conſtat, calor paulo major eſt; nempe partium 200 vel 210. Et ignis magni major adhuc eſt calor; præſertim ſi follibus cieatur.

In hujus Tabulæ columnâ primâ habentur gradus caloris in proportione arithmeticâ; computum inchoando à calore, quo aquâ incipit gelu rigeſcere, tanquam ab infimo caloris gradu, ſeu communi termino caloris & frigoris; & ponendo calorem externum corporis humani eſſe partium duodecim. In ſecundâ columnâ habentur gradus caloris in ratione geometricâ; ſic ut ſecundus gradus ſit duplo major primo, tertius item ſecundo, & quartus tertio, & primus ſit calor externus corporis humani ſenſibus æquatus. Patet autem per hanc Tabulam, quòd calor aquæ bullientis ſit ferè triplo major quàm calor corporis humani; & quòd calor ſtanni liqueſcentis ſit ſextuplo major, & calor plumbi liqueſcentis octuplo major, & calor Reguli liqueſcentis duodecuplo ma-

jor, & calor ordinarius ignis culinaris ſexdecim vel ſeptemdecim vicibus major, quàm calor idem corporis humani.

Conſtructa fuit hæc Tabula ope Thermometri & Ferri candentis. Per Thermometrum inveni menſuram calorum omnium uſque ad calorem quo ſtannum funditur, & per ferrum calefactum inveni menſuram reliquorum. Nam calor quem ferrum calefactum corporibus frigidis ſibi contiguis dato tempore communicat, hoc eſt, calor quem ferrum dato tempore amittit, eſt ut calor totus ferri. Ideoque ſi tempora refrigerii ſumantur æqualia, calores erunt in ratione geometricâ, & propterea per tabulam logarithmorum facilè inveniri poſſunt.

Primùm igitur per Thermometrum, ex oleo lini conſtructum, inveni, quòd ſi oleum, ubi Thermometer in nive liqueſcente locabatur, occupabat ſpatium partium 10000; idem oleum, calore primi gradûs ſeu corporis humani rarefactum, occupabat ſpatium 10256; & calore aquæ jamjam ebullire incipientis, ſpatium 10705; & calore aquæ vehementer ebullientis, ſpatium 10725; & calore ſtanni liquefacti deferveſcentis, ubi incipit rigeſcere, & conſiſtentiam amalgamatis induere, ſpatium 11516; & ubi omnino rigeſcit, ſpatium 11496. Igitur oleum rarefactum fuit ad dilatatum in ratione 40 ad 39, per calorem corporis humani; in ratione 15 ad 14, per calorem aquæ bullientis; in ratione 15 ad 13, per calorem ſtanni deferveſcentis, ubi incipit coagulari & rigeſcere; & in ratione 23 ad 20, per calorem quo ſtannum deferveſcens omnino rigeſcit. Rarefactio aëris æquali calore fuit decuplo major quàm rarefactio olei; & rarefactio olei quaſi quindecim vicibus major quàm rarefactio ſpiritûs vini. Et ex his inventis, ponendo calores olei ipſius rarefactioni proportionales, & pro calore corporis humani ſcribendo partes 12, prodiit calor aquæ ubi incipit ebullire, partium 33; & ubi vehementiùs ebullit, partium 34; & calor ſtanni, ubi vel liqueſcit, vel deferveſcendo incipit rigeſcere, & conſiſtentiam amalgamatis induere, prodiit partium 72; & ubi deferveſcendo rigeſcit, & induratur, partium 70.

His cognitis, ut reliqua inveſtigarem, calefeci ferrum ſatis craſſum, donec ſatis canderet; & ex igne cum forcipe etiam candente exemptum locavi ſtatim in loco frigido, ubi ventus conſtanter ſpirabat; & huic imponendo particulas diverſorum metallorum, &

aliorum

aliorum corporum liquabilium, notavi tempora refrigerii; donec particulæ omnes, amiſſâ fluiditate, rigeſcerent, & calor ferri æquaretur calori corporis humani. Deinde ponendo quòd exceſſus calorum ferri & particularum rigeſcentium ſupra calorem atmoſphæræ, Thermometro inventum, eſſent in progreſſione geometricâ, ubi tempora ſunt in progreſſione arithmeticâ, calores omnes innotuere. Locavi autem ferrum, non in aëre tranquillo, ſed in vento uniformiter ſpirante; ut aër à ferro calefactus ſemper abriperetur à vento, & aër frigidus in locum ejus uniformi cum motu ſuccederet. Sic enim aëris partes æquales æqualibus temporibus calefactæ ſunt, & calorem conceperunt calori ferri proportionalem.

Calores autem ſic inventi eandem habuerunt rationem inter ſe cum caloribus per Thermometrum inventis; & propterea rarefactiones olei ipſius caloribus proportionales eſſe rectè aſſumpſimus.

II.

Tabula Refractionum Siderum ad Altitudines apparentes.

Alt. Appar. deg.	m.	Refractio. m.	sec.	Alt. Appar. deg.	Refractio. m.	sec.	Alt. Appar. deg.	Refractio. m.	sec.
0	0	33	45	16	3	4	46	0	52
0	15	30	24	17	2	53	47	0	50
0	30	27	35	18	2	43	48	0	48
0	45	25	11	19	2	34	49	0	47
1	0	23	7	20	2	26	50	0	45
1	15	21	20	21	2	18	51	0	44
1	30	19	46	22	2	11	52	0	42
1	45	18	22	23	2	5	53	0	40
2	0	17	8	24	1	59	54	0	39
2	30	15	2	25	1	54	55	0	38
3	0	13	20	26	1	49	56	0	36
3	30	11	57	27	1	44	57	0	35
4	0	10	48	28	1	40	58	0	34
4	30	9	50	29	1	36	59	0	32
5	0	9	2	30	1	32	60	0	31
5	30	8	21	31	1	28	61	0	30
6	0	7	45	32	1	25	62	0	28
6	30	7	14	33	1	22	63	0	27
7	0	6	47	34	1	19	64	0	26
7	30	6	22	35	1	16	65	0	25
8	0	6	0	36	1	13	66	0	24
8	30	5	40	37	1	11	67	0	23
9	0	5	22	38	1	8	68	0	22
9	30	5	6	39	1	6	69	0	21
10	0	4	52	40	1	4	70	0	20
11	0	4	27	41	1	2	71	0	19
12	0	4	5	42	1	0	72	0	18
13	0	3	47	43	0	58	73	0	17
14	0	3	31	44	0	56	74	0	16
15	0	3	17	45	0	54	75	0	15

DE

PROBLEMATIS BERNOULLIANIS.

PRÆNOBILI VIRO

D. CAROLO MONTAGUE

Armig. Scacarii Cancellari, & Societatis Regiæ Præsidi.

Jan. 30, 169⅚.

Accepi, Vir Ampliſſime, heſterno die duo Problematum, à JOHANNE BERNOULLIO Mathematicorum acutiſſimo propoſitorum, exemplaria, *Groningæ* edita in hæc verba

Acutiſſimis qui toto Orbe florent Mathematicis S. P. D.

JOHANNES BERNOULLI, MATH. P. P.

CUM *compertum habeamus, vix quicquam eſſe, quod magis excitet generoſa ingenia ad moliendum quod conducit augendis ſcientiis, quàm difficilium pariter & utilium quæſtionum propoſitionem; quarum enodatione, tanquam ſingulari, ſiquâ aliâ viâ, ad nominis claritatem perveniant ſibique apud poſteritatem æterna extruant monumenta: ſic me nihil gratius orbi mathematico facturum*

rum ſperavi, quàm ſi, imitando exemplum tantorum virorum, MERSENNI, PASCALII, FERMATII, *præſertim recentis illius anonymi ænigmatiſtæ* Florentini *, *aliorumque qui idem ante me fecerunt, præſtantiſſimis hujus ævi analyſtis proponerem aliquod Problema, quo, quaſi lapide Lydio, ſuas methodos examinare, vires intendere, &, ſi quid invenirent, nobiſcum communicare poſſent; ut quiſque ſuas exinde promeritas laudes à nobis, publicè id profitentibus, conſequeretur.*

Factum autem illud eſt ante ſemeſtre in Actis Lipſ. m. Jun. *pag.* 269, *ubi tale problema propoſui, cujus utilitatem cum jucunditate conjunctam videbunt omnes, qui cum ſucceſſu ei ſe applicabunt. Sex menſium ſpatium à primâ publicationis die Geometris conceſſum eſt, intra quod, ſi nulla ſolutio prodiret in lucem, me meam exhibiturum promiſi. Sed ecce elapſus eſt terminus, & nihil ſolutionis comparuit; niſi quod Celeb.* LEIBNITIUS, *de profundiore geometriâ præclarè meritus, me per litteras certiorem fecerit, ſe jam feliciter diſſolviſſe nodum pulcherrimi hujus, uti vocabat, & inauditi antea problematis; inſimulque humaniter rogavit, ut præſtitutum limitem ad proximum paſcha extendi paterer; quo interea apud Gallos, Italóſque, idem illud publicari poſſet, nulluſque adeò ſupereſſet locus ulli de anguſtiâ termini querelæ. Quam honeſtam petitionem non ſolùm indulſi, ſed ipſe hanc prorogationem promulgare decrevi; viſurus num qui ſint, qui nobilem hanc & arduam quæſtionem aggreſſuri, poſt longum temporis intervallum, tandem enodationis compotes fierent. Illorum interim in gratiam, ad quorum manus* Acta Lipſienſia *non perveniunt, propoſitionem hîc repeto.*

* *Vincentius* VIVIANI ann. 1692. ænigma geometricum propoſuit, de miro opificio Teſtudinis quadrabilis hemiſphericæ; videantur *Acta Eruditorum* hujus anni, menſe Junio, *pag.* 274, vel Vita VIVIANI in Hiſt. Acad. Reg. Scient. Pariſ. ann. 1703.

PROB. I.

Determinare lineam curvam data duo puncta, in diverſis ab horizonte diſtantiis & non in eâdem rectâ verticali poſita, connectentem, ſuper quâ mobile, propriâ gravitate decurrens & à ſuperiori puncto moveri incipiens, citiſſimè deſcendat ad punctum inferius.

Senſus problematis hic eſt: ex infinitis lineis, quæ duo illa data puncta conjungunt, vel ab uno ad alteram duci poſſunt, eligatur illa, juxta quam ſi incurvetur lamina tubi canaliſve formam habens, ut ipſi impoſitus globulus & liberè dimiſſus, iter ſuum ab uno puncto ad alterum emetiatur tempore breviſſimo.

Ut verò omnem ambiguitatis anſam præcaveamus; *ſcire B. L. volumus, nos hìc admittere* GALILÆI *hypotheſin, de cujus veritate, ſepoſitâ reſiſtentiâ, jam nemo eſt ſaniorum geometrarum qui ambigat*; velocitates ſcilicet acquiſitas gravium cadentium eſſe in ſubduplicatâ ratione altitudine emenſarum: *quamquam aliàs noſtra ſolvendi methodus univerſaliter ad quamvis aliam hypotheſin ſeſe extendat.*

Cùm itaque nihil obſcuritatis ſuperſit, obnixè rogamus omnes & ſingulos hujus ævi geometras, accingant ſe promptè, tentent, diſcutiant quicquid in extremo ſuarum methodorum receſſu abſconditum tenent. Rapiat qui poteſt præmium, quod ſolutori paravimus; *non quidem auri, non argenti ſummam, quo abjecta tantùm & mercenaria conducuntur ingenia, à quibus ut nihil laudabile, ſic nihil, quod ſcientiis fructuoſum, expectamus*; *ſed cùm virtus ſibi ipſi ſit merces pulcherrima, atque gloria immenſum habeat calcar, offerimus præmium, quale convenit ingenui ſanguinis viro, conſertum ex honore, laude, & plauſu*; *quibus magni noſtri Apollinis perſpicacitatem, publicè & privatim, ſcriptis & dictis coronabimus, condecorabimus, & celebrabimus.*

Quòd ſi verò feſtum paſchatis præterierit, nemine deprehenſo, qui quæſitum noſtrum ſolverit; *nos quæ ipſi invenimus publico non invidebimus: incomparabilis enim* LEIBNITIUS *ſolutiones tum ſuam, tum noſtram, ipſi jam pridem commiſſam, protinus, ut ſpero, in lucem emittet*; *quas ſi geometræ, ex penitiori quodam fonte petitas, perſpexerint,*

perſpexerint, nulli dubitamus, quin anguſtos vulgaris geometriæ limites agnoſcant, noſtraque proin inventa tanto pluris faciant, quanto pauciores eximiam noſtram quæſtionem ſoluturi extiterint, etiam inter illos ipſos, qui per ſingulares, quas tantoperè commendant, methodos, interioris geometriæ latibula non ſolùm intimè penetrâſſe, ſed etiam ejus pomœria, theorematibus ſuis aureis, nemini, ut putabant, cognitis, ab aliis tamen jam longè priùs editis, mirum in modum extendiſſe gloriantur.

PROBLEMA ALTERUM PURE GEOMETRICUM.

Quod priori ſubnectimus, & Strenæ loco Eruditis proponimus.

Ab EUCLIDIS *tempore vel tyronibus notum eſt, ductam utcunque à puncto dato rectam lineam, à circuli peripheriâ ita ſecari, ut rectangulum duorum ſegmentorum, inter punctum datum & utramque peripheriæ partem interceptorum, ſit eidem conſtanti perpetuò æquale. Primus ego oſtendi, in eodem* Actorum Jun. *pag.* 265, *hanc proprietatem infinitis aliis curvis convenire, illamque adeò circulo non eſſe eſſentialem: arreptâ hinc occaſione, propoſui geometris determinandam Curvam, vel Curvas, in quibus non rectangulum, ſed ſolidum ſub uno & quadrato alterius ſegmentorum æquetur ſemper eidem: ſed à nemine hactenus ſolvendi modus prodiit; exhibebimus eum, quandocumque deſiderabitur. Quoniam autem non niſi per Curvas tranſcendentes quæſito ſatisfacimus; en aliud, cujus ſolutio per merè algebraicas in noſtrâ eſt poteſtate.*

Quæritur Curva ejus proprietatis, ut duo illa ſegmenta, ad quamcunque potentiam datam elevata & ſimul ſumta, faciant ubique unam eandemque ſummam.

Caſum ſimpliciſſimum, exiſtente ſcilicet numero potentiæ 1, *ibidem in* Actis, *pag.* 266, *jam ſolutum dedimus; generalem verò ſolutionem, quam etiamnum premimus, analyſis eruendam relinquimus.*

Dabam Groningæ, ipſis Cal. Jan. 1697.

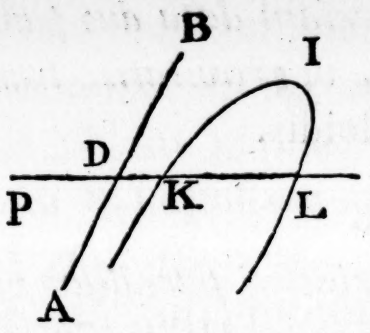
HACTENUS BERNOULLIUS. *Problematum verò ſolutiones ſunt hujuſmodi.*

A dato

A dato puncto A, ducatur recta infinita, APCZ, horizonti parallela; & ſuper eâdem rectâ deſcribatur, tum Cyclois quæcunque, AQP, rectæ AB ductæ, & ſi opus ſit productæ, occurrens in puncto Q, tum cyclois alia ABC; cujus baſis, & altitudo ſit ad prioris baſem & altitudinem reſpectivè, ut AB ad AQ. Et hæc cyclois noviſſima tranſibit per punctum B, & erit curva illa linea, in quâ grave à puncto A ad punctum B vi gravitatis ſuæ citiſſimè perveniet. Q. E. I.

PROBLEMA II.

Problema alterum, ſi rectè intellexi (nam, quæ in *Actis Lipſ.* ab auctore citantur ad id ſpectantia nondum vidi) ſic proponi poteſt. Quæritur Curva KIL eâ lege, ut ſi recta PKL à dato quodam puncto P, ceu polo, utcunque ducatur, & eidem curvæ in punctis duobus K, & L occurrat; poteſtates duorum ejus ſegmentorum, PK, & PL, à dato illo puncto P ad occurſus illos ductorum, ſi ſint æquè altæ, (id eſt, vel quadrata, vel cubi, vel quadrato-quadrata, &c.) datam ſummam PKq + PLq vel PK *cub.* + PL *cub.* &c. (in omni rectæ illius poſitione) conficiant.



SOLUTIO.

Per datum quodvis punctum, A, ducatur recta quævis infinita poſitione data, ADB, rectæ mobili PKL occurrens in D: & nominentur AD, x; & PK, vel PL, y; ſintque Q, & R quantitates ex quantitatibus quibuſcunque datis, & quantitate x quomodocunque conſtantes, & relatio inter x, & y, definiatur per hanc æquationem $yy + Qy + R = 0$. Et ſi R ſit quantitas data, rectangulum ſub ſegmentis PK & PL dabitur. Si Q ſit quantitas data, ſumma ſegmentorum illorum, ſub ſignis propriis conjunctorum, dabitur (a). Si $QQ - 2R$ datur, ſumma quadratorum (PKq + PLq) dabitur. Si $Q^3 - 3QR$ data ſit quantitas, ſumma cuborum (PK *cub.* + PL *cub.*) dabitur. Si $Q^4 - 4QQR + 2RR$ data ſit quantitas, ſumma quadrato-

(a) Sequuntur hæc ex notiſſimo Theoremate Girardi de coefficientium procreatione.. Vide Arith. Vniv. C. XVIII.

quadratorum ($PKqq + PLqq$) dabitur. Et ſic deinceps in infinitum (a). Efficiatur itaque ut R, Q, $QQ-2R$, $Q3-3QR$, &c. datæ ſint quantitates, & problema ſolvetur. Q. E. F.

Ad eundem modum Curvæ inveniri poſſunt, quæ tria, vel plura abſcindent ſegmenta, ſimiles proprietates habentia. Sit æquatio $y^3 + Qyy + Ry + S = 0$, ubi Q, R, & S, quantitates ſignificant ex quantitatibus quibuſcunque datis, & quantitate x utcunque conſtantes, & Curva abſcindet ſegmenta tria. Et ſi S data ſit quantitas contentum ſolidorum illorum trium dabitur. Si Q ſit quantitas data ſumma trium illorum dabitur. Si $QQ-2R$ ſit data quantitas, ſumma quadratorum ex tribus illis dabitur.

IV.

De ratione temporis, quo grave labitur per rectam data duo puncta conjungentem ad tempus breviſſimum, quo, vi gravitatis, tranſit ab horum uno ad alterum per arcum Cycloidis.

THEOREMA.

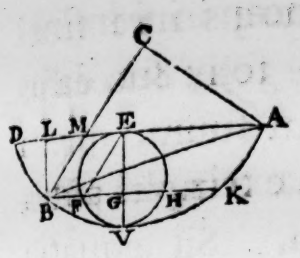
Si in Cycloide AVD, *cujus baſis* AD *eſt horizonti parallela, vertice* V *deorſum ſpectante, ex* A *ducatur utcunque recta* AB Cycloidi *occurrens in* B, *ex quo ducatur recta* BC *curvæ* Cycloidis BD *in* B *normalis, ad quam ex* A *demittatur perpendicularis recta* AC: *dico tempus, quo grave è quiete cadens ex* A, *vi ſuæ gravitatis, decurrit rectam* AB, *eſſe ad tempus, quo percurrit curvam* AVB, *ſicut recta* AB *ad rectam* AC.

Per B ducatur BL, parallela Cycloidis axi VE; & BK, baſi AD parallela occurrens axi in G, &, circulo ſuper diametrum EV deſcripto, in F & H; Cycloidi denique in K. Ducatur recta EF, quæ, ex Cycloidis naturâ, parallela eſt rectæ BC. Unde BM eſt æqualis EF, & EM æqualis BF; quæ, propter Cycloidem, æquatur arcui VF; & proinde AM eſt æqualis arcui EHVF.

Per Prop. XXV. Par. II. *Horologii Oſcillatorii* HUGENII, tempus, quo grave è quiete cadens percurrit AV, eſt ad tempus caſûs per EV, ut ſemi-circumferentia ad diametrum; &, per dictæ partis propoſitionem ultimam, tempus, quo grave percurrit VB, poſt decurſam AV (nempe æquale tempori, quo grave percurrit KV,

(a) Patent hæc per formulas Girardi pro conſummatione poteſtatum è radicibus æquationum ncognitis. Vide Arithmet. Univ. C. XVIII. Not. b.

poſt



poſt decurſam AK), eſt ad tempus lapſûs per AV, ſicut arcus VF ad ſemi-circumferentiam ; adeòque ad tempus caſûs per EV, ſicut FV ad diametrum, quare tempus, quo grave percurrit curvam AVB, eſt ad tempus caſûs per EV, ſicut arcus EHVF, ad diametrum EV. Sed, tempus caſûs per EV, eſt ad tempus caſûs per LB (ſive EG), ſicut EV ad EF. Igitur, ex æquo, tempus, quo grave percurrit AVB, eſt ad tempus caſûs per LB, ſicut arcus EHVF ad ſubtenſam EF ; hoc eſt ut recta AM, ad rectam MB. Rurſus, tempus caſûs per LB eſt ad tempus lapſûs per AB, ut LB ad AB. Ergo ratio temporis, quo grave percurrit AVB, ad tempus quo percurrit AB, componitur ex ratione AM ad MB, & ratione LB ad BA ; adeoque æqualis eſt rationi AM × LB ad MB × BA. Sed AM × LB eſt æquale MB × AC ; quia utrumque æquatur duplo trianguli ABM. Et igitur, tempus, quo grave è quiete cadens percurrit curvam Cycloidis AVB, eſt ad tempus, quo percurrit rectam AB, ſicut MB × AC, ad MB × BA ; id eſt, ſicut AC ad AB. Q. E. D.

Similiterque procedet demonſtratio, ſi punctum B, ſit inter A & V.

V.

PROBLEMATIS

Olim in Actis Eruditorum Lipſiæ propoſiti

SOLUTIO GENERALIS.

In *Actis Eruditorum* pro menſe *Octobri* anni 1698, pag. 471, D. JOHANNES BERNOULLIUS hæc ſcripſit. " Methodum, quam " optaveram generalem ſecandi Curvas ordinatim poſitione datas, " ſive algebraicas, ſive tranſcendentales, in angulo recto, ſive " obliquo invariabili, ſive datâ lege variabili, tandem ex voto " erui : cui, LEIBNITIO approbatore, ne γρὺ addi poteſt ad ul- " teriorem perfectionem, & vel ideo tantùm quòd perpetuò ad " æquationem deducat : in quâ ſi interdum indeterminatæ ſunt " inſeparabiles, methodus non ideo imperfectior eſt, non enim " hujus ſed alîus eſt methodi indeterminatas ſeparare : rogamus

" igitur fratrem ut velit ſuas quoque vires exercere in re tanti
" momenti. Suſcepti laboris non pœnitebit, ſi felix ſucceſſus
" fructu jucundo compenſaverit. Scio relicturum ſuum, quem
" nunc fovet, modum, qui in pauciſſimis tantùm exemplis adhi-
" beri poteſt."

Hi tres viri celeberrimi feſe, jam ab annis quatuor vel quinque circiter, in ſolvendis hujuſmodi problematibus exercuerant. Abſque ſpiritu divinandi eandem ſolutionem cum *Bernoullianâ* tradere difficile fuerit. Sufficit quòd ſolutio ſequens ſit generalis, & ad æquationem ſemper deducat.

PROBLEMA.

Quæritur methodus generalis inveniendi ſeriem Curvarum, quæ Curvas in ſerie aliâ quâcumque datâ conſtitutas, ad angulum vel datum, vel datâ lege variabilem, ſecabunt.

SOLUTIO.

Natura Curvarum ſecandarum dat tangentes earundem ad interſectionum puncta quæcunque: & anguli interſectionum dant perpendicula Curvarum ſecantium; & perpendicula duo coeuntia, per concurſum ſuum ultimum, dant centrum curvaminis Curvæ ſecantis ad punctum interſectionis cujuſcunque. Ducatur abſciſſa in ſitu quocumque commodo, & ſit ejus fluxio unitas; & poſitio perpendiculi dabit fluxionem primam ordinatæ ad Curvam quæſitam pertinentis; & curvamen hujus Curvæ dabit fluxionem ſecundam ejuſdem ordinatæ. Et ſic problema ſemper deducetur ad æquationes. Quod erat faciendum.

SCHOLIUM.

Non hujus, ſed alîus eſt methodi æquationes reducere, & indeterminatas ſeparare; abſolutè, ſi fieri poſſit; ſin minus, per ſeries infinitas. Problema hocce, cùm nullius ferè ſit uſûs, in *Actis Eruditorum* annos plures neglectum & inſolutum manſit. Et eâdem de cauſâ ſolutionem ejus non ulteriùs proſequor.

PROPOSITIONS

FOR

DETERMINING THE MOTION OF A BODY

URGED BY

TWO CENTRAL FORCES.

COMMUNICATED

BY THE LATE WILLIAM JONES, ESQ.

TO

THE LATE DR. BRADLEY.

PROPOSITION I.

If a body, projected in a given direction, be conſtantly drawn towards two fixed points, which are not both in the ſame plane with the direction, it ſhall deſcribe, by lines drawn from the two fixed points, equal ſolids in equal times, about the line joining the ſaid points.

SUPPOSE a body projected in the direction AP (fig. 1.) and acted upon by two centripetal forces towards the fixed points

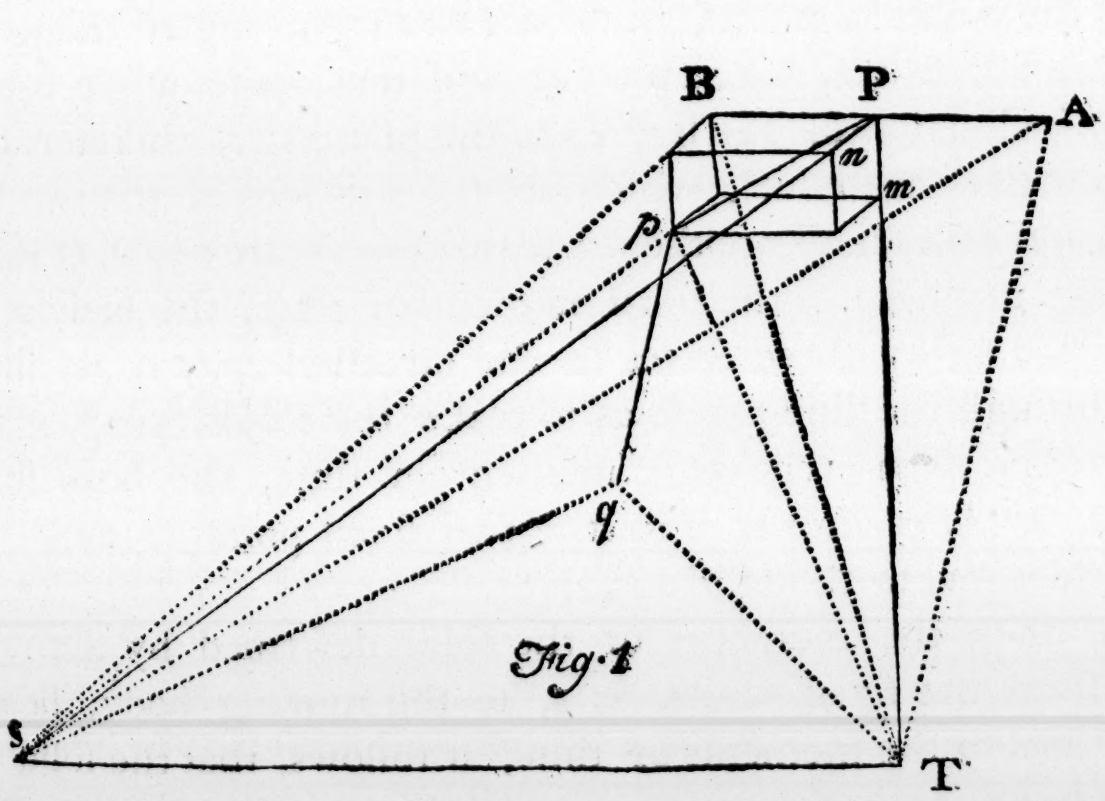


Fig. 1

T and S; and the angles PAS, PAT in different planes. Imagine the time divided into equal moments; and in the firſt moment the

the body, by its given force, ſhould move through the line AP: likewiſe in the ſecond moment, if no new force was added, it ſhould continue to move in the ſame ſtrait line through PB = AP. But when the body has come to P, ſuppoſe it acted upon by two centripetal forces, in the directions PT, PS. Suppoſe theſe forces in proportion to that in the direction AP, as the lines P*m*, and P*n*, to the line AP = PB; and with the three lines PB, P*n*, P*m*, compleat the parallelopipid P*p*; and the body in P being acted on by theſe three forces, in the directions PB, P*m*, P*n*, which forces are as theſe three lines, it ſhall move through the diagonal of the parallelopipid made on theſe lines; that is, in the ſecond moment of time the body, inſtead of moving from P to B, ſhall move from P to *p*. And drawing the lines s*p*, T*p*, and SA, TA, I ſay the ſolid SPT*p* is equal to the ſolid SATP.

Draw the lines SB, TB. The ſolids SPTB, SPT*p*, having the ſame baſe SPT, and being between the ſame parallel planes *nm* and B*p*, are equal. Again, the ſolids SPTB, SPTA, are equal; becauſe being on the ſame baſe SPT, they have equal altitudes. (Letting fall perpendiculars from the points B and A, upon the ſame plane SPT, in which from theſe perpendiculars, drawing lines to the points A and B, there are two right-angled triangles, with one ſide equal, *viz.* PB = AP; and one acute angle equal, *viz.* the inclination of the line AB to the plane SPT, therefore the perpendiculars are equal, which are the altitudes of the two ſolids SPTB, SPTA). Therefore the ſolids SPT*p*, SATP, are equal. Q. E. D.

In like manner, in the third moment of time, the body at *p*, being acted on by three forces in the direction P*p*, *p*T, *p*S, ſhall move through the line *pq*, ſo as to make the ſolids, SPT*p*, s*p*T*q*, equal. And ſo in every other moment of time, the body ſhall deſcribe, by lines drawn from the two fixed points T and S, a little ſolid equal to the ſolid SATP.

Thus then the moments of the ſolid deſcribed by the rays drawn from the fixed points, T, S, to the moving body, being proportional to the moments of time, it follows, that the ſolid itſelf is proportional to the time in which it is deſcribed. Q. E. D.

PROP.

PROP. II.

If a body, acted upon by two centripetal forces, as above, revolve in a curve round the line joining these two centers, its velocity in any point of the curve shall be reciprocally proportional to a rectangle of the perpendicular from one of the centers upon the tangent to the given point in the curve, and the perpendicular from the other center upon the plane of the aforesaid perpendicular and tangent.

Let the body P move in the curve P*p* (fig. 2.) round the line TS, joining the two centers of attraction S and T. Let PZ be a

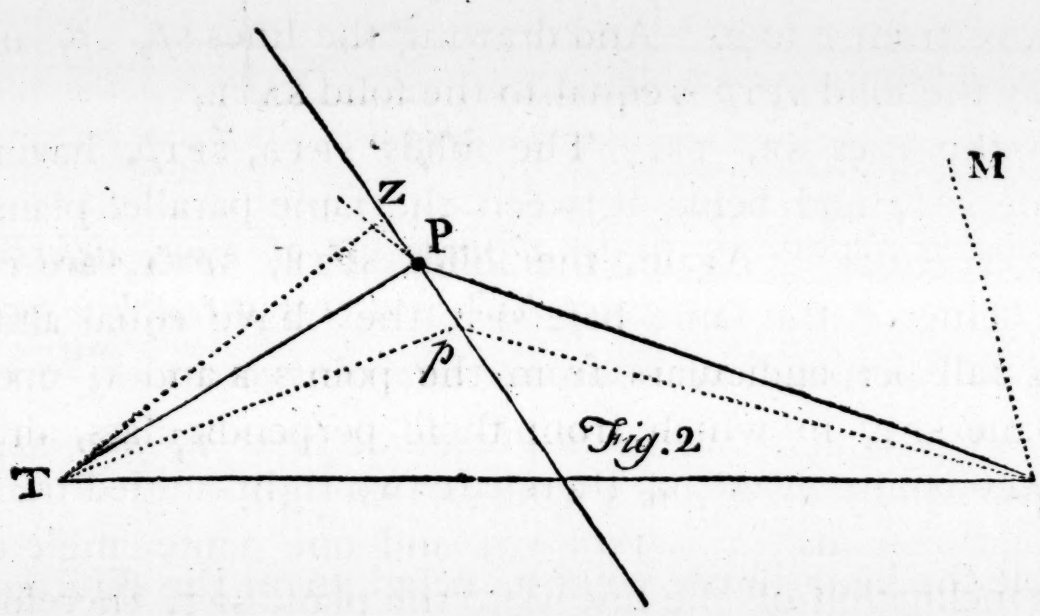


Fig. 2.

tangent to the curve in P ; upon which, from T, let fall the perpendicular TZ ; and upon the plane of the lines TZ, ZP let fall from S the perpendicular SM ; and the velocity of the body in P shall be reciprocally as the rectangle TZ × SM.

Suppose in a moment of time the body moves from P to *p*, draw the lines *p*S and *p*T. The time being given, the velocity is directly as the space ; that is, the velocity of the body at P is directly as the line P*p*.

Again, the time being given, the solid TP*p*S, which is proportional to the time (by the preceding Proposition) is also given. But the solid TP*p*S is proportional to the base multiplied by the perpendicular altitude, that is, to the triangle TP*p*, into the perpendicular altitude SM. The triangle TP*p* is proportional to

Pp × TZ, therefore the ſolid TPps is proportional to Pp × TZ × SM. And this being a given quantity, Pp becomes reciprocally proportional to TZ × SM, that is, the velocity is reciprocally as the rectangle TZ × SM. Q. E. D.

PROP. III.

If a body, attracted by two fixed points T, S *(fig.* 3.*) revolve in a curve, it ſhall, with a ray drawn from one of theſe points,* T, *to*

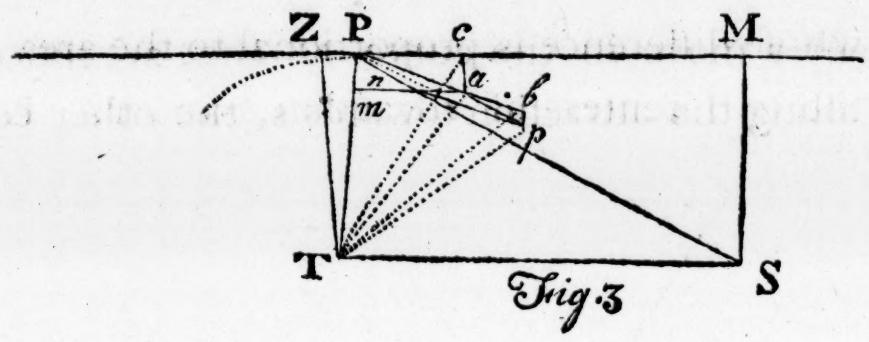


Fig. 3

any point in the curve, in equal moments of time, deſcribe unequal areas, whoſe differences ſhall be as the attractive force towards the other point S, *multiplied into the line* TZ × $\frac{PM}{PS}$; PM *being a tangent to the curve in the given point* P, *and* TZ, SM, *perpendiculars from* T *and* S *upon this tangent.*

Suppoſe the body in the point P, acted on in the direction Pc, P*m*, P*n*, with forces proportional to theſe lines. By the forces Pc, and P*m*, it ſhould move through P*a*, the diagonal of the parallelogram *m*Pc; and with the forces P*a* and P*n* it ſhould move through P*p*, the diagonal of the parallelogram *a*P*n*. Draw the lines *c*T, *a*T, *p*T; deſcribe the arch *pb* with the radius P*p*, and draw *b*T. I ſay, the difference of the triangles P*c*T, P*a*T, is proportional to P*n* × TZ × $\frac{PM}{PS}$.

The triangles P*c*T, P*a*T, being between the ſame parallelas P*m*, *ca*, are equal.—Again, the triangle P*p*T = P*b*T, becauſe the ſides P*p*, P*b*, are equal, and the angle *p*PT common (the angle *b*P*p* being infinitely ſmall) therefore P*p*T − P*c*T = P*b*T − P*a*T = *ab*T. But the triangle *ab*T is as the baſe *ab* × into the perpendicular altitude TZ. And the triangles *apb*, SPM, right-angled at M and *b*, have

have the angle *pab* = the angle SPM (the lines PS, *ap*, and PM, P*p*, P*b*, being ſuppoſed parallel) therefore they are ſimilar, that is, PS : PM :: *ap* : *ab*. Therefore $ab = ap \times \frac{PM}{PS}$: and the triangle *ab*T is as $\frac{TZ \times ap \times Pm}{PS} = Pn \times TZ \times \frac{PM}{PS}$. Q. E. D.

Now the difference of the triangles P*c*T, P*p*T, is the difference of the areas deſcribed by the body P, with the ray PT in the ſame moment of time. Therefore the body P, thus conſtantly acted upon by two centripetal forces, ſhall revolve in a curve; deſcribing about one of theſe centers, in equal moments of time, unequal areas, whoſe difference is proportional to the area $f \times TZ \times \frac{PM}{PS}$. Q. E. D. (Calling the attraction towards S, the other center, *f*.)

FOUR

LETTERS

FROM

SIR ISAAC NEWTON

TO

DOCTOR BENTLEY;

CONTAINING

SOME ARGUMENTS

IN

PROOF OF A DEITY.

LETTERS, &c.

SIR,

WHEN I wrote my Treatiſe about our Syſtem, I had an eye upon ſuch principles as might work with conſidering men, for the belief of a Deity; and nothing can rejoice me more than to find it uſeful for that purpoſe. But if I have done the public any ſervice this way, it is due to nothing but induſtry and patient thought.

As to your firſt Query, it ſeems to me, that if the matter of our ſun and planets, and all the matter of the univerſe, were evenly ſcattered throughout all the heavens, and every particle had an innate gravity towards all the reſt, and the whole ſpace, throughout which this matter was ſcattered, was but finite; the matter on the outſide of this ſpace would by its gravity tend towards all the matter on the inſide, and by conſequence fall down into the middle of the whole ſpace, and there compoſe one great ſpherical maſs. But if the matter was evenly diſpoſed throughout an infinite ſpace, it could never convene into one maſs; but ſome of it would convene into one maſs and ſome into another,

ſo

LETTER FIRST.

ſo as to make an infinite number of great maſſes, ſcattered at great diſtances from one to another throughout all that infinite ſpace. And thus might the ſun and fixed ſtars be formed, ſuppoſing the matter were of a lucid nature. But how the matter ſhould divide itſelf into two ſorts; and that part of it, which is fit to compoſe a ſhining body, ſhould fall down into one maſs and make a ſun; and the reſt, which is fit to compoſe an opake body, ſhould coaleſce, not into one great body, like the ſhining matter, but into many little ones: Or if the ſun at firſt were an opake body like the planets, or the planets lucid bodies like the ſun, how he alone ſhould be changed into a ſhining body, whilſt all they continue opake; or all they be changed into opake ones, whilſt he remains unchanged; I do not think explicable by meer natural cauſes, but am forced to aſcribe it to the counſel and contrivance of a voluntary Agent.

The ſame power, whether natural or ſupernatural, which placed the ſun in the center of the ſix primary planets, placed *Saturn* in the center of the orbs of his five ſecondary planets; and *Jupiter* in the center of his four ſecondary planets; and the earth in the center of the moon's orb; and therefore, had this cauſe been a blind one without contrivance or deſign, the ſun would have been a body of the ſame kind with *Saturn*, *Jupiter*, and the earth; that is, without light or heat. Why there is one body in our ſyſtem qualified to give light and heat to all the reſt, I know no reaſon, but becauſe the author of the ſyſtem thought it convenient; and why there is but one body of this kind, I know no reaſon, but becauſe one was ſufficient to warm and enlighten all the reſt. For the *Carteſian* hypotheſis of ſuns loſing their light, and then turning into comets, and comets into planets, can have no place in my Syſtem, and is plainly erroneous: becauſe it is certain, that as often as they appear to us, they deſcend into the ſyſtem of our planets, lower than the orb of *Jupiter*, and ſometimes lower than the orbs of *Venus* and *Mercury*; and yet never ſtay here, but always return from the ſun with the ſame degrees of motion by which they approached him.

To your ſecond Query I anſwer, that the motions, which the planets now have, could not ſpring from any natural cauſe alone, but were impreſſed by an intelligent Agent. For ſince comets deſcend into the region of our planets, and here move all manner of ways, going ſometimes the ſame way with the planets, ſometimes the contrary way, and ſometimes in croſs ways, the planes inclined to the plane of the ecliptick, and at all kinds of angles, it is plain that there is no natural cauſe which could determine all the planets, both primary and ſecondary, to move the ſame way and in the ſame plane, without any conſiderable variation: this muſt have been the effect of counſel. Nor is there any natural cauſe which could give the planets thoſe juſt degrees of velocity, in proportion to their diſtances from the ſun, and other central bodies, which were requiſite to make them move in ſuch concentrick orbs about thoſe bodies. Had the planets been as ſwift as comets, in proportion to their diſtances from the ſun (as they would have been, had their motion been cauſed by their gravity, whereby the matter, at the firſt formation of the planets, might fall from the remoteſt regions towards the ſun) they would not move in concentrick orbs, but in ſuch eccentrick ones as the comets move in. Were all the planets as ſwift as *Mercury*, or as ſlow as *Saturn* or his ſatellites; or were their ſeveral velocities otherwiſe much greater or leſs than they are, as they might have been, had they aroſe from any other cauſe than their gravities; or had the diſtances from the centers, about which they move, been greater or leſs than they are with the ſame velocities; or had the quantity of matter in the ſun, or in *Saturn*, *Jupiter*, and the earth, and by conſequence their gravitating power been greater or leſs than it is; the primary planets could not have revolved about the ſun, nor the ſecondary ones about *Saturn*, *Jupiter*, and the Earth, in concentrick circles as they do, but would have moved in hyperbolas or parabolas, or in ellipſes very eccentrick. To make this ſyſtem, therefore, with all its motions, required a cauſe which underſtood, and compared together, the quantities of matter in the ſeveral bodies of the ſun and planets, and the gravitating powers reſulting from thence; the ſeveral diſtances of the primary planets from the ſun, and of the ſecondary

LETTER FIRST.

LETTER FIRST.

condary ones from *Saturn*, *Jupiter*, and the Earth; and the velocities, with which theſe planets could revolve about thoſe quantities of matter in the central bodies; and to compare and adjuſt all theſe things together in ſo great a variety of bodies, argues that cauſe to be not blind and fortuitous, but very well ſkilled in mechanicks and geometry.

To your third Query I anſwer, that it may be repreſented that the ſun may, by heating thoſe planets moſt which are neareſt to him, cauſe them to be better concocted, and more condenſed by that concoction. But when I conſider that our earth is much more heated in its bowels below the upper cruſt, by ſubterraneous fermentations of mineral bodies than by the ſun; I ſee not why the interior parts of *Jupiter* and *Saturn* might not be as much heated, concocted, and coagulated by thoſe fermentations as our earth is: and therefore this various denſity ſhould have ſome other cauſe than the various diſtances of the planets from the ſun. And I am confirmed in this opinion by conſidering, that the planets of *Jupiter* and *Saturn*, as they are rarer than the reſt, ſo they are vaſtly greater, and contain a far greater quantity of matter, and have many ſatellites about them; which qualifications ſurely aroſe not from their being placed at ſo great a diſtance from the ſun, but were rather the cauſe why the Creator placed them at great diſtance. For by their gravitating powers they diſturb one another's motions very ſenſibly, as I find by ſome late obſervations of Mr. *Flamſteed*; and had they been placed much nearer to the ſun and to one another, they would by the ſame powers have cauſed a conſiderable diſturbance in the whole ſyſtem.

To your fourth Query I anſwer, that, in the hypotheſis of *vortices*, the inclination of the axis of the earth might, in my opinion, be aſcribed to the ſituation of the earth's *vortex* before it was abſorbed by the neighbouring *vortices*, and the earth turned from a ſun to a comet; but this inclination ought to decreaſe conſtantly in compliance with the motion of the earth's *vortex*, whoſe axis is much leſs inclined to the ecliptick; as appears by the motion of the moon carried about therein. If the ſun by

his

his rays could carry about the planets, yet I do not ſee how he could thereby effect their diurnal motions. LETTER FIRST.

Laſtly, I ſee nothing extraordinary in the inclination of the earth's axis for proving a Deity; unleſs you will urge it as a contrivance for Winter and Summer, and for making the earth habitable towards the poles; and that the diurnal rotations of the ſun and planets, as they could hardly ariſe from any cauſe purely mechanical, ſo by being determined all the ſame way with the annual and menſtrual motions, they ſeem to make up that harmony in the ſyſtem, which, as I explained above, was the effect of choice, rather than chance.

There is yet another argument for a Deity, which I take to be a very ſtrong one; but till the principles on which it is grounded are better received, I think it more adviſeable to let it ſleep.

I am, &c.

Cambridge,
Dec. 10, 1692.

LETTER II.

SIR,

LETTER SECOND. I AGREE with you, that if matter, evenly diffuſed through a finite ſpace, not ſpherical, ſhould fall into a ſolid maſs, this maſs would affect the figure of the whole ſpace, provided it were not ſoft, like the old chaos, but ſo hard and ſolid from the beginning, that the weight of its protuberant parts could not make it yield to their preſſure. Yet by earthquakes looſening the parts of this ſolid, the protuberances might ſometimes ſink a little by their weight, and thereby the maſs might, by degrees, approach a ſpherical figure.

The reaſon why matter evenly ſcattered through a finite ſpace would convene in the midſt, you conceive the ſame with me; but that there ſhould be a central particle, ſo accurately placed in the middle, as to be always equally attracted on all ſides, and thereby continue without motion, ſeems to me a ſuppoſition fully as hard, as to make the ſharpeſt needle ſtand upright on its point upon a looking-glaſs. For if the very mathematical center of the central particle be not accurately in the very mathematical center of the attractive power of the whole maſs, the particle will not be attracted equally on all ſides. And much harder it is to ſuppoſe all the particles in an infinite ſpace ſhould be ſo accurately poiſed one among another, as to ſtand ſtill in a perfect equilibrium. For I reckon this as hard as to make, not one needle only, but an infinite number of them (ſo many as there are particles in an infinite ſpace) ſtand accurately poiſed upon their points. Yet I grant it poſſible, at leaſt by a Divine Power; and if they were once to be placed, I agree with you, that they would continue in that poſture without motion for ever, unleſs put into new motion by the ſame power. When therefore I ſaid, that matter, evenly ſpread through all ſpace, would convene by its gravity

gravity into one or more great maſſes, I underſtand it of matter not reſiſting in an accurate poiſe.

But you argue, in the next paragraph of your letter, that every particle of matter, in an infinite ſpace, has an infinite quantity of matter on all ſides, and by conſequence an infinite attraction every way, and therefore muſt reſt in equilibrio, becauſe all infinites are equal. Yet you ſuſpect a paralogiſm in this argument; and I conceive the paralogiſm lies in the poſition, that all infinites are equal. The generality of mankind conſider infinites no other ways than indefinitely; and in this ſenſe, they ſay all infinites are equal; though they would ſpeak more truly if they ſhould ſay, they are neither equal nor unequal, nor have any certain difference, or proportion one to another. In this ſenſe, therefore, no concluſions can be drawn from them, about the equality, proportions, or differences of things; and they that attempt to do it, uſually fall into paralogiſms. So when men argue againſt the infinite diviſibility of magnitude, by ſaying, that if an inch may be divided into an infinite number of parts, the ſum of thoſe parts will be an inch; and if a foot may be divided into an infinite number of parts, the ſum of thoſe parts muſt be a foot; and therefore ſince all infinites are equal, thoſe ſums muſt be equal, that is, an inch equal to a foot; the falſeneſs of the concluſion ſhews an error in the premiſes: and the error lies in the poſition, that all infinites are equal. There is therefore another way of conſidering infinites, uſed by mathematicians, and that is under certain definite reſtrictions and limitations, whereby infinites are determined to have certain differences or proportions to one another. Thus Dr. *Wallis* conſiders them in his *Arithmetica Infinitorum*; where, by the various proportions of infinite ſums, he gathers the various proportions of finite magnitudes: which way of arguing is generally allowed by mathematicians, and yet would not be good, were all infinites equal. According to the ſame way of conſidering infinites, a mathematician would tell you, that though there be an infinite number of infinite little parts in an inch, yet there is twelve times that number of ſuch parts in a foot; that is, the infinite number of thoſe parts in a foot is not equal to, but twelve

LETTER SECOND.

times bigger than the infinite number of them in an inch. And ſo a mathematician will tell you, that if a body ſtood in equilibrio between any two equal and contrary attracting infinite forces, and if to either of theſe forces you add any new finite attracting force; that new force, how little ſoever, will deſtroy their equilibrium, and put the body into the ſame motion into which it would put it, were thoſe two contrary equal forces but finite, or even none at all: ſo that in this caſe the two equal infinites, by the addition of a finite to either of them, become unequal in our ways of reckoning; and after theſe ways we muſt reckon, if from the conſiderations of infinites we would always draw true concluſions.

To the laſt part of your letter I anſwer, firſt, that if the earth (without the moon) were placed any where with its center in the *Orbis Magnus*, and ſtood ſtill there without any gravitation or projection, and there at once were infuſed into it, both a gravitating energy towards the ſun, and a tranſverſe impulſe of a juſt quantity moving it directly in a tangent to the *Orbis Magnus*; the compounds of this attraction and projection would, according to my notion, cauſe a circular revolution of the earth about the ſun. But the tranſverſe impulſe muſt be a juſt quantity; for if it be too big or too little, it will cauſe the earth to move in ſome other line. Secondly, I do not know any power in Nature which would cauſe this tranſverſe motion without the Divine arm. *Blondel* tells us ſomewhere in his book of Bombs, that *Plato* affirms, that the motion of the planets is ſuch, as if they had all of them been created by God in ſome region very remote from our ſyſtem, and let fall from thence towards the ſun, and ſo ſoon as they arrived at their ſeveral orbs, their motion of falling turned aſide into a tranſverſe one. And this is true, ſuppoſing the gravitating power of the ſun was double, at that moment of time in which they all arrive at their ſeveral orbs; but then the Divine power is here required in a double reſpect, namely, to turn the deſcending motions of the falling planets into a ſide motion, and at the ſame time to double the attractive power of the ſun. So then gravity may put the planets into motion, but without the Divine Power it could never put them into ſuch a circulating motion,

tion, as they have about the ſun; and therefore for this, as well as other reaſons, I am compelled to aſcribe the frame of this ſyſtem to an intelligent Agent. LETTER SECOND.

You ſometimes ſpeak of gravity as eſſential and inherent to matter. Pray do not aſcribe that notion to me; for the cauſe of gravity is what I do not pretend to know, and therefore would take more time to conſider of it.

I fear what I have ſaid of infinites will ſeem obſcure to you; but it is enough if you underſtand that infinites, when conſidered abſolutely without any reſtriction or limitation, are neither equal nor unequal, nor have any certain proportion one to another; and therefore the principle that all infinites are equal, is a precarious one.

I am, Sir, &c.

Trinity College,
Jan. 17, 1692-3.

LETTER III.

SIR,

Cambridge,
Feb. 25, 1692-3.

BECAUSE you deſire ſpeed, I will anſwer your letter with what brevity I can. In the ſix poſitions you lay down in the beginning of your letter, I agree with you. Your aſſuming the *Orbis Magnus* 7000 diameters of the earth wide, implies the ſun's horizontal parallax to be half a minute. *Flamſteed* and *Caſſini* have of late obſerved it to be about 10″, and thus the *Orbis Magnus* muſt be 21,000, or in a rounder number 20,000 diameters of the earth wide. Either computation I think will do well, and I think it not worth while to alter your numbers.

In the next part of your letter you lay down four other poſitions founded upon the ſix firſt. The firſt of theſe four ſeems very evident; ſuppoſing you take attraction ſo generally, as by it to underſtand any force, by which diſtant bodies endeavour to come together without mechanical impulſe. The ſecond ſeems not

LETTER THIRD.

not ſo clear. For it may be ſaid, that there might be other ſyſtems of worlds before the preſent ones, and others before thoſe, and ſo on to all paſt eternity; and by conſequence, that gravity may be co-eternal to matter, and have the ſame effect from all eternity as at preſent; unleſs you have ſomewhere proved, that old ſyſtems cannot gradually paſs into new ones; or that this ſyſtem had not its original from the exhaling matter of former decaying ſyſtems, but from a chaos of matter evenly diſperſed throughout all ſpace. For ſomething of this kind, I think, you ſay was the ſubject of your ſixth Sermon; and the growth of new ſyſtems out of old ones, without the mediation of a Divine Power, ſeems to me apparently abſurd.

The laſt clauſe of the ſecond poſition I like very well. It is inconceivable, that inanimate brute matter ſhould, without the mediation of ſomething elſe, which is not material, operate upon, and affect other matter without mutual contact; as it muſt do, if gravitation, in the ſenſe of *Epicurus*, be eſſential and inherent in it. And this is one reaſon, why I deſired you would not aſcribe innate gravity to me. That gravity ſhould be innate, inherent and eſſential to matter, ſo that one body may act upon another at a diſtance through a *vacuum*, without the mediation of any thing elſe, by and through which their action and force may be conveyed from one to another, is to me ſo great an abſurdity, that I believe no man who has in philoſophical matters a competent faculty of thinking, can ever fall into it. Gravity muſt be cauſed by an agent acting conſtantly according to certain laws; but whether this agent be material or immaterial, I have left to the conſideration of my readers.

Your fourth aſſertion, that the world could not be formed by innate gravity alone, you confirm by three arguments. But in your firſt argument you ſeem to make a *Petitio Principii*; for whereas many ancient philoſophers and others, as well Theiſts as Atheiſts, have all allowed, that there may be worlds and parcels of matter innumerable or infinite, you deny this, by repreſenting it as abſurd, as that there ſhould be poſitively an infinite arithmetical ſum or number, which is a contradiction *in terminis*; but you do not prove it as abſurd. Neither do you prove, that what

what men mean by an infinite ſum or number, is a contradiction in nature; for a contradiction *in terminis* implies no more than an impropriety of ſpeech. Thoſe things, which men underſtand by improper and contradictious phraſes, may be ſometimes really in Nature without any contradiction at all: a ſilver irkhorn, a paper lanthorn, an iron whetſtone, are abſurd phraſes; yet the things ſignified thereby are really in Nature. If any man ſhould ſay, that a number and a ſum, to ſpeak properly, is that which may be numbered and ſummed, but things infinite are numberleſs, or, as we uſually ſpeak, innumerable and ſumleſs, or inſummable, and therefore ought not to be called a number or ſum; he will ſpeak properly enough, and your argument againſt him will, I fear, loſe its force. And yet if any man ſhall take the words, number and ſum, in a larger ſenſe, ſo as to underſtand thereby things, which in the proper way of ſpeaking are numberleſs and ſumleſs (as you ſeem to do when you allow an infinite number of points in a line) I could readily allow him the uſe of the contradictious phraſes of innumerable number, or ſumleſs ſum, without inferring from thence any abſurdity in the thing he means by thoſe phraſes. However, if by this, or any other argument, you have proved the finiteneſs of the univerſe, it follows, that all matter would fall down from the outſides, and convene in the middle. Yet the matter in falling might concrete into many round maſſes, like the bodies of the planets; and theſe, by attracting one another, might acquire an obliquity of deſcent, by means of which they might fall, not upon the great central body, but upon the ſide of it, and fetch a compaſs about; and then aſcend again by the ſame ſteps and degrees of motion and velocity with which they deſcended before, much after the manner that the comets revolve about the ſun; but a circular motion in concentrick orbs about the ſun, they could never acquire by gravity alone. LETTER THIRD.

And though all the matter were divided at firſt into ſeveral ſyſtems, and every ſyſtem by a Divine Power conſtituted like ours; yet would the outſide ſyſtems deſcend towards the middlemoſt; ſo that this frame of things could not always ſubſiſt without a

LETTER THIRD.

Divine Power to conſerve it, which is the ſecond argument; and to your third I fully aſſent.

As for the paſſage of *Plato*, there is no common place from whence all the planets being let fall, and deſcending with uniform and equal gravities (as *Galileo* ſuppoſes) would at their arrival to their ſeveral orbs acquire their ſeveral velocities, with which they now revolve in them. If we ſuppoſe the gravity of all the planets towards the ſun to be of ſuch a quantity as it really is, and that the motions of the planets are turned upwards, every planet will aſcend to twice its height from the ſun. *Saturn* will aſcend till he be twice as high from the ſun as he is at preſent, and no higher; *Jupiter* will aſcend as high again as at preſent, that is, a little above the orb of *Saturn*; *Mercury* will aſcend to twice his preſent height, that is, to the orb of *Venus*; and ſo of the reſt; and then by falling down again from the places to which they aſcended, they will arrive again at their ſeveral orbs with the ſame velocities they had at firſt, and with which they now revolve.

But if ſo ſoon as their motions by which they revolve are turned upwards, the gravitating power of the ſun, by which their aſcent is perpetually retarded, be diminiſhed by one half, they will now aſcend perpetually, and all of them at all equal diſtances from the ſun will be equally ſwift. *Mercury*, when he arrives at the orb of *Venus*, will be as ſwift as *Venus*; and he and *Venus*, when they arrive at the orb of the Earth, will be as ſwift as the Earth; and ſo of the reſt. If they begin all of them to aſcend at once, and aſcend in the ſame line, they will conſtantly, in aſcending, become nearer and nearer together, and their motions will conſtantly approach to an equality, and become at length ſlower than any motion aſſignable. Suppoſe, therefore, that they aſcended till they were almoſt contiguous, and their motions inconſiderably little, and that all their motions were at the ſame moment of time turned back again; or, which comes almoſt to the ſame thing, that they were only deprived of their motions, and let fall at that time, they would all at once arrive at their ſeveral orbs, each with the velocity it had at firſt; and if their motions were then turned ſideways, and at the ſame time the

the gravitating power of the ſun doubled, that it might be ſtrong enough to retain them in their orbs, they would revolve in them as before their aſcent. But if the gravitating power of the ſun was not doubled, they would go away from their orbs into the higheſt heavens in parabolical lines. Theſe things follow from my *Princip. Math. Lib. I. Prop.* XXXIII, XXXIX, XXXV, XXXVI. LETTER THIRD.

I thank you very kindly for your deſigned preſent, and reſt yours, &c.

LETTER IV.

SIR, Cambridge, Feb 11, 1693.

THE hypotheſis of deriving the frame of the world, by mechanical principles, from matter evenly ſpread through the heavens, being inconſiſtent with my ſyſtem, I had conſidered it very little before your letters put me upon it, and therefore trouble you with a line or two more about it, if this comes not too late for your uſe. LETTER FOURTH.

In my former I repreſented, that the diurnal rotations of the planets could not be derived from gravity, but required a Divine Arm to impreſs them. And though gravity might give the planets a motion of deſcent towards the ſun, either directly or with ſome little obliquity, yet the tranſverſe motions by which they revolve in their ſeveral orbs, required the Divine Arm to impreſs them according to the tangents of their orbs. I would now add, that the hypotheſis of matter's being at firſt evenly ſpread through the heavens, is, in my opinion, inconſiſtent with the hypotheſis of innate gravity, without a ſupernatural power to reconcile them; and therefore it infers a Deity. For if there be innate gravity, it is impoſſible now for the matter of the Earth and all the planets and ſtars to fly up from them, and become evenly ſpread throughout all the heavens, without a ſupernatural power; and certainly that which can never be hereafter without a ſupernatural power, could never be heretofore without the ſame power.

You queried whether matter, evenly ſpread throughout a finite ſpace, of ſome other figure than ſpherical, would not, in falling down towards a central body, cauſe that body to be of the ſame figure with the whole ſpace; and I anſwered, yes. But in my anſwer it is to be ſuppoſed that the matter deſcends directly downwards to that body, and that that body has no diurnal rotation.

This, Sir, is all I would add to my former letters.

I am yours, &c.

COMMERCIUM
EPISTOLICUM
DE
VARIA RE MATHEMATICA,
INTER
CELEBERRIMOS PRÆSENTIS SECULI MATHEMATICOS.
VIZ.

Iſaacum Newtonum Equitem Auratum.
D. Iſaacum Barrow.
D. Jacobum Gregorium.
D. Johannem Walliſium.
D. J. Keillium.

D. J. Collinium.
D. Gulielmum Leibnitium.
D. Henricum Oldenbourgum.
D. Franciſcum Sluſium.

ET ALIOS.

JUSSU SOCIETATIS REGIÆ IN LUCEM EDITUM

ET JAM

Unà cum Recenſione præmiſſâ inſignis Controverſiæ inter Leibnitium & Keillium de primo Inventore Methodi Fluxionum; & Judicio primarii, ut ferebatur, Mathematici ſubjuncto, iterum impreſſum. A. D. 1725.

AD

LECTOREM.

CUM primum Commercium Epiſtolicum lucem vidit, D. Leibnitius *Viennæ agens, ut librum ſine reſponſo dimitteret, prætendit per biennium ſe eundem non vidiſſe, ſed ad judicium primarii Mathematici, & harum rerum peritiſſimi, & à partium ſtudio alieni ſe provocáſſe. Et ſententiam ejus,* 7 Jun. 1713, *datam, ſchedulæ volanti* 29 Julii *datæ incluſam, per orbem ſparſit, ſine nomine vel judicis, vel Impreſſoris, vel Urbis in quâ impreſſa fuit. Et ſub finem anni* 1715, *in Literis quas ad Abbatem de Comitibus tunc* Londini *agentem ſcripſit, confugit ad Quæſtiones novas de Qualitatibus occultis, gravitate univerſali, Miraculis, Organis & Senſorio Dei, ſpatio, Tempore, Vacuo, Atomis, Perfectione mundi, & Intelligentiâ ſupramundanâ; & Problema ex Actis Eruditorum deſumptum propoſuit ab Analyſtis Anglis ſolvendum. Quæ omnia ad rem nil ſpectant.*

Sed & Conſeſſum à Regiâ Societate conſtitutum, qui Commercium ex antiquis monumentis ediderant, accuſavit, quaſi partibus ſtuduiſſent; &, literas antiquas edendo, omiſiſſent omnia quæ vel pro ipſo, vel contra Newtonum *facerent. Et ut hoc probaret, ſcripſit is in primâ ſuâ ad Abbatem epiſtolâ, quòd in ſecundo ſuo in Angliam itinere,* Collinius *oſtenderit ipſi partem Commercii ſui; in quâ* Newtonus *agnoſcebat ignorantiam ſuam in pluribus, dicebatque*

(inter alia) quòd nihil invenisset circa dimensiones Curvilinearum, quæ celebrantur, præter dimensionem Cissoidis : sed Consessus hoc totum suppressit. Et Newtonus, *in Epistolâ suâ ad dictum Abbatem* 26 *Feb.* $17\frac{15}{16}$ *datâ, respondit, hoc non fuisse omissum, sed extare in Epistolâ suâ ad* Oldenburgum 24 Octob. 1676 *missâ, & impressum fuisse in Commercio Epistolico pag.* 74, *lin.* 10, 11. *Et subinde* Leibnitius, *in proximâ suâ ad Abbatem illum Epistolâ* Apr. 9, 1719 *datâ, agnovit se* errâsse. Sed, *inquit*, exemplum dabo aliud. *Newtonus* in unâ Epistolarum ejus ad *Collinium* agnovit, se non posse invenire magnitudinem sectionum secundarum (vel segmentorum secundorum) sphæroidum & corporum similium: sed Consessus hunc locum, vel hanc Epistolam, minimè edidit. Newtonus *autem, in Observationibus quas in hanc* Leibnitii *Epistolam scripsit, respondit : si Consessus hoc omisisset, rectè omnino omissum fuisse, cùm hujusmodi cavillationes ad Quæstionem, de quâ agitur, nil spectent : sed Consessum hoc minimè omisisse.* Collinius, *in Epistolâ ad D.* Gregorium 24 Decem. 1670, *ut & in alterâ ad D.* Bertet 1671 (*utrisque impressis in Commercio, p.* 24, 26) *scripsit quòd Methodus* Newtoni *se extenderet ad secunda Solidorum segmenta, quæ per rotationem generantur. Et* Oldenburgius *idem scripsit ad* Leibnitium *ipsum* 8 Dec. 1674, *ut videre est in Commercio, pag.* 39. Leibnitius *igitur iterum erravit. Nam & in Transactionibus Philosophicis pro* Jan. & Feb. 1718, *pag.* 925, *dicitur quòd Abbas de Comitibus* per horas aliquot inspexit Epistolas antiquas, & Libros Epistolarum in Archivis R. Societatis asservatos, ut aliquid inveniret, quod vel pro *Leibnitio*, vel contra *Newtonum* faceret, & in Commercio Epistolico omissum fuisset; sed ejus generis nihil invenire potuit.

Insuper D. Leibnitius, *ut Commercium Epistolicum sine responso dimitteret, in primâ suâ ad Abbatem de Comitibus Epistolâ dixit, eos, qui contra ipsum scripsissent (id est Consessum à Regia Societate constitutum) candorem ejus aggressos esse per interpretationes duras & malè fundatas, & voluptatem non habituros esse videndi responsa ejus ad pusillas rationes eorum, qui iis tam malè utuntur. Interpretationes illæ nullius quidem sunt auctoritatis, nisi quam ab Epistolis derivant; sed malè fundatas esse* Leibnitius *nunquam ostendit.*

Subinde

Subinde vero Newtonus, *qui ægre adductus eſt ut ſcriberet, in primâ ſuâ ad Abbatem Epiſtolâ* 26 Feb. 171$\frac{5}{6}$ *ita reſcripſit.* D. *Leibnitius* hactenus reſpondere recuſavit, bene intelligens impoſſibile eſſe res factas refutare. Silentium ſuum hâc in re excuſat, prætexens ſe librum nondum vidiſſe, & otium illi non eſſe ad examinandum, ſed ſe orâſſe Mathematicum celebrem, ut hoc negotium in ſe ſuſciperet.—Utitur & novo prætextu ne reſpondeat, dicens quòd Angli voluptatem non habebunt videndi reſponſa ejus ad puſillas eorum rationes, & proponens diſputationes novas Philoſophicas ineundas & problemata ſolvenda: quæ duo ad rem nil ſpectant. *D.* Leibnitius *autem, in proximâ ſuâ ad Abbatem Epiſtolâ* 9 Apr. 1716 *datâ, pergebat ſe excuſare ne reſpondeat.* Ut operi, *inquit*, contra me edito ſigillatim reſpondeam, opus erit alio opere non minore quàm hoc eſt; percurrendum erit corpus magnum minutorum ante annos 30 vel 40 præteritorum, quorum perparvum reminiſcor; examinandæ erunt veteres epiſtolæ, quarum plures ſunt perditæ; præterquam quòd maximâ ex parte non conſervavi minuta mearum, & reliquæ ſepultæ ſunt in maximo chartarum acervo, quem non poſſum ſine tempore & patientiâ diſcutere. Sed otium mihi minimè ſuppetit aliis negotiis alterius prorſus generis occupato. *Et paulo poſt:* literas truncare non debuerunt. Nam parvum eſt inter chartas meas, vel cujus minuta mihi relinquuntur. Sic omnibus perpenſis, videns tantas malignitatis & fallaciæ notas, credidi indignum eſſe me ingredi diſcuſſionem cum hominum genere, qui ſe tam malè gerunt. Sentio quòd in iis refutandis difficile fuerit ab opprobriis & expreſſionibus aſperis abſtinere, talibus quas eorum facta merentur; & non cupio hujuſmodi ſpectaculum exhibere publico, in animo habens tempus meum melius impendere, quod mihi pretioſum eſſe debet, & contemnens judicium eorum qui ſuper tali opere ſententiam contra me pronunciare vellent; præſertim cum Societas Regia hoc facere noluit. *Hæc* Leibnitius. *Quæſtionem primam deſerit rixando, & Quæſtiones novas proponit.*

Attamen poſt ejus mortem, quæ contigit proximo Menſe Novembris, *in Elogio ejus quod in Actis Eruditorum pro Menſe* Julio *anni* 1717 *impreſſum fuit, amici ejus ſcripſerunt* eum Commercio Epiſtolico Anglorum quoddam ſuum idemque amplius opponere decreviſſe:

creviſſe : & paucis ante obitum diebus Cl. *Wolfio* ſignificâſſe ſe Anglos, famam ipſius laceſſentes, reipſâ refutaturum. Quamprimum enim à laboribus hiſtoricis vacaturus ſit, daturum ſe aliquid in Analyſi prorſus inexpectatum, & cum inventis quæ hactenus in publicum proſtant, ſive *Newtoni*, ſive aliorum, nihil quicquam affine habens. *Hæc illi. Verùm ex jam dictis patet, illum non aliud habuiſſe Commercium Epiſtolicum, quod ederet. Et inventum novum, his nihil affine habens, ad rem nihil ſpectat. Miſſis ægrorum ſomniis, Quæſtio tota ad epiſtolas antiquas referri debet.*

Initio ſecundæ ad Abbatem de Comitibus Epiſtolæ, D. Leibnitius *primam* Newtoni *Epiſtolam vocavit* ſpeciem chartæ provocatoriæ ex parte *Newtoni: dein addidit*; in arenam deſcendere nolui contra ejus milites emiſſarios, ſive intelligas accuſatorem ſupra fundamentum Commercii Epiſtolici, ſive præfationem ſpectes acrimoniæ plenam, quam alius quidam novæ Principiorum editioni præmiſit : ſed cùm is per ſe jam lubens apparebit, paratus ſum ipſi ſatisfactionem dare. *Et* Newtonus *reſpondit*, D. *Leibnitium* literas & chartas antiquas ſeponere, & ad quæſtiones circa philoſophiam & res alias confugere. Et magnum illum Mathematicum, cui ſine nomine, ut judici, epiſtolam 7 *Jun.* 1713 datam attribuerat, jam velo ſublato ut militem in hâc rixâ pro ſe inducere, mathematicos in Anglia provocantem ad problemata ſolvenda ; quaſi duellum [*cum* Leibnitio *ſcilicet*] vel forte prælium cum exercitu diſcipulorum ejus [*quos jactat*] methodus eſſet magis idonea ad veritatem dirimendam, quàm diſcuſſio veterum & authenticorum ſcriptorum, & Matheſis factis heroicis, vice rationum ac demonſtrationum, abhinc implenda eſſet. *Hic rationes ac demonſtrationes alludunt ad argumenta è ſcriptis veteribus deſumpta, & facta heroica ad contentiones philoſophicas & problematicas ad rem nil ſpectantes, ad quas D.* Leibnitius *à prioribus aufugit.*

Quæ novæ Principiorum editioni præmiſſa ſunt, Newtonus *non vidit, antequam Liber in lucem prodiit. Quæ de Quæſtionibus Philoſophicis diſputata ſunt, D.* Des Maizeaux *collegit, & in lucem edidit. Solutiones Problematum maximâ ex parte lucem viderunt in Actis Eruditorum. Hæc omnia ad rem nil ſpectant. Commercii Epiſtolici exempla tantùm pauca impreſſa fuerunt, & ad Mathematicos miſſa qui de his rebus judicare poſſent, neque proſtant venalia. Ideoque*

que hunc Librum, ut & ejus Recenſionem quæ in Tranſactionibus Philoſophicis ac Diario Literario, anno 1715, *anno & ſeptem vel octo menſibus ante obitum D.* Leibnitii, *impreſſa fuit, iterum imprimere viſum eſt; ut hiſtoria vera ex antiquis monumentis deducta, miſſis diſputationibus quæ ad rem nil ſpectant, ad poſteros perveniat; & ſic finis imponatur huic controverſiæ. Nam D.* Leibnitius *à Quæſtione deſciſcens emortuus eſt, & judicium poſteris relinquitur.*

Denique Judicium primarii Mathematici ſubjunctum eſt, unà cum notis; quibus pateat, eidem in Recenſione prædictâ, vivente Leibnitio, *reſponſum eſſe, & ſcopum ejus fuiſſe tantùm, ut Commercium Epiſtolicum ſine reſponſo dimitteretur.*

RECENSIO LIBRI

Qui inſcriptus eſt *Commercium Epiſtolicum Collinii & aliorum de Analyſi Promotâ*, & publicatus eſt juſſu Regiæ Societatis Londinenſis, circa controverſiam inter D. Leibnitz & D. Keill, de primo inventore *Methodi Fluxionum*, ſive, ut nonnulli appellant, *Methodi Differentialis:* Anglicè primùm edita in Actis Regiæ Societatis, A. D. 1715, & Gallicè eodem anno in Diario Literario Tom. VII. nunc ex Anglico in Latinum verſa.

CUM variæ Relationes apud exteros de Commercio hoc publicatæ ſint, mutilæ omnes & imperfectæ; viſum eſt ut plenior hæc, quæ ſequitur, Recenſio in publicum edatur.

Commercium hoc contextum eſt ex variis epiſtolis chartiſque, in archivis Regiæ Societatis repoſitis; quæ ſingulæ hîc ſuo ordine ac ſerie collocantur, & vel ex latinis fideliter tranſcriptæ ſunt, vel ex anglicis fideliter in latinum tranſlatæ: numeroſo Conſeſſu à Regiâ Societate deputato, ut & literæ originales inſpicerentur, & earum exemplaria examinarentur. Cæterùm hæc, de quâ agitur, eſt methodus generalis reſolvendi finitas æquationes in infinitas, & applicandi æquationes illas, tam finitas quàm infinitas, ad ſolutionem problematum, per methodum *Fluxionum* & *Momentorum*. Primò autem differemus de eâ Methodi parte, quæ conſiſtit in reſolvendo finitas æquationes in infinitas, & eâ ratione quadrando figuras curvilineas. Per infinitas æquationes intelliguntur illæ, quæ involvunt ſeriem terminorum convergentium, & ad veritatem propiùs propiúſque accedentium in infinitum; ita ut poſtremò à veritate diſtent minùs ullâ datâ quantitate; &, ſi in infinitum continuentur, nullam omnino differentiam relinquant.

Walliſius

Wallisius in opere suo arithmetico, publicato A. D. 1657. Cap. 33. Prop. LXVIII. reduxit refractionem $\frac{A}{1-R}$, per perpetuam divisionem, in seriem $A+AR+AR^2+AR^3+AR^4+$ &c.

Vicecomes Brounker quadravit hyperbolam per hanc seriem $\frac{1}{1x}+\frac{1}{3x^4}+\frac{1}{5x^6}+\frac{1}{7x^8}+$ &c. hoc est per hanc $1-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+\frac{1}{5}-\frac{1}{6}+\frac{1}{7}-\frac{1}{8}+$ &c. conjungendo singulos binos terminos in unum. Et hæc quadratura publicata est in Actis Regiæ Societatis, mense Aprili 1668.

Paulo pòst Dominus Mercator evulgavit demonstrationem hujus quadraturæ per divisionem Domini Wallisii; & deinceps haud multo pòst Jacobus Gregorius geometricam ejusdem demonstrationem in lucem edidit. Hi libelli, paucis postquàm editi sunt mensibus, Cantabrigiam missi sunt ad Dominum Barrovium per Dominum Johannem Collins; & per Barrovium traditi Isaaco Newtono tunc Cantabrigiæ degenti, utpote Collegii S. Trinitatis Socio, (nunc autem Londini Equiti Aurato) mense Junio 1669. Hâc occasione, Barrovius vicissim Collinio misit tractatum Newtoni, qui inscribebatur *Analysis per æquationes numero terminorum infinitas.* Commerc. Epist. N° I. Is tractatus in Commercio Epistolico agmen ducit, continetque universalem methodum id in omnibus figuris faciendi, quod vicecomes Brounker & Mercator in solâ hyperbolâ fecerant. Porro Mercator, per annos sexdecim adhuc superstes, nihil tentavit aut progressus est ultra solam illam hyperbolæ quadraturam. Illa verò Newtoni per omnes figuras progressio satis ostendit, nihil eum in eâ re Mercatoris operâ aut ope indiguisse. Ne tamen litiget quisquam, aut cavilletur; concedit Newtonus & Brounkerum inveniisse, & Mercatorem demonstrâsse, seriem illam pro hyperbolâ quadrandâ, annos prius aliquot quàm in publicum ederent; & proinde prius quàm Newtonus generalem suam methodum inveniisset.

De tractatu isto qui inscribitur *Analysis*, &c. Newtonus in epistolâ ad Oldenburgium missâ, datâque 24 *Octob.* 1676, hæc verba habet, quæ sequuntur: Ib. N° LVII.

‘ *Eo ipso tempore quo Mercatoris Logarith-*
‘ *motechnia prodiit, communicatum est per amicum D. Barrow*
‘ *(tunc Matheseos professorem Cantab.) cum D. Collinio compendi-*
‘ *um quoddam harum serierum, in quo significaveram areas & lon-*
‘ *gitudines Curvarum omnium, & Solidorum superficies & contenta*

'*ex datis rectis*; &, *vice versâ*, *ex his datis rectas determinari posse*; '& *methodum indicatam illustraveram diversis seriebus*.' Hujus porro serierum compendii certiores fecit Collinius Jacobum Gregorium Scotum, Dominos Bertet & Vernon apud Gallos, Alphonsum Borellum Italum, Dominos Strode, Townsend, Oldenburg, Dary, aliosque apud Anglos, variis epistolis datis ann. 1669, 1670, 1671 & 1672, ut ipsæ epistolæ adhuc testantur. Ipse præterea Oldenburgius communicavit eandem *Analysin* cum D. Francisco Slusio, Leodii tum agente, & ex eâ aliquot ῥήσεις citavit; literis datis 14 *Sept.* 1669, & in librum Regiæ Soc. epistolarem transcriptis. Porro Collinius in epistolâ ad Jac. Gregorium, 25 *Novemb.* 1669, sic de methodo in *Analysi* illâ contentâ loquitur.

Ib. N° XIV, XIX, XXI, XXII, XXIII, XXIV.

Ib. N° XIII.

Ib. N° XIV.

'Barrovius provinciam suam publicè prælegendi remisit cui-'dam nomine Newtono Cantabrigiensi; cujus, tanquam viri acu-'tissimo ingenio præditi, in præfatione Prælectionum Opticarum 'meminit. Quippe antequam ederetur Mercatoris Logarithmo-'technia, eandem methodum adinvenerat, eamque ad omnes 'Curvas generaliter, & ad circulum diversimodè applicârat.' Literis verò ad D. Davidem Gregorium datis 11 *Aug.* 1676, his verbis de eâ loquitur: 'Paucos post menses quàm editi sunt hi libri (viz. 'Mercatoris Logarithmotechnia, & Exercitationes Geometricæ Gre-'gorii) missi sunt ad Barrovium Cantabrigiæ. Ille autem re-'sponsum dedit, hanc infinitarum serierum doctrinam à Newtono 'biennium ante, excogitatam fuisse, quàm ederetur Mercatoris Lo-'garithmotechnia, & generaliter omnibus figuris applicatam; si-'mulque transmisit D. Newtoni opus manuscriptum.' Horum autem librorum posterior prodiit circa finem anni 1668; Barrovius verò dictum serierum compendium Collinio misit, Julio insequente; ut ex tribus ejus epistolis constat. Collinius porro, in literis ad D. Strode 26 Julii 1672, sic de eo compendio scribit: 'Exemplar ejus (Logarithmotechniæ) misi Barrovio Cantabrigiam, 'qui quasdam Newtoni chartas extemplo remisit; è quibus & 'aliis, quæ priùs ab auctore cum Barrovio communicatæ fuerant, 'patet illam methodum à dicto Newtono aliquot annis antea exco-'gitatam, & modo universali applicatam fuisse. Ita ut ejus ope 'in quâvis figurâ curvilineâ propositâ, quæ unâ vel pluribus pro-'prietatibus definitur, quadratura vel area dictæ figuræ, accurata

Ib. N° XLVII.

Ib. N° I.

Ib. N° XXIV.

'si

‘ſi poſſibile ſit, ſin minus infinitè vero propinqua, evolutio vel ‘longitudo lineæ curvæ, centrum gravitatis figuræ, Solida ejus ‘rotatione genita, & eorum ſuperficies, ſine ullâ radicum extrac- ‘tione obtineri queant. Poſtquam intellexerat D. Gregorius hanc ‘methodum, à D. Mercatore in Logarithmotechniâ uſurpatam, & ‘hyperbolæ quadrandæ adhibitam, quamque adauxerat ipſe Gre- ‘gorius, jam univerſalem redditam eſſe, omnibuſque figuris ap- ‘plicatam, acri ſtudio eandem acquiſivit, multumque in eâ eno- ‘dandâ deſudavit. Uterque D. Newtonus & Gregorius in animo ‘habent hanc methodum exornare: D. Gregorius autem D. New- ‘tonum primum ejus inventorem anticipare haud integrum ducit.’ In aliâ verò epiſtolâ, ad Oldenburgium ſcriptâ & cum D. Leibnitio communicandâ, datâque 14 *Jun.* 1676, hæc memorat Collinius: Ib. N° XLV. ‘Hujus autem methodi ea eſt præſtantia; ut, cùm tam latè pa- ‘teat, ad nullam hæreat difficultatem. Gregorium autem alioſ- ‘que in eâ fuiſſe opinione arbitror, ut quicquid uſpiam antea de ‘hâc re innotuit, quaſi dubia diluculi lux fuit, ſi cum meridianâ ‘claritate conferatur.’

Porro hic Newtoni tractatus primùm typis editus eſt à D. Guli- A. D. 1710. elmo Jones, qui apographum ejus repperit in ſcriniis Collinii, ip- ſius manu ſcriptum; & poſtea cum originali contulit, à D. New- tono mutuato. Continet autem prædictam generalem methodum Analyſeos, monſtrantem quomodo reſolvendæ ſunt finitæ æqua- tiones in infinitas; utque, per methodum momentorum, applican- dæ ſunt æquationes, tam finitæ quàm infinitæ, ad omnium proble- matum ſolutionem. Incipit verò, ubi finem fecit Walliſius, & methodum quadraturarum ſuper tres regulas ſtruit.

Walliſius anno 1655, *Arithmeticam* ſuam *Infinitorum* in lucem dedit; per cujus libri Prop. LIX. ſi abſciſſa cujuſvis curvilinearis figuræ vocetur x, & *n* atque *m* ſint numeri, & ordinatæ ad rectos angulos erectæ ſint $x^{\frac{m}{n}}$; area figuræ erit $\frac{n}{m+n} x^{\frac{m+n}{n}}$. Atque hoc Ib. N° II. aſſumitur à D. Newtono, tamquam prima regula, ſuper quam fundat ſuam Curvarum quadraturam. Walliſius autem propoſi- tionem hanc demonſtravit gradatim, per multas particulares pro- poſitiones; tandemque omnes in unam collegit per tabulam ca- ſuum. Newtonus verò omnes caſus in unum reduxit, per digni-

tatem

tatem cum indefinito indice: & ſub extremo Compendii, ſemel ſimulque demonſtravit per methodum ſuam momentorum; primuſque indefinitos dignitatum indices in operationes analyſeos introduxit.

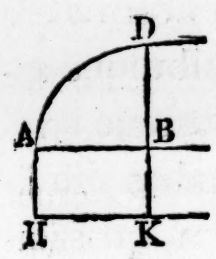
Cæterum per CVIII Propoſitionem *Arithmeticæ Infinitorum* Walliſii, perque plures alias propoſitiones quæ ſequuntur; ſi Ordinata compoſita fuerit ex duabus vel pluribus ordinatis cum ſignis ſuis + & − acceptis, Area compoſita erit ex duabus vel pluribus areis cum ſignis ſuis + & − acceptis reſpectivè. Atque hoc
Ib. N° III. à D. Newtono aſſumitur, tamquam regula ſecunda, ſuper quam inſtituit ſuam quadraturarum methodum.

Ib. N° IV. Tertia verò regula eſt, ut reducantur fractiones & radicales, & affectæ radices æquationum in ſeries convergentes, cùm quadratura non aliter ſuccedat: & ut per regulas primam ac ſecundam quadrentur figuræ, quarum Ordinatæ ſunt ſinguli termini ſerierum.
Ib. N° XLVIII. Newtonus, in epiſtolâ ad Oldenburgium ſcriptâ 13 *Jun.* 1676, & Leibnitio tranſmiſſâ, modum docuit reducendi quamlibet dignitatem cujuſlibet binominalis in ſeriem convergentem; & per eam ſeriem quadrandi Curvam, cujus Ordinata eſt illa dignitas. Et à D. Leibnitio rogatus, ut fontem hujus theorematis explicare
Ib. N° LV. vellet, reſcripſit per epiſtolam datam 24 *Octob.* 1676; ſe paulo ante peſtem, quæ Londini graſſabatur anno 1665, cùm legeret Arithmeticam Infinitorum Walliſii, cogitaretque de interpolandâ ſerie x, $x-\frac{1}{3}x^3$, $x-\frac{2}{3}x^3+\frac{1}{5}x^5$, $x-\frac{3}{3}x^3+\frac{3}{5}x^5-\frac{1}{7}x^7$, &c. inveniſſe aream circuli eſſe $x-\frac{1}{3}x^3-\frac{1}{5}x^5-\frac{1}{7}x^7-\frac{1}{9}x^9$ − &c. & perſequendo methodum interpolationis, ſe prædictum theorema excogitâſſe; atque ejus ope reductionem fractionum & ſurdarum in ſeries convergentes, per diviſionem & radicum extractionem, inveniſſe; ac tum ad affectarum radicum extractionem perrexiſſe. Atquæ hæ
Ib. N° IV. reductiones regula ſunt tertia.

Cùm in hoc ſerierum compendio trinas has regulas explicuiſſet Newtonus, variiſque exemplis eas illuſtrâſſet; deſignavit is ideam deducendi aream ex ordinatâ, conſiderando aream tamquam
Ib. N° X. quantitatem naſcentem, & augeſcentem ſive creſcentem per fluxionem continuam, in proportione longitudinis ordinatæ, & ſupponendo abſciſſam uniformiter creſcere in proportione ad tempus.

Atque

Atque ex momentis temporis, nomen *Momentorum* indidit momentaneis augmentis, ſive partibus areæ atque abſciſſæ, infinitè parvis, quæ in momentis temporis generantur. Momentum lineæ punctum vocavit, ex mente Cavallerii; quamvis non ſit punctum geometricum, ſed lineola infinitè brevis: momentum autem areæ vel ſuperficiei vocavit lineam, ſecundum eundem Cavallerium; licet non ſit linea geometrica, ſed ſuperficies latitudine infinitè exili. Cúmque Ordinatam conſideraret tanquam momentum areæ, eo nomine intellexit rectangulos ſub geometricâ Ordinatâ & momento abſciſſæ; licet illud momentum non ſemper exprimatur. *Sit* ABD, inquit, *Curva quævis*, & AHKB



rectangulum, cujus latus AH, *vel* KB, *eſt unitas. Et cogita rectam* DBK, *uniformiter ab* AH, *motam areas* ABD & AK *deſcribere*: & *quòd* [recta] BK (1) *ſit momentum quo* [area] AK (*x*), & [recta] BD (*y*) *momentum quo* [curvilinea] ABD *gradatim augetur*: & *quòd ex momento* BD, *perpetim dato, poſſis per præcedentes* [tres] *regulas aream* ABD, *ipſo deſcriptam, inveſtigare*; *ſive cum areâ* AK (*x*), *momento* I *deſcriptâ, conferre*. Hæc Newtoni idea eſt operationis in curvis quadrandis: quoque modo hanc ad alia problemata applicet, in verbis proxime ſequentibus monſtrat: *jam*, inquit, *quâ ratione ſuperficies* ABD, *ex momento ſuo perpetim dato, per præcedentes* [tres] *regulas elicitur, eâdem quælibet alia quantitas ex momento ſuo ſic dato elicietur. Exemplo res fiet clarior.* Cæterùm poſt aliquot exemplа, methodum addit regreſſionis ab areâ, arcu, ſolidove contento ad abſciſſam; docetque ut eadem methodus extendat ſe ad Curvas mechanicas, determinando earum ordinatas, tangentes, areas, longitudines, &c. Utque aſſumendo quamvis æquationem exprimentem relationem inter aream abſciſſamque Curvæ, per hanc methodum invenias Ordinatam. Atque hoc eſt fundamentum methodi fluxionum & momentorum, quod Newtonus, in literis datis 24 *Octob.* 1676, hâc ſententiâ comprehendit: *Datâ æquatione quotcunque fluentes quantitates involvente, invenire fluxiones*; & *vice verſâ*. Ib. N° X, XII.

In hoc *Compendio* uniformem fluxionem temporis, vel cujuſvis exponentis temporis, per unitatem repreſentat Newtonus; momentum autem temporis, vel exponentis ſui, per literam *o*; fluxiones Ib. N° X, XII.

verò

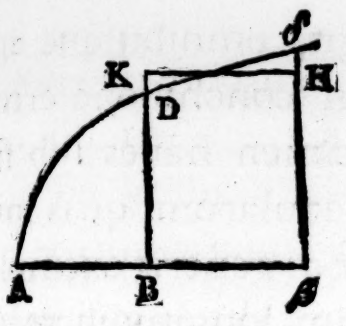
verò aliarum quantitatum, per quævis alia ſymbola; ac momenta earum quantitatum, per rectangulos ſub illis ſymbolis & literâ *o*; aream porro Curvarum, per ordinatam in quadrato incluſam; areâ pro fluente, & ordinatâ pro ejus fluxione poſitis. Cùm autem propoſitionem aliquam demonſtrat, literam *o* adhibet pro finito momento temporis vel ejus exponentis, aut cujuſvis quantitatis uniformiter fluentis; totamque calculationem abſolvit per geometriam veterum, in finitis figuris ſive ſchematibus, ſine ullâ approximatione: & cùm primùm calculatio peracta eſt, & æquatio reducta, ſupponit momentum *o* decreſcere in infinitum, atque evaneſcere. Cùm verò non demonſtrat, ſed ſolùm inveſtigat propoſitionem; quo citiùs rem conficiat, ſupponit momentum *o* eſſe infinitè parvum, & in ſcribendo illud negligit, omnibuſque approximationum modis utitur, quos nullum in concluſione errorem parituros autumat. Prioris generis ſpecimen habes ſub fi-
Ib. N° XII. nem Compendii, ubi primam trium illarum regularum, quas initio libri poſuerat, erat demonſtraturus. Secundi generis exempla ibidem habes; cùm invenit curvarum linearum longitudinem *p.* 15 *; & cùm eruit ordinatas, areas, & longitudines Curvarum mechanicarum *p.* 18, 19 †: narratque, quâ viâ per eandem methodum tangentes duci poſſint ad Curvas mechanicas, *p.* 19 ‡. Atque
Ib. N°XXVI. in epiſtolâ datâ 10 *Decemb.* 1672, addit, problemata de curvaturâ Curvarum, ſeu geometricarum ſive mechanicarum, per eandem methodum ſolvi. Ex quibus manifeſtum eſt, ſe jam tum ſuam methodum ad ſecunda ac tertia momenta extendiſſe: cùm enim areæ Curvarum conſiderantur tamquam fluentes, ut in hâc Analyſi fieri ſolet, ordinatæ exprimunt fluxiones primas; tangentes autem datæ ſunt per fluxiones ſecundas, & curvaturæ per tertias. Et vel in Analyſi hâc *p.* 16 §. ubi Newtonus ait, *Momentum eſt ſuperficies, cùm de Solidis*; *& linea, cùm de ſuperficiebus*; *& punctum, cùm de lineis agitur*; perinde eſt ac ſi dixiſſet, cùm Solida conſiderantur tamquam fluentia, eorum momenta ſuperficies ſunt; & eorum momentorum momenta, vel ſecunda momenta, lineæ ſunt; & horum momentorum momenta, ſive tertia momenta, puncta ſunt, ſecundum ſententiam Cavallerii: atque in *Principiis* ſuis *Philoſophiæ*, ubi frequenter conſiderat lineas tamquam fluentes, à punctis deſcriptas, quorum velocitates creſcunt vel decreſcunt,

* Tom. I. p. 275. † Tom. I. p. 278, 279. ‡ Tom. I. p. 279. § Tom. I. p. 276.

velocitates

velocitates ſunt fluxiones primæ; & earum incrementa, ſecundæ. Ac problema illud, *Datâ æquatione fluentes quantitates involvente, fluxiones invenire, & vice verſâ*, ad fluxiones omnes pertinet; ut conſtat ex ſolutionis ejus exemplis à Walliſio publicatis, Tom. II. operum ſuorum, p. 391, 392, 396. Quin & in Lib. II. *Principiorum Philoſophiæ*, Prop. XIV. differentiam ſecundam Newtonus appellat differentiam momentorum.

Quoque meliùs intelligas, quo calculationis genere Newtonus uſus fuerit anno 1669, vel ante, cùm hoc analyſeos ſuæ compendium ſcripſit; ponam hîc ejus demonſtrationem primæ illius regulæ, ſuprà memoratæ. N° XII.



‘ Sit Curvæ alicujus, ADδ, baſis AB=x; perpendiculariter applicata BD=y; & area ABD =z, ut prius. Item ſit Bβ=o, BK=v, & rectangulum BβHK (ov) æquale ſpatio Bβδ D.

‘ Eſt ergo Aβ=$x+o$, & Aδβ=$z+ov$. His præmiſſis, ex relatione inter x & z, ad arbitrium aſſumptâ, quæro y; ut ſequitur. Pro lubitu ſumatur [æquatio] $\frac{2}{3}x^{\frac{3}{2}}=z$, ſive $\frac{4}{9}x^3=zz$. Tum $x+o$ (Aβ) pro x, & $z+ov$ (Aδβ) pro z ſubſtitutis, prodibit $\frac{4}{9}$ in $x^3+3xxo+3xoo+o^3=$ (ex naturâ Curvæ) $z^2+2zou+o^2v^2$. Et ſublatis $\frac{4}{9}x^3$ & zz æqualibus, reliquiſque per o diviſis, reſtabit $\frac{4}{9}$ in $3x^2+3xo+o^2=2zv+ov^2$. Si jam ſupponamus Bβ in infinitum diminui, & evaneſcere, ſive o eſſe nihil; erunt v & y æquales, & termini per o multiplicati evaneſcent; ideoque reſtabit $\frac{4}{9}\times 3xx=2zv$, ſive $\frac{2}{3}xx\,(=zy)=\frac{2}{3}x^{\frac{3}{2}}y$, ſive $x^{\frac{1}{2}}\left(=\frac{x^2}{x^{\frac{3}{2}}}\right)=y$. Quare contra, ſi $x^{\frac{1}{2}}=y$, erit $\frac{2}{3}x^3=z$.

‘ Vel generaliter, ſi $\frac{n}{m+n}\times ax^{\frac{m+n}{n}}=z$: ſive ponendo $\frac{na}{m+n}=c$, & $m+n=p$; ſi $cx^{\frac{p}{n}}=z$, ſive $c^nx^p=z^n$; tum $x+o$ pro x, & $z+ov$ (ſive quod perinde eſt $z+oy$) pro z ſubſtitutis, prodit c^n in x^p+pox^{p-1}, &c. $=z^n+noyz^{n-1}$, &c. reliquis nempe [ſerierum] terminis, qui tandem evaneſcerent, omiſſis. Jam ſublatis c^nx^p & z^n æqualibus, reliquiſque per o diviſis, reſtat $c^npx^{p-1}=nyz^{n-1}$ $=\frac{nyz^n}{z}=\frac{nyc^nx_p}{cx^{\frac{p}{n}}}$; ſive dividendo per c^nx^p, erit $px^{-1}=\frac{ny}{cx^{\frac{p}{n}}}$; ſive

'$pcx^{\frac{p-n}{n}} = ny$; vel reſtituendo $\frac{na}{m+n}$ pro c, & $m+n$ pro p, hoc eſt
'm pro $p-n$, & na pro pc, fiet $ax^{\frac{m}{n}} = y$. Quare è contra ſi $ax^{\frac{m}{n}} = y$,
'erit $\frac{n}{m+n}ax^{\frac{m+n}{n}} = z$. Q. E. D.'

Eâdem operandi ratione, etiam regula ſecunda demonſtrari poteſt. Et ſi quælibet æquatio aſſumatur, relationem exprimens inter abſciſſam & aream; ordinata inveniri poterit eâdem ratione; ut in proximis Analyſeos verbis indicatur. Et ſi illa ordinata, in unitatem ducta, pro areâ novæ Curvæ ponatur, novæ illius Curvæ ordinata eâdem methodo inveniri poteſt; atque ita in perpetuum. Hæque ordinatæ primam, ſecundam, tertiam, quartam, ſequenteſque fluxiones primæ areæ repræſentant.

Hæc Newtoniana fuit operandi methodus, eo tempore quo compendium illud ſuæ Analyſeos ſcripſit: eâdemque methodo uſus eſt in libro Quadraturarum, atque in hunc uſque diem adhuc utitur.

In exemplis, quibus methodum ſerierum & momentorum in Compendio hoc illuſtrat, hæc ſunt: Eſto radius circuli 1; arcus, z; ſinus, x; æquationes pro inveniendo arcu, cujus ſinus eſt datus, & ſinu, cujus arcus eſt datus, erunt

N° X.
$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \&c.$$
$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362\,88}z^9 - \&c.$$

N° XIV, XIX, XX. Hujus methodi notitiam Collinius Gregorio dedit ſub autumno anni 1669; ac Gregorius, ope unius ex ſeriebus Newtonianis, poſt integrum annuum laborem, methodum demum invenit *Decemb.* 1670: & bimeſtri poſt tempore, in epiſtolâ datâ 15 *Feb.* 1671, varia theoremata, per eam reperta, Collinio miſit; datâ etiam communicandi licentiâ. Collinius autem facillimus erat ad communicandum quæcunque vel à Newtono vel Gregorio acceperat; ut patet ex epiſtolis in hoc Commercio jam publicatis. In ſeriebus, quas in dictâ epiſtolâ miſit Gregorius, hæ duæ ſunt: Eſto radius circuli r; arcus, a; & tangens, t: æquationes, pro inveniendo arcu cujus tangens data eſt, & tangente cujus arcus datus eſt, erunt hæ,

$$a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \&c.$$
$$t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \&c.$$

Eo ipso anno 1671 D. Leibnitius duos tractatus edidit Londini; unum Societati Regiæ, alterum Academiæ Scientiarum *Parisiensi* dedicatum; & in prioris dedicatione commercium suum epistolare cum D. Oldenburgio memorat.

Mense *Feb.* $167\frac{2}{3}$, cùm, in ædibus D. Boyle, Leibnitius in D. Pel- N° XXX.
lium incidisset, sibi arrogare visus est differentialem methodum Moutoni: cúmque Pellius ostendisset methodum illam non novam esse, sed Moutoni, perstitit nihilominus Leibnitius in vindicandâ sibi inventione illâ; præ se ferens, suo se marte inveniisse, Moutoniani operis ignarum, multumque eam promovisse.

Cùm Newtoni serierum una Gregorio missa esset, tentavit ille eam deducere ex seriebus suis, inter se combinatis; ut ipse in epistolâ narrat datâ 19 *Decemb.* 1670. Ac per similem aliquam methodum Leibnitius, priusquam Londino discederet, repperisse videtur summam seriei fractionum decrescentium in infinitum, N° XXX.
quarum numerator est numerus datus, denominatores autem sunt triangulares, vel pyramidales, vel triangulo-triangulares, &c. Ingens verò mysterium! De serie $\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+\frac{1}{5}+$ &c. subduc omnes terminos præter primum, & remanebit $1\ (=1-\frac{1}{2}+\frac{1}{2}-\frac{1}{3}+\frac{1}{3}-\frac{1}{4}+\frac{1}{4}-\frac{1}{5}+\frac{1}{5}-$ &c.$)=\frac{1}{1\times 2}+\frac{1}{2\times 3}+\frac{1}{3\times 4}+\frac{1}{4\times 5}+$ &c. Et ab hâc serie deme omnes terminos, excepto primo, & remanebit $\frac{1}{2}=\frac{2}{1\times 2\times 3}+\frac{2}{2\times 3\times 4}+\frac{2}{3\times 4\times 5}+\frac{2}{4\times 5\times 6}+$ &c. Et à priore serie deduc omnes terminos præter duos primos, & remanebit $\frac{3}{2}=\frac{2}{1\times 3}+\frac{2}{2\times 4}+\frac{2}{3\times 5}+\frac{2}{4\times 6}+$ &c.

Sub finem *Febr.* vel initium *Martii*, $167\frac{2}{3}$, Leibnitius, Londino N° XXXI.
relicto, Parisios se contulit; & ad usque mensem Junium sequentem commercium cum Oldenburgio habuit: deinde Algebram & Geometriam sublimiorem didicit, & mense *Julio* anni 1674, N° XXXII.
commercium cum Oldenburgio renovavit; scribens se mirificum habere theorema, quod daret circuli, vel ejus sectoris cujuscunque, aream accuratè in serie numerorum rationalium; *Octobri* autem insequente scripsit, se inveniisse circumferentiam circuli in serie simpliciſſimorum numerorum; & eâdem *methodo* (sic enim theorema illud nominat) quemvis arcum, cujus sinus datus sit, posse in-

veniri in ſimili ſerie, licet proportio ad totam circumferentiam ignoretur. Theorema ergo iſtud hoc efficiebat; ut inveniretur quivis ſector, vel arcus, cujus ſinus datus ſit. Si ignota eſſet arcûs proportio ad circumferentiam totam, theorema, ſive methodus iſta, tantummodo arcum exhibuit; ſin nota eſſet, etiam integram circumferentiam dedit: & proinde erat theorema prius illud ex duobus ſupradictis Newtoni. Demonſtratio vero hujus theorematis Leibnitio tum non innotuit. Quippe in epiſtolâ datâ 12 *Maii* 1676, rogavit Oldenburgium, ut demonſtrationem ejus à Collinio ſibi pararet; eam ſignificans methodum, per quam Newtonus id invenerat.

N° XLIV.

N° XXXVI. In epiſtolâ à Collinio ſcriptâ, datâque 15 *Apr.* 1675, Oldenburgius ad Leibnitium miſit octo ex Newtonianis & Gregorianis ſeriebus; in quibus erant duæ illæ Newtoni ſupradictæ, pro inveniendo arcu, cujus ſinus datus eſt, & ſinu, cujus datus eſt arcus; duæque illæ Gregorii jam antè memoratæ, pro inveniendo arcu, cujus tangens data eſt, & tangente cujus datus eſt arcus. Leib-

N°XXXVII. nitius verò in reſponſo dato 20 *Maii* 1675, ſe epiſtolam illam accepiſſe his verbis confitebatur. *Literas tuas multâ fruge algebraicâ refertas accepi; pro quibus tibi & doctiſſimo Collinio gratias ago. Cùm nunc, præter ordinarias curas, mechanicis imprimis negotiis diſtrahar, non potui examinare ſeries quas miſiſtis, ac cum meis comparare. Ubi fecero, perſcribam tibi ſententiam meam: nam aliquot jam anni ſunt, quod inveni meas viâ quâdam ſic ſatis ſingulari.*

Nunquam tamen poſtea, vel * agnovit Leibnitius ſe recepiſſe illas ſeries; vel indicavit, quâ in re ſuæ ab illis differrent; vel unquam ullas alias protulit, præter illas ab Oldenburgio miſſas, aut ſeries numerales ex eis deductas in caſibus particularibus. Quid autem egerit cum Gregorii ſerie, pro inveniendo arcu cujus tangens data eſt, ipſe narrat in Actis Eruditorum menſis *April.* 1691, p. 178. *Jam, inquit, anno* 1675, *compoſitam habebam opuſculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum, &c.* Per theorema pro tranſmutandis figuris, ſimile illis Barrovii & Gregorii, jam tandem invenerat ſeriei hujus demonſtrationem; atque

* Cùm hæc recenſio ſcriberetur, non agnoverat; ſed anno ſubſequente, in epiſtolâ ad Cometiſ-
ſam

atque id erat Opuſculi iſtius argumentum. Nondum tamen acquiſiverat cæterarum demonſtrationem; & occaſionem nactus hujus quoque expetendæ, ſequentem epiſtolam Oldenburgio ſcripſit 12 *Maii*, 1676, Pariſiis datam:

Cùm Gregorius Mohr Danus nobis attulerit communicatam ſibi à doctiſſimo Collinio veſtro expreſſionem rationis inter arcum & ſinum, per infinitas ſeries ſequentes; poſito ſinu x, *arcu* z, *radio* 1, N° XLIV.

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \&c.$$

$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 - \&c.$$

Hæc, INQUAM, *cùm nobis attulerit ille, quæ mihi valdè ingenioſa videntur, & poſterior imprimis ſeries elegantiam quandam ſingularem habeat: ideo rem gratam mihi feceris, vir clariſſime, ſi demonſtrationem tranſmiſeris. Habebis viciſſim mea, ab his longè diverſa, circa hanc rem meditata; de quibus jam aliquot abhinc annis ad te perſcripſiſſe credo, demonſtratione tamen non additâ, quam nunc polio. Oro ut Clariſſimo Collinio multam à me ſalutem dicas: is facilè tibi materiam ſuppeditabit ſatisfaciendi deſiderio meo.*

Hîc, qui illud INQUAM legerit, facilè exiſtimaverit Leibnitium duas illas ſeries nunquam antea vidiſſe; *diverſaque ejus circa hanc rem meditata* prorſus aliud eſſe, quàm ſerierum unam, quas anno ſuperiore receperat ab Oldenburgio, demonſtrationemque iſtam, quam tunc expoliret, quantivis pretii fore; quippe quam pro Newtonianæ methodi munere ἀντίδωρον acceptiſſimum erat miſſurus.

Hâc epiſtolâ receptâ, Oldenburgius Colliniuſque, literis ad Newtonum ſcriptis, vehementer operam dabant, ut ipſe Newtonus methodum ſuam deſcriberet, Leibnitio communicandam. Quam ob rem Newtonus epiſtolam ſcripſit, 13 *Junii* 1676 datam: in N° XLVIII. quâ eo modo ſerierum methodum deſcripſit, quo antea in ſupradicto compendio fecerat; hâc tamen differentiâ: hîc fuſè deſcripſit reductionem dignitatis binomialis in ſeriem; at reductionem per diviſionem, radicumque affectarum extractionem, leviter tantùm attigit: illîc reductionem fractionum & radicalium in ſeries, per diviſionem radicumque extractionem, fuſè deſcripſit; at poſuit tantummodo duos primos terminos ſeriei, in quam dignitas binomialis reduci poſſit. Inter exempla, quæ epiſtola illa conti-

ſam de *Kilmanſegg*, agnovit ſe tunc ab Oldenburgio accepiſſe [*Des Eſſais*] ſerierum ſpecimina.

nebat,

Nº XLIX. nebat, erant ſeries pro inveniendo numero, cujus logarithmus ſit datus, & pro inveniendo verſo ſinu, cujus arcus datus ſit. Hæc epiſtola Pariſios miſſa eſt 26 *Jun.* 1676, unà cum manuſcripto quodam Collinii, extracta quædam continente ex epiſtolis Jacobi Gregorii.

Nº XLVI. Gregorius enim prope finem anni 1675 diem ſuum obierat; Colliniuſque, exoratus à Leibnitio aliiſque ex Academiâ Scientiarum, extracta ex ejus epiſtolis confecit; quæ adhuc extant ipſius Collinii manu exarata, hoc titulo: *Extracta ex D.* Gregorii *literis, D.* Leibnitio *commodanda, qui exorandus eſt, ut cùm uſus eis fuerit, tibi ea remittat.* Porro hæc extracta ad Leibnitium miſſa fuiſſe, teſtis eſt ipſe Collinius, in epiſtolâ ad Davidem Gregorium, Jacobi τῦ μακαρίτυ fratrem, datâ 11 *Aug.* 1676; idque amplius conſtat ex Leibnitii Tſchurnhauſiique reſponſis.

Leibnitii reſponſum, Oldenburgio miſſum datumque 27 *Aug.*
Nº LI. 1676, ſic incipit. *Literæ tuæ, die* Julii 26 *datæ, plura ac memorabiliora circa rem analyticam continent, quàm multa volumina ſpiſſa de his rebus edita. Quare tibi pariter, ac clariſſimis viris Newtono ac Collinio, gratias ago, qui nos participes tot meditationum egregiarum eſſe voluiſtis.* Et prope finem epiſtolæ, poſtquam Newtonia-
Nº LIII. næ epiſtolæ contenta enarrâſſet, ita pergit: *Ad alia tuarum literarum venio, quæ doctiſſimus Collinius communicare gravatus non eſt. Vellem adjeciſſet appropinquationis Gregorianæ linearis demonſtrationem. Fuit enim his certè ſtudiis promovendis ampliſſimus.* Reſponſum verò Tſchurnhauſii, datum 1 *Sept.* 1676, cùm New-
Nº LIV. toni de ſeriebus epiſtolam commemorâſſet, his verbis concluditur: *Similia porro, quæ hâc in re præſtitit eximius ille geometra Gregorius, memoranda certè ſunt. Et quidem optimè famæ ipſius conſulturi ſunt, qui, ipſius relicta manuſcripta luci publicæ ut exponantur, operam navabunt.* In priore epiſtolæ parte, ubi de ſeriebus Newtonianis loquitur, ſe eas leviter percurriſſe dicit, viſurum ſi fortè in eis inveniret Leibnitii ſeriem pro circulo hyperbolâve quadrandis. Quòd ſi in extractis Gregorianarum epiſtolarum eam inqui-
Nº XX, XLVI. ſiviſſet, repperiſſet utique in epiſtolâ 15 *Feb.* 1671, ſuprà memoratâ. Quippe extracta illa, in quibus ea habetur epiſtola, ſuperſunt adhuc Collinii manu ſcripta.

Quamquam

Quamquam autem ſeriem illam, de quâ agitur, jam bis ab Oldenburgio Leibnitius accepiſſet, illam ipſam tamen, in epiſtolâ datâ 27 *Aug.* 1676, velut ſuam Oldenburgio remiſit, quaſi munus ἀντάξιον pro methodo Newtoni; præ ſe ferens, ſe jam triennio antè, vel amplius, amicis ſuis Pariſienſibus eam oſtendiſſe; hoc eſt, biennio prius quàm eam accepiſſet in Oldenburgii epiſtolâ 15 *Apr.* 1675. Atqui illo tempore ſeriem illam ſuam eſſe neſciebat; uti conſtat ex ipſius reſponſo 20 *Maii* 1675, ſuprà citato. Fieri quidem potuit, ut Londini eam acceperit, ac amicis Pariſienſibus oſtenderit; triennio prius quàm Oldenburgio eam remiſerit: minimè tamen conſtat, ſe ejus demonſtrationem tam maturè nactum eſſe. Hanc ubi primùm reppererat, tunc demum in opuſculo ſuo eam exhibuit, cumque amicis communicavit: idque ipſe narrat contigiſſe anno 1675. Illud verò probandum & evincendum eſt, ſe prius eam penes ſe habuiſſe, quàm ab Oldenburgio eam accepiſſet. Quippe in reſponſo ſuo ad Oldenburgium, nullam ex ſeriebus tunc miſſis ſuam eſſe ſciebat; celabatque ab amicis Pariſienſibus, ſe illam cum pluribus aliis ab Oldenburgio accepiſſe, ac vidiſſe ſe Gregorii epiſtolam, in quâ is Collinio eam miſerat, ineunte anno 1671. N° LII. N° XXXVI, XXXVII.

In eâdem epiſtolâ, 27 *Aug.* 1676, poſtquam deſcripſerat quadraturam ſuam circuli hyperbolæque æquilateræ, hæc addit Leibnitius: *Viciſſim, ex ſeriebus regreſſuum, pro hyperbolâ hanc inveni.* N° LII. *Sit numerus aliquis, unitate minor,* $1-m$; *ejuſque logarithmus hyperbolicus,* l. *Erit* $m=\frac{l}{1}-\frac{l^2}{1\times 2}+\frac{l^3}{1\times 2\times 3}-\frac{l^4}{1\times 2\times 3\times 4}+$ &c. *Si numerus ſit major unitate, ut* $l+n$, *tunc pro eo inveniendo mihi etiam prodiit regula, quæ in Newtoni epiſtolâ expreſſa eſt: ſcilicet erit* $n=\frac{l}{1}+\frac{l^2}{1\times 2}+\frac{l^3}{1\times 2\times 3}+\frac{l^4}{1\times 2\times 3\times 4}+$ &c. *Quod regreſſum ex arcubus attinet, incideram ego directè in regulam, quæ ex dato arcu ſinum complementi exhibet. Nempe ſinus complementi* $=1-\frac{a^2}{1\times 2}+\frac{a^4}{1\times 2\times 3\times 4}-$ &c. *Sed poſtea quoque deprehendi, ex eâ illam nobis communicatam, pro inveniendo ſinu recto, qui eſt* $\frac{a}{1}-\frac{a^3}{1\times 2\times 3}+\frac{a^4}{1\times 2\times 3\times 4\times 5}-$ &c. *poſſe demonſtrari.*

In his verbis Leibnitius ſibi laudem vindicat co-inventionis quatuor harum ſerierum: quamvis methodus eas inveniendi, ipſo expetente,

expetente, ad eum missa fuerit; quam tamen nondum intelligere, nec comprehendere poterat. In eâdem utique epistolâ 27 *Aug.* 1676, orabat D. Newtonum, ut clariùs eam explicaret.

N° LII. Verba ipsius sunt: *Sed desideraverim, ut clarissimus Newtonus nonnulla quoque ampliùs explicet; ut originem theorematis, quod initio ponit: item modum quo quantitates p, q, r, in suis operationibus invenit: ac denique quomodo in methodo regressuum se gerat; ut cùm ex logarithmo quærit numerum. Neque enim explicat quomodo id ex methodo suâ derivetur.* Præ se tulit, inveniisse se duas series pro numero cujus logarithmus sit datus; & tamen in ipsâ eâ epistolâ Newtonum rogat, ut methodum eas ipsas duas series inveniendi sibi explicare velit.

Ubi hanc ejus epistolam accepisset Newtonus, rescripsit se om-

N° LXI. nes illas quatuor series jam ei communicâsse; quarum duæ priores una eademque series esset, literâ *l* pro logarithmo positâ cum suo signo + vel −; tertia verò excessus esset radii supra sinum versum, pro quo jam antea series ad eum missa fuisset. His lectis, destitit Leibnitius ab inventione hâc sibi vindicandâ. Præter hæc,

N° LXIV. in eâdem epistolâ 24 *Oct.* 1676, quod petierat Leibnitius, methodos suas regressionis apertiùs explicavit. Leibnitius tamen, epistolâ 21 *Jun.* 1677 datâ, ulteriorem adhuc petebat explicationem: paulo verò pòst, cùm Newtoni epistolam repetitâ vice le-

N° LXX. gisset, rescripsit 12 *Jul.* 1677, se jam, quod ignoraverat, intelligere; & ex chartis suis repositis animadvertere, se jam antea unam ex Newtoni methodis adhibuisse; in exemplo verò, quo fortè esset usus, cùm nihil pulchri & elegantis proveniret, se, pro solitâ suâ impatientiâ, postea eam abjeciisse. Plures itaque (si credere fas est) directas series, & proinde earum inveniendarum methodum habuit, priusquam inveniisset methodum inversam, ejusque postea oblitus esset. Quòd si chartas suas repositas diligentiùs pervolvisset, etiam hanc inversam methodum ibi repperiisset. Sed, propriarum scilicet methodorum oblitus, Newtonianas desiderabat.

N° L. Cùm Newtonus, in epistolâ datâ 13 *Jun.* 1676, methodum suam serierum enarrâsset, hæc addidit: *Ex his videre, est quantum fines analyseos per hujusmodi infinitas æquationes ampliantur: quippe quæ, earum beneficio, ad omnia penè dixerim problemata, si numeralia*

meralia Diophanti & ſimilia excipias, ſeſe extendit. Non tamen omnino univerſalis evadit, niſi per ulteriores quaſdam methodos eliciendi ſeries infinitas. Sunt enim quædam problemata, in quibus non licet ad ſeries infinitas per diviſionem vel extractionem radicum, ſimplicium affectarumve, pervenire. Sed quomodo in iſtis caſibus procedendum ſit, jam non vacat dicere; ut neque alia quædam tradere, quæ circa reductionem infinitarum ſerierum in finitas, ubi rei natura tulerit, excogitavi. Nam parciùs ſcribo, quòd hæ ſpeculationes diu mihi faſtidio eſſe cæperunt; adeo ut ab iiſdem jam per quinque fere annos abſtinuerim. His D. Leibnitius, in epiſtolâ ſuâ N° LIII. 27 *Aug.* 1676 datâ, ſic reſpondit: *Quod dicere videmini, pleraſque difficultates (exceptis problematis Diophanteis) ad ſeries infinitas reduci, id mihi non videtur. Sunt enim multa uſque adeo mira & implexa, ut neque ab æquationibus pendeant, neque ex quadraturis. Qualia ſunt, ex multis aliis, problemata methodi tangentium inverſæ.* Et D. Newtonus in epiſtolâ ſuâ 24 *Octob.* 1676 reſcripſit: N° LXIV. *Ubi dixi omnia pene problemata ſolubilia exiſtere; volui de iis præſertim intelligi, circa quæ Mathematici ſe hactenus occupârunt; vel ſaltem in quibus raticionia mathematica locum aliquem obtinere poſſunt. Nam alia ſane adeo perplexis conditionibus implicita excogitare liceat, ut non ſatis comprehendere valeamus: & multo minùs tantarum computationum onus ſuſtinere, quod iſta requirerent. Attamen ne nimium dixiſſe videar, inverſa de tangentibus problemata ſunt in poteſtate, aliaque illis difficiliora. Ad quæ ſolvenda uſus ſum duplici methodo, unâ concinniori, alterâ generaliori. Utramque viſum eſt impræſentia literis tranſpoſitis conſignare, ne propter alios idem obtinentes, inſtitutum in aliquibus mutare coger.* 5acedæioeffh &c. id eſt: *Una methodus conſiſtit in extractione fluentis quantitatis, ex æquatione ſimul involvente fluxionem ejus: altera tantùm in aſſumptione ſeriei pro quantitate quâlibet incognitâ, ex quâ cætera commodè derivari poſſunt; & in collatione terminorum homologorum æquationis reſultantis, ad eruendos terminos aſſumptæ ſeriei.*

Ex duabus his Newtoni epiſtolis certò conſtat, jam tum, vel potius ante quinquennium, inveniſſe illum reductionem problematum ad æquationes fluxionales & ſeries convergentes: & ex reſponſo Leibnitii ad harum epiſtolarum priorem, æquè certum eſt,

tum nondum hunc inveniſſe reductionem problematum, vel adæquationes differentiales, vel ad ſeries convergentes.

N° XXXVIII. Idque ampliùs ex eis manifeſtum eſt, quæ de hâc re ſcripſit Leibnitius anno 1691, in Actis Eruditorum: *Jam anno* 1675, *inquit, compoſitum habebam opuſculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum; ſed quod, materiâ ſub manibus creſcente, limare ad editionem non vacavit, poſtquam aliæ occupationes ſupervenere; præſertim cùm nunc prolixiùs exponere, vulgari more, quæ analyſis noſtra paucis exhibet, non ſatis operæ pretium videatur.* Hanc Quadraturam, vulgari more compoſitam, proferre cœpit Pariſiis anno 1675. Anno proximo demonſtrationem ejus expoliebat, Oldenburgio mittendam, ceu methodi Newtonianæ ἀντάλλαγμα; N° XLIV. ut narrat in epiſtolâ 12 *Maii* 1676: & proinde, in epiſtolâ 27 *Aug.* 1676, eam miſit contextam & edolatam more vulgari. Hieme inſequente, in Germaniam reverſus per Angliam & Hollandiam, ut negotia publica capeſſeret, non vacavit ampliùs ad eam prælo parandam, nec operæ pretium exiſtimavit, ea more vulgari prolixiùs explicare, quæ analyſis ejus paucis exhibet. Hanc ergo novam analyſin excogitavit, jam in Germaniam reverſus; & proinde non ante annum 1677.

Idque ampliùs adhuc conſtat ex conſideratione ſequenti. Barrovius methodum ſuam tangentium anno 1670 in lucem edidit. Inde Gregorius methodum tangentium hauſit abſque computa-
N° XVII. tione, uti ad Collinium ſcripſit 5 *Sept.* 1670. Newtonus autem ſuam tangentium cum Collinio communicavit anno 1672, in epiſ-
N° XXVI. tolâ 10 *Decemb.* datâ, atque hæc ibi addidit. *Hoc eſt unum particulare, vel Corollarium potius, methodi generalis; quæ extendit ſe, citra moleſtum ullum calculum, non modò ad ducendum tangentes ad quaſvis Curvas, ſive geometricas, ſive mechanicas, vel quomodocunque rectas lineas, aliaſve Curvas reſpicientes; verùm etiam ad reſolvendum alia abſtruſiora problematum genera de curvitatibus, areis, longitudinibus, centris gravitatis Curvarum, &c. Neque, quemadmodum Huddenii methodus de maximis & minimis, ad ſolas reſtringitur æquationes illas, quæ quantitatibus ſurdis ſunt immunes. Hanc methodum intertexui alteri iſti, quâ æquationum exegeſin inſtituo, reducendo eas ad ſeries infinitas.* D. autem Sluſius ſuam tangentium methodum ad Oldenburgium miſit 17 *Jan.* $167\frac{2}{3}$,

$167\frac{2}{3}$, eaque paulo poſt in Tranſactionibus eſt publicata. Comperta verò eſt eadem prorſus eſſe cum illâ Newtoni. Fundata erat ſuper tribus lemmatibus; quorum primum erat, *Differentia duarum dignitatum ejuſdem gradûs applicata ad differentiam laterum dat partes ſingulares gradûs inferioris ex binomio laterum, ut* $\frac{y^2-x^3}{y-x} = yy+yx+xx$, *id eſt, ſecundum notationem Leibnitii,* $\frac{dy^3}{dy} = 3yy$. Newtonianæ epiſtolæ, 10 *Decemb.* 1672, exemplar ad Leibnitium ab Oldenburgio miſſum eſt, inter chartas Jacobi Gregorii, unà cum aliâ Newtoni epiſtolâ 13 *Jun.* 1676 datâ. In his duabus cùm memoraret Newtonus, ſe generalem admodum analyſin habere, partim conſiſtentem ex methodo ſerierum convergentium, partim ex aliâ methodo, quâ applicabat eas ſeries ad ſolutionem omnium ferè problematum, exceptis forte numeralibus quibuſdam quales illæ ſunt Diophanti, eruebatque tangentes, areas, longitudines, contenta ſolida, centra gravitatis, curvitateſque Curvarum ac curvilinearum figurarum, ſeu geometricarum, ſive mechanicarum, minimè hærendo ad ſurdas; methodumque illam tangentium Sluſianam non niſi ramum, vel corollarium, eſſe alterius hujus methodi: his Leibnitius viſis, dum domum per Hollandiam reverteretur, tum demum meditabatur promotionem methodi Sluſianæ. Quippe in epiſtolâ ad Oldenburgium Amſte- N° LXV.
lodami datâ $\frac{18}{28}$ *Novemb.* 1676, ſic ſcripſit: *Methodus tangentium, à Sluſio publicata, nondum rei faſtigium tenet. Poteſt aliquid ampliùs præſtari in eo genere, quod maximi foret uſûs ad omnis generis problemata, etiam ad meam (ſine extractionibus) æquationum ad ſeries reductionem. Nimirum poſſet brevis quædam calculari circa tangentes tabula, eouſque continuanda donec progreſſio tabulæ appareret; ut eam ſcilicet quiſque, quouſque libuerit, ſine calculo continuare poſſit.* Hæc verò erat illa promotio Sluſianæ methodi in methodum generalem, quam tum in animo verſabat Leibnitius: exque illis ejus verbis, *poteſt aliquid amplius præſtari in eo genere, quod maximi foret uſûs ad omnis generis problemata*, unica res hæc fuiſſe videtur, quâ ille methodum eam ad omnis generis problemata vellet extendere. Promotio verò per calculum differentialem nondum ei in mentem venerat; ea quippe referenda erit ad annum ſequentem. N° XLVI.

N° LVII. In proximis literis, 24 *Octob.* 1676, mentionem analyſeos ſuæ fecit Newtonus, communicatæ per Barrovium cum Collinio anno 1669; alteriuſque item tractatûs, anno 1671 ſcripti, de ſeriebus convergentibus, deque alterâ illâ methodo, quâ tangentes ducerentur more Sluſii, maximæque ac minimæ determinarentur, & quadratura Curvarum expeditior fieret, idque non hæſitando ad radicales; quâque invenirentur ſeries, quæ certis caſibus finirentur, & quadraturam Curvarum darent in æquationibus finitis, ubi fieri poſſet. Fundamentum autem harum operationum concluſit in hanc ſententiam, ænigmaticè, ut ſuprà, expreſſam; *Datâ æ-*

N° LVII. *quatione fluentes quotcunque quantitates involvente, fluxiones invenire; & vice verſâ.* Quibus extra omnem dubitationem ponitur, ſe jam antea fluxionum methodum excogitâſſe. Quòd ſi reliqua in epiſtolâ illâ animadvertantur; conſtabit utique, ſe methodum illam jam tum ad magnam perfectionem provexiſſe, & feciſſe admodum generalem: cùm illæ in libro ſuo Quadraturarum propoſitiones, methodique ſerierum convergentium, lineamque curvam ducendi per quemvis datorum punctorum numerum, jam tum ſibi innotuerunt. Quippe, cùm fluxionum methodus haud procedit in æquationibus finitis, æquationes in ſeries convergentes reducit per theorema binomiale, perque fluentium extractionem ex æquationibus, fluxiones earum involventibus vel non involventibus. Cúmque æquationes finitæ defuerint, ſeries convergentes ex problematis conditionibus deducit; aſſumendo terminos ſerierum gradatim, & per conditiones illas determinando. Cúmque porro fluentes à fluxionibus ſint derivandæ, & fluxionum lex defuerit; legem eam invenit quamproximè, parabolicam lineam per quemlibet datorum punctorum numerum ducendo. Atque his progreſſionibus, vel illo tempore fluxionum ſuam methodum multo magis univerſalem fecerat Newtonus, quàm vel hodie eſt methodus Leibnitii differentialis.

Hæc Newtoni epiſtola, data 24 *Octob.* 1676, in fine menſis illius, vel initio ſequentis, viſa eſt Leibnitio Londini; ejuſque exemplar Hanoveriæ ei obtigit, initio veris inſequentis: atque ipſe

N° LXVI. paulo pòſt Leibnitius, epiſtolâ datâ 21 *Jun.* 1677, reſcripſit: *Clariſſimi Sluſii methodum tangentium nondum eſſe abſolutam, celeberrimo Newtono aſſentior. Et jam à multo tempore rem tangentium generaliùs*

generaliùs tractavi, ſcilicet per differentias ordinatarum—Hinc nominando in poſterum, dy *differentiam duarum proximarum* y, &c. Hîc demum primò cœpit Leibnitius differentialem ſuam methodum proferre: neque vel minimum argumentum eſt, prius eam ſe ſciviſſe, quàm poſtremas Newtoni literas accepiſſet. Dicit quidem, *jam à multo tempore rem tangentium generaliùs ſe tractaviſſe, ſcilicet per differentias ordinatarum.* Atqui in aliis literis eodem modo jam affirmaverat, ſe plures convergentes ſeries, tam directas quàm inverſas, inveniſſe; priuſquam ullam inveniendi eas methodum haberet: oblitumque jam fuiſſe inverſæ methodi ſerierum; priuſquam utilitatem ejus perciperet. Nemo in cauſâ propriâ ſibi teſtis eſt. Iniquus admodum fuerit judex, omniumque gentium jura conculcaverit, qui quemquam in ſuâ cauſâ pro legitimo teſte admiſerit. Illud ergo eſt probandum ac oſtendendum, jam antea methodum hanc Leibnitium inveniſſe, quàm literas illas Newtoni accepiſſet. Quòd ſi hoc nullo argumento confirmatum fuerit; de primo methodi inventore nulla ſupereſt controverſia.

Marchio Hoſpitalius, vir candidiſſimus, in præfatione libri ſui *De Analyſi quantitatum infinitè parvarum*, A. D. 1696 editâ, narrat; ut, paulo poſt tangentium methodum à Carteſio publicatam, Fermatius quoque methodum invenerit, quam ipſe tandem Carteſius ſuâ in pleriſque ſimpliciorem eſſe confeſſus eſt. ‘ Non-
‘ dum tamen, inquit Hoſpitalius, tam ſimplex erat, quàm poſtea
‘ à Barrovio reddita eſt, naturam polygonorum propius conſide-
‘ rando; quæ ſponte ſuâ menti objicit parvulum triangulum,
‘ compoſitum ex particulâ Curvæ, inter duas ordinatas ſibi infinitè
‘ propinquas jacentis, & ex differentiâ duarum iſtarum ordinata-
‘ rum, duarumque itidem correſpondentium abſciſſarum. At-
‘ que hoc triangulum illi ſimile eſt, quod ex tangente et ordinatâ
‘ & ſubtangente fieri debet: adeo ut per unam ſimplicem analo-
‘ giam omnis jam calculatio evitetur, quæ & in Carteſianâ, & in
‘ hâc ipſâ priùs methodo, neceſſaria erat. Quo tamen vel hæc, vel
‘ Carteſiana, revocari ad uſus poſſet, neceſſariò tollendæ erant
‘ fractiones & radicales. Ob hujus itaque calculi imperfectio-
‘ nem, introductus eſt ille alter celeberrimi Leibnitii; qui inſig-
‘ nis geometra inde eſt exorſus, ubi Barrovius aliique deſierant.

‘ Porro

'Porro hic ejus calculus in regiones hactenus ignotus aditum fe-
'cit; atque ibi tot & tanta patefecit, quæ vel doctiſſimos totius
'Europæ Mathematicos in admirationem conjecerunt, &c.'

Hactenus Hoſpitalius. Non viderat nimirum Newtoni analyſin, neque epiſtolas ejus 10 *Dec.* 1672, 13 *Jun.* 1676, & 24 *Octob.* 1676 datas: quarum nulla ante annum 1699 typis publicata eſt. Neſcius itaque Newtonum hæc omnia effeciſſe, atque indicâſſe Leibnitio, Leibnitium ipſum arbitratus eſt inde incepiſſe, ubi deſierat Barrovius; Leibnitium docuiſſe, quo pacto Barrovii methodus adhiberetur non hærendo ad fractiones & ſurdas, eâque re mirificè eam ampliâſſe & promoviſſe. Similiterque Jacobus Bernoullius in Actis Eruditorum *Jan.* 1691, p. 14, ſic memorat: *Qui calculum Barrovianum, quem in lectionibus ſuis geometricis adumbravit author, cujuſque ſpecimina ſunt tota illa propoſitionum inibi contentarum farrago, intellexerit*; [calculum] *alterum, à domino Leibnitio inventum, ignorare vix poterit*; *utpote qui in priori illo fundatus eſt, & niſi fortè in differentialium notatione, & operationis aliquo compendio, ab eo non differt.*

Jam verò, in methodo ſuâ tangentium, Barrovius ducit duas ordinatas indefinitè ſibi invicem propinquas, literamque *a* ponit pro ordinatarum differentiâ, proque abſciſſarum differentiâ literam *e*; & in ducendis tangentibus has tres regulas ſtatuit: 1. *Inter computandum, inquit, omnes abjicio terminos, in quibus ipſarum* a *vel* e *poteſtas habeatur, vel in quibus ipſæ ducuntur in ſe. Etenim ipſi termini nihil valebunt.* 2. *Poſt æquationem conſtitutam, omnes abjicio terminos, literis conſtantes quantitates notas ſeu determinatas ſignificantibus, aut in quibus non habentur* a *vel* e. *Etenim illi termini ſemper ad unam æquationis partem adducti, nihilum adæquabunt.* 3. *Pro* a *ordinatam, & pro* e *ſubtangentem ſubſtituo. Hinc demum ſubtangentis quantitas dignoſcitur.*

N° LXVI. Hactenus Barrovius. Leibnitius autem in epiſtolâ 21 *Jun.* 1677 ſuprà citatâ, in quâ primò differentialem ſuam methodum cœpit proponere, Barrovianam hanc tangentium methodum exactè ſecutus eſt; præterquam quòd literas, *a* & *e*, Barrovianas mutaverit in *dx* & *dy*. Quippe in exemplo, quod ibi exhibet, duas ducit parallelas lineas; atque omnes terminos ſub inferiore lineâ ponit, in quibus *dx* & *dy* (diviſim vel junctim) ſunt plus unius dimenſionis;

ſionis ; omnes verò terminos, in quibus *dx* & *dy* abſunt, ſuper lineam ſuperiorem ſtatuit ; &, ob rationes à Barrovio datas, omnes hos terminos facit evaneſcere. Jam autem per terminos in quibus *dx* & *dy* unius tantùm dimenſionis ſunt, quoſque inter binas illas lineas ponit, proportionem ſubtangentis ad ordinatam determinat. Rectè itaque animadvertit Marchio Hoſpitalius, Leibnitium inde incipere, ubi Barrovius deſierat ; quippe utriuſque methodus tangentium prorſus eſt eadem.

Illud tamen Leibnitius de hâc methodo ſuperannotat ; concluſionem nempe hujus calculi cum Sluſii regulâ coincidere ; illamque regulam cuivis, qui hanc methodum intelligat, in promptu occurrere. Acutè ſanè : quippe in epiſtolis ſuis Newtonus indicaverat, Sluſianam regulam generalis ſuæ methodi Corollarium tantùm eſſe.

Cúmque in epiſtolis Newtonus dixiſſet, in ducendis tangentibus, maximiſque & minimis determinandis, methodum ſuam procedere, non hæſitando ad ſurdas ; Leibnitius itidem annotat, ſic promoveri poſſe tangentium ſuam methodum, ut ad ſurdas & fractiones non hæreamus ; & deinde addit : *Arbitror, quæ celare* N° LXVI. *voluit Newtonus, de tangentibus ducendis, ab his non abludere. Quod addit, ex hoc eodem fundamento quadraturas quoque reddi faciliores, me in hâc ſententiâ confirmat ; nimirum ſemper figuræ illæ ſunt quadrabiles, quæ ſunt ad æquationem differentialem.* Ex quibus ejus verbis, cum præcedente calculatione comparatis, non dubium eſt, quin tum ſatis ſciverit Leibnitius, Newtono ad manum fuiſſe methodum hæc omnia efficientem ; apparetque eum tentaviſſe, ſi fortè differentialis tangentium methodus Barroviana ad eadem efficienda promoveri poſſet.

Differentialis hujus methodi elementa publicavit Leibnitius in Actis Eruditorum *Nov.* 1684 ; exemplíſque ducendi tangentes, maximaſque & minimas determinandi, eam illuſtravit ; quibus addit : *Et hæc quidem initia ſunt geometriæ cujuſdam multo ſublimioris, ad difficillima ac pulcherrima quæque etiam mixtæ matheſeos problemata pertingentis ; quæ ſine calculo differentiali,* AUT SIMILI, *non temerè quiſquam pari facilitate tractabit.* Ubi, cùm dicit AUT SIMILI, ſine dubio ad Newtoni methodum reſpexit : totaque iſta periodus

periodus nihil ampliùs in ſe habet, quàm quod Newtonus, in literis 1672 & 1676, de ſuâ generali methodo affirmaverat.

Et in Actis Eruditorum 1686, p. 279, *Malo autem, inquit Leibnitius,* dx *& ſimilia adhibere, quàm literas pro illis, quia iſtud* dx *eſt modificatio quædam ipſius* x, *&c.* Sciebat ſcilicet in hâc methodo literas, more Barrovii, ſatis commodè poſſe adhiberi; malebat tamen novis ſymbolis uti, *dx* & *dy*; etſi nihil per hæc ſymbola fieri poſſit, quod non breviùs commodiúſque per ſingulas literas fiat.

Anno ſequente in lucem edita ſunt Newtoni Principia Philoſophiæ; refertus liber ejuſmodi problematibus, qualia Leibnitius difficillima appellaverat, *& pulcherrima etiam mixtæ matheſeos problemata; quæ ſine calculo differentiali, aut* SIMILI, *non temerè quiſquam pari facilitate tractabit.* De hoc libro ſic locutus eſt Marchio Hoſpitalius, *quaſi totus ferè per hunc calculum compoſitus eſſet.* Et ipſe adeo Leibnitius, epiſtolâ ad Newtonum datâ Hanoveriæ $\frac{7}{17}$ *Martii* 1693, ipſiuſque manu ſcriptâ, quæ adhuc ſupereſt, & Regiæ Societati nuper eſt exhibita, eandem rem agnoſcebat his verbis: *Mirificè ampliaveras geometriam tuis ſeriebus; ſed edito Principiorum opere, oſtendiſti patere tibi, quæ analyſi receptæ non ſubſunt. Conatus ſum ego quoque, notis commodis adhibitis, quæ differentias & ſummas exhibeant, geometriam illam, quam tranſcendentem appello, analyſi quodammodo ſubjicere; nec res malè proceſſit.* Atque iterum in reſponſo ad D. Fatium, quod habetur in Actis Eruditorum *Maii* 1700, p. 203, verſu 21, id faſſus eſt Leibnitius.

In ſecundi libri Principiorum lemmate ſecundo, elementa hujus calculi ſyntheticè demonſtrata ſunt; & in fine lemmatis eſt ſcholium, his verbis: *In literis, quæ mihi cum Geometrâ peritiſſimo G. G. Leibnitio annis abhinc decem intercedebant, cùm ſignificarem me compotem eſſe methodi determinandi maximas & minimas, ducendi tangentis & ſimilia peragendi, quæ in terminis ſurdis, æquè ac in rationalibus, procederet; & literis tranſpoſitis hanc ſententiam involventibus* [Datâ æquatione quotcunque quantitates fluentes involvente, fluxiones invenire, & vice verſâ] *eandem celarem: reſcripſit vir clariſſimus* [anno proximo] *ſe quoque in ejuſmodi methodum incidiſſe; & methodum ſuam communicavit, à meâ vix abludentem*

dentem, præterquam in verborum & notarum formulis. Utriuſque fundamentum continetur in hoc lemmate. In illis epiſtolis, & in aliâ 10 *Decemb.* 1672 datâ (cujus exemplar poſt annos quatuor ab Oldenburgio ad Leibnitium mittebatur, ut ſuprà diximus) adeo apertè explicaverat ſuam methodum Newtonus; ut non difficile fuerit Leibnitio, ſubſidio methodi tangentium Barrovianæ, ex illis epiſtolis eam exſculpere. Certum tamen eſt, ex argumentis ſuprà allatis, non priùs eum ſciviſſe eam, quàm epiſtolas illas legiſſet. N° XXVI.

Duarum Newtoni epiſtolarum, 13 *Jun.* & 24 *Octob.* 1676, exemplar ab Oldenburgio acceperat Walliſius, & ex eis plura publicaverat in Algebrâ ſuâ, Anglicè editâ 1683, Latinè autem 1693; pauloque pòſt ex Hollandiâ admonitus eſt, ut epiſtolas illas integras publicaret; quia notiones de fluxionibus Newtonianæ cum plauſu ibi per hominum ora ferrentur, ſub nomine methodi differentialis Leibnitii. Quamobrem in præfatione primi ſuorum operum Tomi, A. D. 1695 editi, ejus rei mentionem injecit. Et in epiſtolâ ad Leibnitium, datâ 1 *Dec.* 1696, quæ in tertio tomo extat, hæc de eâ re habet: *Cùm præfationis præfigendæ poſtremum folium erat ſub prælo, ejuſque typos jam poſuerant typothetæ; me monuit amicus quidam, harum rerum gnarus, qui peregrè fuerat, tum talem methodum in Belgio prædicari, tum illam cum Newtoni methodo fluxionum quaſi coincidere. Quod fecit ut, tranſlatis typis jam poſitis, monitum interſeruerim.* Quin & in epiſtolâ ad Newtonum, datâ 10 *April.* 1695 & Regiæ Societati nuper exhibitâ, ſic de eâ re verba facit: *Utinam typis ederes prolixas illas duas epiſtolas* Junii & Auguſti (Octobrem *dicere debuit)* 1676. *'Ex Hollandiâ certior factus ſum, amicos ibi tuos hoc poſtulare; 'quia notiones tuæ de fluxionibus Leibnitio ibi aſcribuntur, ſub 'nomine calculi differentialis. Hoc ex Hollandiâ accepi, cùm totus 'hic tomus, præter partem præfationis, jam prælo ſubjectus eſſet; ita 'ut nihil aliud inſerere hîc potuerim, dum ceſſarent operæ, præter 'brevem illam, quam ibi reperies, narrationem. Non tam æquus es 'vel tuo vel gentis tuæ honori, quàm oportebat: cùm res quantivis 'pretii tam diu in ſcriniis celas, donec alii honorem tibi debitum 'præripiant. Conatus ſum in eo negotio debitum tibi reddere; do-'leoque me non binas iſtas epiſtolas integras atque αὐτολεξὶ edidiſſe.'* Operum Vol. II. p. 386.

Porro illa brevis mentio, quam Walliſius præfationi illi inſeruit, his verbis habetur: *In ſecundo volumine inter alia habetur Newtoni methodus de Fluxionibus, ut ille loquitur, conſimilis naturæ cum Leibnitii, ut hic loquitur, calculo differentiali; quod, qui utramque methodum contulerit, ſatis advertat; utut ſub loquendi formulis diverſis: quam ego deſcripſi (Algebræ, Cap.* XCI. *&c. præſertim, Cap.* XCV.) *ex binis Newtoni literis, aut earum alteris,* Junii 13, *& Octob.* 24, 1676, *ad Oldenburgium datis, cum Leibnitio communicandis; iiſdem ferè verbis, ſaltem leviter mutatis, quæ in illis literis habentur, ubi* METHODUM HANC LEIBNITIO EXPONIT, *tum ante* DECEM ANNOS *nedum plures* [id eſt anno 1666 vel 1665] *ab ipſo excogitatam. Quod moneo, ne quis cauſetur, de hoc calculo differentiali nihil à nobis dictum eſſe.*

His ad hunc modum actis, anni ſequentis menſe Junio, editores Actorum Lipſienſium, vel potius, ut ex ſtylo colligitur, ipſe Leibnitius, cùm de duobus prioribus Walliſii tomis narrationem contexunt, hujus in præfatione clauſulæ mentionem fecerunt; queſtique ſunt, non quòd dixerit Newtonum, in duabus illis epiſtolis, explicuiſſe Leibnitio fluxionum methodum, decennio antè vel amplius à ſe inventam; ſed quòd, de calculo differentiali verba faciens, eamque ut ait ob rationem, *ne quis cauſetur de calculo differentiali nihil ab ipſo dictum fuiſſe*, non monuerit lectorem, jam tum Leibnitium calculum illum penes ſe habuiſſe, cùm mutuæ illæ inter ipſum & Newtonum literæ, Oldenburgii operâ, hinc inde ſcriberentur. Et in pluribus poſt illa epiſtolis, inter Leibnitium & Walliſium de eâ re conſcriptis, non negabat Leibnitius, Newtonum, toto ante eas literas datas decennio, dictam methodum inveniſſe; id quod affirmaverat ibi Walliſius: non præ ſe ferebat, tam maturè ſe ſuam methodum excogitâſſe; nullo argumento probabat, ſe ante annum 1677 in eam incidiſſe; neque id ipſum probabat, niſi ex Newtoni conceſſo; non affirmabat ipſe, ſe maturiùs habuiſſe; laudabat Newtonum, quòd in hâc re tam candidè egerit; concedebat, utramque methodum eodem in ſummâ recidere; ſeque idcirco ſolitum, communi Analyſeos infiniteſimæ nomine utramque indigitare: adjiciebat, ſicuti Vietæ Carteſiique methodi, communi Analyſeos ſpecioſæ nomine ferebantur, licet in aliquibus differrent; ita fortè ſuam Newtonique methodos

thodos in aliquibus differre poſſe: nihilque ſibi vindicabat, præterquam illa in quibus, ut ipſi videbatur, inter ſe differebant, notatione ſcilicet, æquationibus differentialibus & æquationibus exponentialibus. In epiſtolâ tamen 21 *Jun.* 1677, æquationes differentiales ſibi ac Newtono communes eſſe exiſtimabat. N° LXVI.

Hic erat, eo tempore, inter Walliſium & Leibnitium controverſiæ ſtatus. Quadriennio vero pòſt, cùm D. Fatius ſuſpicionem injecerat, poſſe fieri ut Leibnitius, ſecundus calculi inventor, à Newtono, primo ejus ante multos annos inventore, nonnihil ſurripuerit; Leibnitius in reſponſo ſuo, in Actis Eruditorum *Maii* 1700 edito, concedebat, Newtonum ſuâ ſolâ Minervâ methodum excogitâſſe; neque negabat, Newtonum multis annis ſe priorem in eam incidiſſe: neque plus ſibi arrogabat, quàm ſe quoque, propriâ Minervâ, ac ſine ope Newtoni, eandem repperiſſe; præque ſe ferebat, tum cùm primùm eam typis ederet, neſcîſſe ſe quicquam præter methodum tangentium à Newtono inventum eſſe. Cúmque de ſublimi quâdam parte methodi loqueretur, quâ Newtonus A. C. 1686, Solidum minimæ reſiſtentiæ invenerat, hæc addidit: *Quam, inquit, methodum ante D. Newtonum & me nullus, quod ſciam, geometra habuit; uti ante hunc, maximi nominis geometram,* NEMO, *ſpecimine publicè dato, ſe habere probavit; ante dominos Bernoullios & me nullus communicavit.* Huc uſquè igitur inventoris primi nomen minimè ſibi vindicavit Leibnitius; non auſus id facere ante obitum Walliſii, poſtremi illorum ſenum, qui, quæ inter Anglos & Leibnitium per annos quadraginta acta erant, optimè noverant. Deceſſit autem Walliſius menſe *Octobri* 1703; Leibnitius verò ſibi demum arrogare hoc cœpit *Januario* 1705.

Newtonus tractatum ſuum de Quadraturis edit 1704; is vero* diu ante editionem ſcriptus erat. Quippe plurima ex eo citata ſunt in epiſtolis 24 *Octob.* & 8 *Novemb.* 1676. Spectat autem ad methodum fluxionum; & ne pro novo opere haberetur, iterabat id Newtonus, quod ante annos novem à Walliſio publicatum erat, nullo tum contradicente, hanc nempe methodum gradatim fuiſſe repertam, annis 1665 & 1666. Jam autem actorum Lipſi-

* De hoc tractatu Ralphſonus in hiſtoria ſua fluxionum Cap. I. ſic ſcripſit. "Newtonus anno "1704, parvum edidit tractatum; quem, circa annum 1676, ex tractatu antiquiore extraxit; "quemque doctus Halleius & ego, circa annum 1691, Cantabrigiæ in manibus noſtris habuimus."

enſium editores (hoc eſt, ipſe Leibnitius) cùm de tractatu hoc
N° LXXIX. agerent, affirmabant Leibnitium fuiſſe primum ejus methodi inventorem, & Newtonum pro differentiis fluxiones ſubſtituiſſe. Atque hæc affirmatio ortum dedit præſenti controverſiæ.

N° LXXIX. Quippe D. Keillius, in epiſtolâ in Tranſactionibus Philoſophicis editâ, retorſit in eos hoc telum : *Fluxionum, inquiens, Arithmeticam ſine omni dubio primus invenit D. Newtonus ; ut cuilibet ejus epiſtolas, à Walliſio editas, legenti facilè conſtabit. Eadem tamen Arithmetica poſtea, mutatis nomine & notationis modo, à D. Leibnitio in Actis Eruditorum edita eſt.*

Newtonus, priuſquam vidiſſet id, quod in actis Leipſienſibus publicatum fuerat, ægrè tulit à D. Keillio hoc dictum eſſe ; ne fortè inde lis aliqua naſceretur. Leibnitius quoque, hoc acerbius interpretans quàm vel à Keillio cogitatum fuerat, in literis ad D.
N° LXXX. Sloane datis 4 *Martii* 1711, de hoc ut calumniâ queſtus eſt ; petiitque, ut Regia Societatis injungeret Keillio, palinodiam ut publicè caneret. Keillius verò id quod ſcriptum erat probaturum ſe ac defenſurum profitetur ; licentiâ à Newtono, cui quod in Actis Lipſienſibus dictum eſt oſtendebatur, impetratâ. Leibnitius au-
N° LXXXV. tem, in alterâ ad D. Sloane epiſtolâ 29 *Decemb.* 1711 datâ, neglectâ accuſationis ſuæ probatione, candorem modò ſuum prædicare, de quo vel dubitare incivile foret ; non modum oſtendere, quo methodum inveniſſet ; in Actis Lipſienſibus ſuum cuique datum eſſe ; ſe inventionem novem annis (ſeptem credo dicere debuit) penes ſe celaviſſe, ne quiſquam (Newtonum intelligit) eam ſibi præripuiſſe glorietur ; Keillium eſſe hominem juvenem, rerum anteactarum ignarum ; dixiſſe illud, Newtono nolente ; rixoſum porro hominem eſſe, cui ſilentium imponi debeat ; ſe cupere, ut Newtonus ipſe ſententiam de hâc re ſuam pronuntiaret. Atqui ſatis noverat, nihil amplius Keillium dixiſſe, quàm quod tredecim ante annis, nullo tum contra eunte, dixerat Walliſius : noverat Newtonum ſententiam de hâc re tuliſſe, in introductione ad librum Quadraturarum, priùs in lucem editum, quàm hæc lis moveretur. Walliſius verò jam ad plures abierat : qui reſtabant in Angliâ Mathematici, pro noviciis habentur : de cujuſvis candore Leibnitius jure ſuo dubitare volet ; Newtonus denique, niſi

vel diſſimulaverit rem, vel abnegaverit, in rixas & moleſtias trahendus eſt.

Regia itaque Societas, cujus auctoritati non minus Leibnitius, quàm Keillius (uterque ſcilicet in ea Socii) parere debebant, bis à Leibnitio huc provocata, nefaſque eſſe exiſtimans, vel damnare vel notare Keillium, re nondum examinatâ; ſcienſque nec Newtonum neque Leibnitium, qui in vivis ſoli vel ſcirent quid, vel meminiſſent, quod in his rebus ante annos quadraginta actum ſit, in hâc Keillii cauſâ teſtes eſſe poſſe; negotium id dederunt numeroſo ex Societate conſeſſui, ut excuterent veteres in Archivis ſuis Epiſtolas & Chartas, & quid in eis de hoc negotio reperiſſent, So- N°LXXXVI. cietati exponerent: quam expoſitionem ut primùm Societas acceperat, & ipſam, & Epiſtolas, Chartaſque ipſas, in publicum edi juſſit. Ceterùm ex illis id Conſeſſui compertum viſum eſt, Newtonum anno 1669, vel antea, methodum illam penes ſe habuiſſe; Leibnitium verò non ante annum 1677.

Ut methodi differentialis primum ſe auctorem venditaret Leibnitius, inſimulavit Newtonum, literâ *o*, more vulgari, pro dato incremento $\tau\tilde{\varepsilon}$ *x* primò fuiſſe uſum; qui mos differentialis methodi utilitates tollit: poſt edita verò Principia mutaviſſe *o* in $\dot{x}$, ſubſtituendo $\dot{x}$ pro *dx*. Hoc verò nunquam quis probaverit; Newtonum unquam *o* in $\dot{x}$ mutaviſſe, vel uſurpâſſe $\dot{x}$ pro *dx*, vel omiſiſſe uti literâ *o*. Newtonus in Analyſi, anno 1669 vel antea ſcriptâ, & in libro de Quadraturis, & in Principiis Philoſophiæ uſus eſt literâ *o*; atque adhuc utitur, eodem planè quo priùs ſenſu. In libro de Quadraturis uſus eſt literâ *o* unà cum ſymbolo $\dot{x}$; ideoque non poſuit unum loco alterius. Symbola iſta, *o* & $\dot{x}$, pro rebus diverſi generis poſita ſunt. Prius eſt momentum; alterum fluxio eſt, ſive velocitas; ut ſuprà eſt explicatum. Cùm litera *x* pro quantitate uniformiter fluente ponitur, ſymbolum $\dot{x}$ eſt unitas; & litera *o* (ſeu $1\dot{x}o$) momentum; atque $\dot{x}o$ & *dx* idem ambo momentum ſignificant. Literæ punctatæ numquam indicant momenta; niſi cum multiplicantur per momentum *o*, vel expreſſum vel ſubintellectum, quo infinitè parvæ evadant; & tum rectangula pro momentis ponuntur.

Newtonus non in formis ſymbolorum ſuam methodum conſtituit, neque ſe alligat ad ullam unam ſpeciem ſymbolorum pro fluentibus

fluentibus & fluxionibus. Ubi areas Curvarum pro fluentibus ponit, ſæpe ponit ordinatas pro fluxionibus, & fluxiones denotat per ſymbola ordinatarum; ut in Analyſi ſuâ fecit. Ubi lineas pro fluentibus ponit, quævis ſymbola ponit pro velocitatibus punctorum lineas deſcribentium, hoc eſt, pro fluxionibus primis; & quævis alia ſymbola, pro incremento earum velocitatum, hoc eſt, pro fluxionibus ſecundis; ut ſæpe fit in Principiis Philoſophiæ. Ubi autem literas x, y, z, pro fluentibus ponit; earum fluxiones denotat vel per alias literas, ut p, q, r; vel per eaſdem literas aliâ formâ poſitas, ut X, Y, Z; vel $\dot{x}$, $\dot{y}$, $\dot{z}$, punctatas; vel per quaſvis lineas, ut DE, FG, HI, conſideratas tamquam earum exponentes. Atque hoc quidem manifeſtum eſt ex libro ejus de Quadraturis: ubi, in primâ propoſitione, fluxiones indicat per literas punctatas; in ultimâ propoſitione, per ordinatas Curvarum; & in introductione, per alia ſymbola, dum methodum explicat, illuſtratque per exempla. Leibnitius, in ſuâ methodo, nulla fluxionum ſymbola habet; & idcirco Newtoniana fluxionum ſymbola ſunt in eo genere prima. Leibnitius ſymbolis illis momentorum, ſive differentiarum, dx, dy, dz, primò uti cœpit anno 1677: Newtonus momenta denotabat per rectangula ſub fluxionibus & momento o, cùm Analyſin ſuam ſcriberet, anno 1669 vel antea. Leibnitius ſymbolis $\int x$, $\int y$, $\int z$, pro ſummis ordinatarum uſus eſt, jam inde ab anno 1686: Newtonus, in Analyſi ſuâ, eandem rem denotavit, inſcribendo ordinatam in quadrato vel rectangulo, ad hunc modum; $\boxed{\frac{aa}{64X}}$. Omnia Newtoni ſymbola ſunt in ſuo quæque genere prima.

N° XII.

N° VIII.

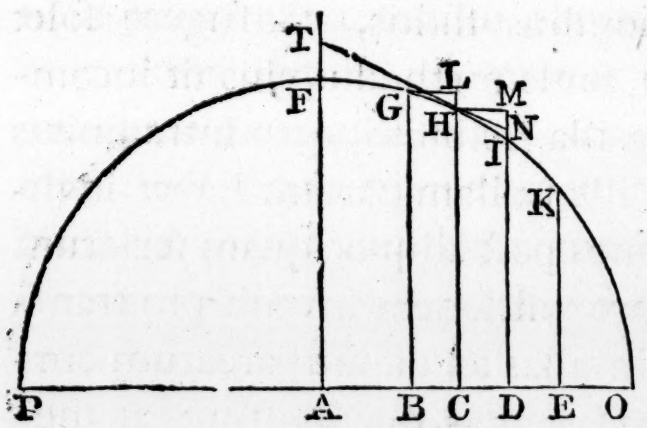
Quandoquidem autem inſimulatum eſt, uſum literæ o vulgarem eſſe, ac methodi differentialis utilitates tollere; è contrario, fluxionum methodus, prout à Newtono uſurpata eſt, omnes differentialis methodi utilitates habet, & præterea alias. Elegantior eſt; quippe in ejus calculo una tantùm infinitè parva quantitas eſt per ſymbolum denotata, idque ſymbolum eſt o. Nullas quantitatum infinitè parvarum ideas habemus: & idcirco in ſuam methodum fluxiones introduxit Newtonus, ut, quantum fieri poſſit, per finitas quantitates procederet. Naturalis magis eſt magiſque geometrica; fundata ſcilicet ſuper primis quantitatum naſcentium

centium rationibus, quæ exiſtentiam in geometria habent: cùm indiviſibilia contrà, ſuper quibus fundata eſt differentialis methodus, nullam exiſtentiam habeant, nec in geometriâ neque in naturâ. Sunt quidem rationes primæ quantitatum naſcentium; at non ſunt quantitates primæ naſcentes. Natura quantitates generat per continuum fluxum, ſive increſcentiam: talemque arearum & Solidorum generationem admiſerunt veteres Geometræ; cùm lineam unam in aliam ducerent, per motum localem, ad generandam aream; atque aream in lineam per motum localem ducerent, ad generandum Solidum: at computatio indiviſibilium, ut inde componatur area vel Solidum, nunquam in hunc uſque diem in geometriâ locum habuit. Porro Newtoniana methodus utilior quoque eſt illâ alterâ, atque certior; quippe adaptata & ad promptè inveniendam propoſitionem per tales approximationes, quales in concluſione nullum errorem creent, & ad eam exactè demonſtrandam: Leibnitiana verò methodus ad inveniendam tantùm propoſitionem, non ad demonſtrandam accommodata eſt. Cùm operatio non ſuccedat in æquationibus finitis, confugere ſolet Newtonus ad ſeries convergentes; unde methodus ejus fit incomparabiliter magis univerſalis, quàm illa Leibnitii, quæ intra finitas æquationes terminatur: ſiquidem ille nullam partem habet in infinitarum ſerierum methodo. Annos poſt aliquot quàm ſerierum methodus inventa eſt, Leibnitius propoſitionem invenit pro tranſmutandis curvilinearibus figuris, in alias æqualium arearum curvilineares, ut inde per ſeries convergentes quadrentur; at methodi figuras illas alias, per tales ſeries, quadrandi non erant Leibnitii. Ope novæ illius Analyſeos, majorem illarum propoſitionum partem, quæ in Principiis Philoſophiæ habentur, invenit Newtonus. At cùm antiqui Geometræ, quo certiora omnia fierent, nihil in geometriam admiſerint, priuſquam ſyntheticè demonſtratum eſſet; idcirco propoſitiones ſuas ſyntheticè demonſtravit Newtonus, ut cœlorum ſyſtema ſuper certâ geometriâ conſtitueretur. Atque ea cauſa eſt, cur homines harum rerum imperiti analyſin latentem, cujus ope propoſitiones illæ inventæ ſunt, difficulter admodum perſpiciant.

Inſimulatum eſt, Newtonum, in ſcholio ſub finem libri de Quadraturis, poſuiſſe tertium, quartum, quintumque terminos ſeriei convergentis

convergentis reſpectivè æquales ſecundæ, tertiæ, quartæque differentiis primi termini; & proinde methodum ſecundæ, tertiæ, quartæque differentiarum tum non intellexiſſe. Atqui in primâ libri ejus propoſitione, anno 1693 à Walliſio editâ, modum oſtendit inveniendi primam, ſecundam, tertiam, ſequenteſque fluxiones in infinitum: ac proinde cùm librum eum ſcriberet, ante annum nempe 1676, omnium omnino fluxionum inveniendarum methodum intellexit; & conſequenter, omnium differentiarum. Quòd ſi eam non intellexit, cùm anno 1704 ſcholium illud in fine libri ſubjunxerit: neceſſe eſt hoc eo contigiſſe, quòd per iſtorum annorum intervallum de memoriâ fortè ei exciderat. Hoc ſolum igitur diſquirendum eſt, oblitus ne fuit methodi ſecundarum tertiarumque differentiarum ante annum 1704.

Principiorum Philoſophiæ libri ſecundi propoſitione decimâ, cùm exponeret utilitates aliquot terminorum convergentis ſeriei ad ſolvenda problemata, hoc docet Newtonus: Si primus nempe ſeriei terminus repræſentet ordinatam, CH, cujuſcumque curvæ lineæ, PHQ; & CHDM ſit parallelogrammum infinitè exile, cujus latus, DM, ſecet Curvam in I, & tangentem ejus, HN, in N; tum ſecundus ſeriei terminus repræſentabit lineam MN, & tertius terminus lineam NI. Atqui linea NI dimidium tantùm eſt ſecundæ differentiæ ordinatæ: & proinde, cùm Principia ſua Newtonus ſcriberet, tertium terminum ſeriei æqualem poſuit dimidio ſecundæ differentiæ termini primi: & conſequenter, non oblitus tum erat methodi differentiarum ſecundarum.



Dum in eo opere verſaretur, ſæpiſſimè ei conſiderandum erat incrementum vel decrementum velocitatum, quibuſcum quantitates generantur; inque eâ re rectè argumentatur. Atqui incrementum illud, vel decrementum, eſt ipſa ſecunda fluxio quantitatis: non ergo oblitus tum erat methodi fluxionum ſecundarum.

Anno 1692 Newtonus, à Walliſio rogatus, miſit ei propoſitionem primam libri de Quadraturis, cum exemplis ejus in primis ſecundis tertiiſque fluxionibus; id quod cuivis videre eſt in Walliſii operum tomo ſecundo (anno 1693 edito) pag. 391, 392, 393

393 & 396. Ideoque ne tum quidem oblitus erat methodi ſecundarum fluxionum.

Nec ſane veriſimile eſt, ſe anno 1704, cùm dictum ſcholium adderet fine libri de Quadraturis, oblitum eſſe non ſolùm primæ ipſius illius libri propoſitionis, ſed & ultimæ quoque, ad quam ſcholium iſtud ſubtextum erat. Si vocula *ut*, quæ in ſcholio illo inter verba *erit* & *ejus* caſu aliquo excidiſſe potuit, ibi reponatur; tum ſcholium iſtud & duabus illis propoſitionibus, & ceteris Newtoni ſcriptis, congruet: & fruſtrà omnino erunt, qui oblivionem hîc cavillantur.

Atque hactenus de naturâ atque hiſtoriâ harum methodorum egimus: porro haud abs re fuerit, de toto hoc negotio obſervationes pauculas ſubjungere.

In Commercio hoc Epiſtolico, tres memorantur Leibnitii tractatus; ſcripti nempe omnes poſtquam exemplar Principiorum Newtoni Hanoveriam ei miſſum fuerat; poſtquam viderat quoque ejuſdem libri recenſionem, in Actis Eruditorum *Jan.* & *Feb.* 1689. In his verò Leibnitii tractatibus, primariæ Newtoniani libri propoſitiones novo modo recomponuntur, Leibnitioque arrogantur; quaſi priùs eas ipſe invenerat, quàm Newtoni liber ederetur. Quis teſtem in ſuâ ipſius cauſâ patienter ferat? Vel fidem faciat Leibnitius, ſe, ante Newtoni librum editum, eas excogitâſſe; vel de eis ſibi vindicandis pudorem habeat. N° LXXII.

In tractatuum illorum poſtremo, viceſima propoſitio (quæ omnium Newtonianarum primaria eſt) Corollarium fit propoſitionis decimæ nonæ. Atqui decima illa nona demonſtrationem ſibi annexam habet παράλογον & falſam. Aut evincat itaque Leibnitius, demonſtrationem illam non falſam eſſe; aut fateatur ſe 19 & 20 propoſitiones non ejus demonſtrationis ope repperiſſe; ſed, quo Newtoni eximiam illam propoſitionem pro ſuâ venditaret, demonſtrationem ejus extundere fruſtrà tentaviſſe. Quippe in xx Propoſitione præ ſe fert, neſciſſe ſe, quâ eam viâ Newtonus invenerat; ut fidem ſcilicet lectori faceret, ſe, ſine illius ope, eandem repperiſſe.

Ex erroribus in xv^a & xix^a Leibnitii propoſitione commiſſis, oſtenderat Keillius, Leibnitium, cùm tres illos tractatus ſcriberet, operandi vias in ſecundis differentiis non optimè calluiſſe. Id

quod ampliùs adhuc conſtat, ex illius Tractatûs tertii Propoſitionibus X, XI, & XII. Has enim conſtituit ceu fundamentum infiniteſimalis ſuæ Analyſeos, in conſiderandis viribus centrifugis; & decimam quidem proponit in relatione ad centrum curvitatis orbitæ; in undecimâ tamen & duodecimâ eam adhibet in relatione ad centrum circulationis. Cùm hæc duo diverſa centra confuderit, in fundamentalibus his propoſitionibus, ſuper quibus calculum ſuum ſtruebat; non potuit fieri, quin in ſuperædificando peccaret: neque ex erroribus illis extricare ſe valuit, per ignorantiam ſuam in ſecundis tertiiſque differentiis. Atque hoc ulteriùs conſtat, ex ſexto ſecundi tractatûs articulo. Quippe in iſto articulo lapſus eſt Leibnitius; peccatumque eo admiſit, quòd neſciret ſecundas tertiaſque differentias rectè tractare. Cùm hos itaque tractatus componeret, in diſcipulorum adhuc claſſe verſabatur; idque eum decet, ſi pudor eſt, candidè fateri.

Omnino ergo veriſimile eſt, quemadmodum ex dictis tribus Newtoni epiſtolis, cum Barrovianâ tangentium methodo comparatis, differentialem illam methodum extuderat Leibnitius; ita decennio pòſt, cùm Newtoni Principia Philoſophiæ in publicum prodirent, aliquatenus eum in illâ progreſſum eſſe; dum tentaret dictam methodum ad primarias Newtoni propoſitiones extendere; & eâ occaſione tres illos tractatus conficeret. Quippe propoſitiones, quæ in illis habentur, ſi errores & quiſquilias dempſeris, omnino vel Newtonianæ ſunt, vel ut corollaria ex eis facilè deducendæ; in aliâ ſcilicet verborum formâ, re tamen non diverſâ, jam antè à Newtono publicatæ. Has tamen Leibnitius venditavit, tamquam à ſe ſolo diu antè inventas, quàm à Newtono ſint editæ. Nempe in extremo primi tractatûs, ſe inveniſſe eas fingit, antequam Newtoniana Principia prodiiſſent; immo nonnullas ex eis, antequam ipſe Pariſiis diſceſſiſſet, hoc eſt, ante *Octobrem* anni 1676. Tractatum autem ſecundum claudit his verbis: *Multa ex his deduci poſſunt praxi accommodata; ſed nobis nunc fundamenta geometrica jeciſſe ſuffecerit, in quibus maxima conſiſtebat difficultas: & fortaſſis, attentè conſideranti, vias quaſdam novas, ſatis antea impeditas, aperuiſſe videbimur. Omnia autem reſpondent noſtræ Analyſi infinitorum; hoc eſt calculo ſummarum & differentiarum, cujus elementa quædam in his Actis dedimus, communibus quoad*

licuit

licuit verbis hîc expreſſo. In his, ut vides, jactat Leibnitius, ſe primum *fundamenta geometrica, in quibus maxima conſiſtebat difficultas*, in hoc ipſo tractatu ſecundo poſuiſſe; ſeque ſolum *vias quaſdam novas ſatis antea impeditas* in tractatu eodem aperuiſſe: cùm tamen priùs fermè biennio prodiiſſent Newtoni Principia, atque in hoc ipſo tractatu edolando, ſubſidio fuiſſent Leibnitio; quin & *communibus quoad licuit verbis* compoſita eſſent; atque omnia iſta fundamenta omneſque iſtas vias novas in ſe continerent. Atque horum omnium conſcius erat Leibnitius, tum cùm tractatum illum ederet; ultroque tum agnoſcere & prædicare debebat, Newtonum fuiſſe, qui *Fundamenta Geometrica in quibus maxima conſiſtebat difficultas* primus poſuerit; *qui vias novas, ſatis antea impeditas*, primus expediverit. Atque hæc quidem omnia quodammodo agnoſcebat in reſponſo ad D. Fatium; *Quam methodum*, inquiens, *ante dominum Newtonum & me, nullus quod ſciam geometra habuit*; *uti ante hunc, maximi nominis geometram*, NEMO, SPECIMINE *publicè dato, ſe habere* PROBAVIT. Atqui quod eâ occaſione tam liberè faſſus eſt Leibnitius; ſi candor, ſi honor ei conſtet, ubique ac ſemper profiteri debet.

In epiſtolâ ſuâ 28 *Maii* 1697, ad Walliſium ſcriptâ, ſic narrat Leibnitius: *Methodum, inquit, fluxionum profundiſſimi Newtoni cognatam eſſe methodo meæ differentiali, non tantùm animadverti, poſtquam opus ejus* [Principiorum ſcilicet] *& tuum prodiit; ſed etiam profeſſus ſum in Actis Eruditorum, & alias quoque monui. Id enim candori meo convenire judicavi, non minùs quàm ipſius merito. Itaque communi nomine deſignare ſoleo Analyſeos Infiniteſimalis; quæ latiùs quàm tetragoniſtica patet. Interim quemadmodum & Vietæa & Carteſiana methodus Analyſeos Specioſæ nomine venit, diſcrimina tamen nonnulla ſuperſunt; ita fortaſſe & Newtoniana & Mea differunt in nonnullis.* Et in his quoque profitetur Leibnitius, cùm Newtoni Principia prodiiſſent, ſe percepiſſe ſtatim affinitatem, quæ inter geminas methodos intercedit; & idcirco communi ſe utramque infiniteſimalis methodi nomine vocitare; quin & candoris ſui eſſe, ut affinitatem illam agnoſcat. Atqui ſi pro homine candido haberi ſe poſtulat; idem hoc, quod agnovit olim, & nunc debet agnoſcere. Quin & fatetur methodum Newtonianam eo ferè gradu ſuæ methodo præiviſſe, quo Vietæam Carteſianæ: N° LXXVI.

fianæ: atque ut inter has, ſic inter ſuam & Newtoni, diſcrimina quædam manere: & deinde ea enumerat, quibus methodum Newtoni ampliâſſe, & promoviſſe ſe arbitratur. Atqui temporis illam prærogativam, quam tum, apud Walliſium, Newtono concedebat; etiam adhuc, & apud ſuos, debet concedere.

Cùm diſcrimina illa, ſive augmenta, Newtoni methodo à ſe addita memorat Leibnitius; in ſecundo loco ponit Differentiales Æquationes. Atqui epiſtolæ illæ, quæ anno 1676 inter ipſos interceſſerunt, clarè monſtrant Newtonum eo tempore differentiales iſtas habuiſſe; Leibnitium verò minimè. In tertio loco recenſet Æquationes Exponentiales: atqui has quoque Anglis debet Leibnitius. Walliſius, in ſerierum interpolatione, conſideravit fractos & negativos dignitatum indices: Newtonus in computationes analyticas fractos (a), ſurdos, negativos, & indefinitos dignitatum
N° LXIV. indices introduxit; & epiſtolâ 24 *Octob.* 1676 certiorem fecit Leibnitium, ad affectas æquationes, quæ dignitates eas, quarum indices fracti vel ſurdi erant, involverent, ſuam methodum ſe ex-
N° LXIX. tendere. In reſponſo autem, 21 *Jan.* 1677 dato, viciſſim petit à Newtono Leibnitius, ut dicere vellet quid de reſolutione æquationum ſentiret, involventium dignitates quarum indices eſſent indeterminati; quales hæ eſſent, $x^y + y^x = xy$. $x^x + y^y = x + y$. Atqui has ipſas æquationes nunc exponentiales nominat; ſeque orbi literato venditat primum earum inventorem; hancque ut eximiam quandam inventionem oſtentat: nec tamen vel agnovit hactenus auxilia, ad rem eam inveniendam, à Newtono ſibi ſubminiſtrata; nec vel uno exemplo utilitatem ejus, ubi dignitatum indices ſint fluentes, oſtendere valuit. Cùm autem, ut credibile eſt, nondum eam, pro ſolitâ ſuâ impatientiâ, præque talis exempli inopiâ abjecerit; æquum eſt, ut ſperemus tandem eum aliquando mirificam ejus utilitatem publicè oſtenſurum eſſe.

N° LXIV. In epiſtolâ ad Leibnitium, 24 *Octob.* 1676 datâ, Newtonus dixerat, ſe binas, inverſa tangentium problemata & alia ejuſmodi difficilia reſolvendi, methodos habere; quarum unam conſiſtere *in aſſumendo ſeriem pro quâvis ignotâ quantitate, unde ceteræ commodè poſſent deduci*; & *in conferendo homologos terminos ſuborientis æquationis, pro determinandis aſſumptæ ſeriei terminis.* Quid hîc

(a) Vide Tom. I. p. 10, Not. [h].

facit

facit Leibnitius? Multos post annos, in Actis scilicet Eruditorum *Augusti* 1693, hanc methodum tamquam suam publicat, ejusque primam sibi inventionem arrogat. Aut publicè verò huic abrenuntiet; aut argumentis vincat se eam inveniisse, priusquam dictas Newtoni literas acceperit.

Illud quoque publicè ei confitendum est; se Oldenburgii epistolam 15 *Aprilis* 1675 accepisse; quâ plurimæ series convergentes pro Curvis quadrandis, & imprimis illa Jacobi Gregorii pro arcu dati tangentis inveniendo, atque inde circulo quadrando, continebantur. Hoc quidem privatim fassus est, epistolâ ad Oldenburgium, propriâ manu 20 *Maii* 1675 scriptâ, quæque etiam adhuc supereſt in libro Regiæ Societatis epistolari: nondum tamen publicè agnovit; ut tum sane factum oportuit, cùm illam ipsam seriem ut suam maluit edere. N° XXXVII.

Porro illud quoque agnoscendum ei est publicè; se extracta epistolarum Gregorianarum accepisse, quæ ipsius rogatu Parisios ei misit Oldenburgius mense *Junio* 1676; in quibus erat una Gregorii, de istâ serie 15 *Feb.* 1671; & Newtoni alia, de Methodo Fluxionum, 10 *Decemb.* 1672. N° XLVI. N° XLIII.

Quandoquidem autem in epistolâ, 28 *Dec.* 1675, Oldenburgio significavit Leibnitius, se seriem illam cum amicis Parisiensibus, biennio antè, communicâsse; deque eâ re aliquoties ad ipsum scripsisse: in aliâ item epistolâ, 12 *Maii* 1676, se de serie illâ ante aliquot annos ad ipsum literas dedisse: porro in aliâ, 27 *Aug.* 1676, se seriem illam amicis ostendisse triennio antè & ampliùs, hoc est, quàm primùm Parisios à Londino veniisset: illud à Leibnitio jure expectamus, ut dicat quî evenerit, ut, cùm Oldenburgii epistolam 15 *Apr.* 1675 acciperet, illam ipsam seriem esse suam ignoraverit. N° XLIV. N° LII. N° XLII.

In epistolis, 15 *Jul.* & 26 *Octob.* 1674 datis, non nisi unam seriem memorat Leibnitius pro circuli circumferentiâ; methodumque, quâ ad hanc pervenerit, sibi etiam seriem obtuliisse dicit, pro arcu cujus sinus datus fuerit, etsi arcûs proportio ad circumferentiam totam sit ignota. Ergo ista methodus, ex dato triginta graduum sinu, seriem ei suppeditavit pro circumferentiâ totâ. Quòd si seriem quoque habuit pro totâ circumferentiâ, à XLV graduum tangente deductam; rogatur ut publicè doceat, quâ methodo, quæ ambas istas series ei dare posset, eo tempore sit usus: N° XXXII, XXXIII.

uſus: cùm methodus per figurarum tranſmutationem nequaquam hoc efficere valeat. Rogatur inſuper, ut rationem reddat, cur in iſtis epiſtolis non niſi unam circuli quadraturam memoret.

Porro ſi anno 1674 jam tum demonſtrationem habuit ſeriei pro inveniendo, cujus ſinus datus ſit, arcu; rogatur, ut eam in pub-
N° XLIV. licum proferat; dicatque, cur in epiſtolâ 12 *Maii* 1676 ab Oldenburgio peteret, ut ille Newtoni demonſtrationem pro ipſâ illâ ſerie à Collinio adipiſceretur; & quâ tandem re Newtoniana ſeries à ſuâ illâ differat. Quippe ex his omnibus non levis ſuſpicio oritur, Newtonianam ſeriem, pro reperiendo cujus ſinus datus ſit arcu, Leibnitio dum in Angliâ commoraretur, eſſe traditam: illumque poſtea, 1673, Pariſienſibus eam amicis pro ſuâ venditâſſe; proximoque anno, etiam ad Oldenburgium, quaſi de ſuâ literas dediſſe, quo demonſtrationem, ſive methodum ſerierum ejuſmodi inveniendarum, expiſcaretur. Anno verò inſequente, cùm Oldenburgius & iſtam, de quâ loquimur, ſeriem, & illam Gregorianam, & ſex præterea alias ad ipſum miſiſſet; non diutiùs eam ſeriem arrogare ſibi Leibnitius ſuſtinuit, inopiâ demonſtrationis: ſeque dixit, ſeries iſtas lentè examinare, cumque ſuis comparare; quaſi ſuæ illæ à ſeriebus ex Angliâ miſſis eſſent diverſæ. Denique, cùm ſeriei Gregorianæ demonſtrationem extudiſſet per figurarum tranſmutationem, Pariſienſibus eam ſeriem amicis velut ſuam oſtentare cœpit; ut ipſe in Actis Eruditorum *Apr.* 1691, pag. 178 narrat; *Jam anno*, inquiens, 1675 *compoſitum habebam opuſculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum, &c.* At amicos iſtos celavit epiſtolam, quâ per Oldenburgium illam ſeriem nactus eſt; ipſique adeo Oldenburgio aſſeveravit, ſe, uno alterove anno ante ſcriptam ejus epiſtolam, ſeriem iſtam repperiſſe. Porro & ſequente anno, cùm binas Newtoni ſeries per
N° XLIV. Gregorium quendam Mohr iteratò accepiſſet, ſic de eis ad Oldenburgium ſcripſit, ut numquam ſibi antea viſis; ab eóque petiit, ut, per Collinium, Newtoni, pro eis inveniendis, methodum nanciſceretur. Cæterùm hanc gravem ſuſpicionem ſi eluere volet Leibnitius; illud imprimis argumentis certis oſtendat, ſe ſeriem iſtam Gregorianam, priuſquam per Oldenburgium eam accepiſſet, ſuo ſolius acumine repperiſſe.

Hoc

Hoc quoque, prout æquum eſt, monſtrabit Leibnitius; quâ primùm methodo diverſas illas regreſſionis ſeries, pro circulo & hyperbolâ, à Newtono quidem ad ipſum miſſas 13 *Jun.* 1676, at in epiſtolâ ſuâ 27 ſequentis *Auguſti* ſibi attributas, invenerit, antequam à Newtono eas accepiſſet. N° XLIX. N° LII.

Cúmque ab ipſo rogatus Newtonus regreſſionis methodum ei indicaviſſet; quam ut primùm legit Leibnitius, neque ſuam eſſe agnovit, & ne intellexit quidem: poſtea verò quàm percipere eam potuit, ut ſuam ſibi arrogavit, olim ſcilicet à ſe inventam, ſed in chartis ſuis reconditis oblivione ſepultam: vel probet Leibnitius, ſi candidi æquique hominis nomen cupit auferre, ſe primum ejus methodi inventorem eſſe, vel vero inventori concedat. N° LXIV. N° LXX.

In literis ad Oldenburgium datis 3 *Feb.* $167\frac{2}{3}$ proprietatem quandam ſeriei numerorum, naturalium, triangularium, pyramidalium, trianguło-triangulorium, &c. ut inventum ſuum oſtentavit Leibnitius; quoque majorem fidem faceret, mirari viſus eſt D. Paſcalium, in triangulo ſuo Arithmetico, eam præteriſſe. Ceterùm is liber Paſcalii anno editus eſt 1665, atque iſtam ipſam ſeriei ejus proprietatem continet. Agnoſcat itaque Leibnitius, proprietatem iſtam minimè à Paſcalio fuiſſe præteritam; neque pergat ſibi vindicare, cum veri inventoris injuriâ. N° XXX.

Abrenuntiet quoque methodo differentiali Newtoni; neque ſe in partes ingerat, quaſi ſecundus ſcilicet inventor. Secundis inventoribus, etiam reverâ talibus, vel exiguus vel nullus eſt honos; tituli vel juris nihil eſt. Quid cum iſtis igitur fiet, qui vel ſecundos ſe fuiſſe, nullis certis argumentis poſſunt evincere? In literis ad D. Sloane, 29 *Decemb.* 1711 datis, amicos ait ſuos probè ſcire, quo pacto differentialem methodum invenerit. Quid amicos nobis narrat? Ipſè planè, aperte, ſine tergiverſatione dicat, quâ eam viâ reppererit. N° XXX. N° LXXXV.

In iiſdem ad D. Sloane literis narrat, ſe, novennio ante quàm eam in lucem ederet, methodo potitum eſſe; hoc eſt, anno 1675 vel priùs. Atqui certum eſt, 27 *Aug.* 1676, cùm literas ad Oldenburgium mitteret, nondum illum habuiſſe eam. Quippe ibi affirmat, problemata inverſæ tangentium methodi, plurimaque alia, non poſſe ad ſeries infinitas, neque ad æquationes, aut quadraturas

draturas reduci. Quomodo hæc duo conciliari inter ſe poſſint, ipſe, ubi otium eſt, videbit.

Jam ſuprà didicimus, Leibnitium, dum per Angliam & Hollandiam domum rediret, dediſſe operam Sluſianæ pro tangentibus methodo promovendæ, & ad omne genus problemata extendendæ; eâque cauſâ generalem tangentium tabulam conficere voluiſſe. Nondum igitur veram iſtius methodi promotionem invenerat. Atqui ſemeſtri ferè pòſt tempore, cùm in veram ejus promotionem recens inciderat, reſcripſit his verbis: *Clariſſimi Sluſii methodum tangentium nondum eſſe abſolutam, celeberrimo*
N° LXVI. *Newtono aſſentior: & jam* A MULTO TEMPORE *rem tangentium generaliùs tractavi, ſcilicet per differentias ordinatarum.* Bene ſanè, à multo tempore; nimirum ſemeſtri. Excogitet jam aliquid, pro candore ſuo, Leibnitius, cur tantillum temporis ut multum deprædicaverit; niſi eo conſilio, ut inventoris titulum Newtono præriperet; fidemque faceret, ſe, diu antequam Newtoni literis eam edoctus eſſet, differentialem methodum penes ſe habuiſſe.

In Actis Eruditorum *Junii* 1696, dum duos priores Walliſianorum Operum tomos recenſent editores (hoc eſt, ipſe Leibnitius) ita narrant: *Ceterùm ipſe Newtonus, non minùs candore quàm præclaris in rem mathematicam meritis inſignis, publicè & privatim agnovit Leibnitium, tum cùm, interveniente celeberrimo viro Henrico Oldenburgio Bremenſi Societatis Regiæ Anglicanæ tunc ſecretario, inter ipſos, ejuſdem jam tum Societatis ſocios, commercium intercederet, id eſt, jam ferè antè annos viginti & amplius, calculum ſuum differentialem, ſerieſque infinitas, & pro iis quoque methodos generales habuiſſe; quod Walliſius, in præfatione operum factæ inter eos communicationis mentionem faciens, præteriit; quoniam de eo fortaſſe non ſatis ipſi conſtabat. Cæterùm differentiarum conſideratio Leibnitiana, cujus mentionem facit Walliſius, nequis ſcilicet, ut ipſe ait, cauſaretur de calculo differentiali nihil ab ipſo dictum fuiſſe, meditationes aperuit, quæ aliunde non æquè naſcebantur.* Ex his patet à Leibnitio lectam eſſe præfationem illam Walliſii; in quâ narrat is, Newtonum, anno 1676, methodum ſuam fluxionum Leibnitio explicaviſſe; quam tamen decennio antè, vel ampliùs, Newtonus inveniſſet. Atqui à Newtono nunquam creditum eſt, Leibnitium ante annum 1677 differentialem methodum

dum inveniſſe: ipſeque adeo Leibnitius, in Actis Eruditorum *Apr.* 1691, p. 178, faſſus eſt, inventam eſſe poſtquam domum Pariſiis rediſſet ad negotia publica capeſſenda; hoc eſt, poſt annum 1676. Quod autem ad generalem ejus infinitarum ſerierum methodum attinet; tantum abeſt ut generalis dicenda ſit, ut vel exiguæ vel nullius prorſus ſit utilitatis; niſi fortè ut anſam Leibnitio præbeat, quâ Gregorianam, pro circulo quadrando, ſeriem ſibi adhamet transferatque.

In reſponſo ad D. Fatium, in Actis Erud. 1700, p. 203, edito, hæc habet Leibnitius. *Ipſe* [Newtonus] *ſcit unus omnium optimè, ſatisque indicavit publicè, cùm ſua Mathematica Naturæ Principia publicaret, anno* 1687, *nova quædam inventa geometrica, quæ ipſi communia mecum fuere,* NEUTRUM LUCI AB ALTERO ACCEPTÆ, *ſed meditationibus quemque ſuis debere*; & *à me decennio antè* [*i. e. anno* 1677] *expoſita fuiſſe.* Atqui in libro Principiorum hîc ad partes vocato, minimè agnovit Newtonus, ſuis eam methodum viribus inveniſſe Leibnitium, non à Newtonianis illis epiſtolis adjutum: Walliſiuſque nuper contrarium aſſeveraverat, refellente tum nemine, vel contradicente. Quòd ſi poſteà eam ſine ope Newtoni quàm maximè inveniſſet Leibnitius, ſecundis tamen inventoribus exilis prorſus eſt gratia, nec niſi in inferiori ſubſellio locus; ne dicam, jus omnino nullum.

In eodem ad Fatium reſponſo hæc quoque habet Leibnitius: *Certè cùm elementa calculi mea edidi anno* 1684, *ne conſtabat quidem mihi aliud de inventis ejus* [ſc. Newtoni] *in hoc genere, quàm, quod ipſe olim ſignificaverat in literis, poſſe ſe tangentes invenire non ſublatis irrationalibus; quod Hugenius quoque ſe poſſe mihi ſignificavit poſtea, etſi cæterorum ejus calculi adhuc expers. Sed majora multo conſecutum Newtonum, viſo demum libro Principiorum ejus, ſatis intellexi.* In his iterum agnovit, librum Principiorum ad Newtonianam fluxionum methodum ſibi aditum patefeciſſe: idem tamen ipſe jam negat, quicquam illius methodi in dicto libro contineri. In his ſimulat, ſe, priuſquàm iſte liber prodiiſſet, nihil amplius de Newtoni inventionibus ſciviſſe, quàm quòd methodum quandam tangentium habuerit; & ex iſto demum methodum ejus fluxionum percepiſſe: atqui in epiſtolâ, 21 *Jun.* 1677 datâ, agnovit methodum eam ad curvilinearum figurarum quadraturas

draturas se extendere, suæque similem esse. Verba ejus hæc sunt:

N° LXVI. *Arbitror quæ celare voluit Newtonus, de tangentibus ducendis, ab his non abludere. Quod addit, ex hoc eodem fundamento quadraturas quoque reddi faciliores, me in sententia hâc confirmât; nimirum semper figuræ illæ sunt quadrabiles, quæ sunt ad æquationem differentialem.*

N° XXVI, XLVI, XLVIII, LIV. Newtonus in tribus illis epistolis, quas, ut diximus, ab Oldenburgio Leibnitius acceperat, tam generalem esse suam methodum dixerat, ut ope æquationum, finitarum & infinitarum, determinaret maximas & minimas, tangentes, areas, solida contenta, centra gravitatis, longitudines ac curvitates curvarum linearum, curvilinearumque figurarum, idque sine ablatione radicalium; extenderetque se ad similia problemata in Curvis, ut vulgo vocantur, Mechanicis; itemque ad problemata tangentium inversa; & ad omnia ferè, nisi fortè numeralia quædam, qualia sunt Diophanti.

N° LI. Leibnitius verò, in epistolâ 27 *Aug.* 1676, vix credere se posse

N° XXVI. finxit, eam methodum tam esse generalem. Newtonus, in primâ ex tribus illis epistolâ, tangentium methodum proposuit, ex generali eâ methodo deductam, exemploque eam illustravit; generalisque methodi ramum, vel corollarium, esse monuit; & ejusmodi esse Slusianam, quæ nondum tum prodierat, conjecit. Hâc re excitatus Leibnitius meditabatur, siquâ viâ promovere posset methodum Slusianam, eamque ad omnia problemata extendere; quemadmodum jam antè ex ejus literis monstravimus. In tertiâ

N° LVIII. verò epistolâ suam methodum illustraverat Newtonus per theoremata pro quadraturis, & eorum exempla. Quibus adjutus Leib-

N° LXVI. nitius, in epistolâ 21 *Junii* 1677, methodum suam cum Newtonianâ congruere dixit, ducendo tangentes; producendo methodum Slusii; procedendo sine fractionum & surdarum ablatione; quadraturasque reddendo multò expeditiores. His tot & toties actis, ad conterraneos suos affirmare, se, cùm differentialem methodum anno 1684 ederet, nihil tum amplius de Newtoni invento inaudivisse, quàm quòd is methodum quandam tangentium haberet, cujus tandem est hominis?

Vide Acta Eruditorum pro mense Nov. 1684. Porro eo tempore Leibnitius de suâ methodo nihil aliud explicaverat, nisi per eam tangentes duci posse, maximasque & minimas determinari, sine ademptione fractionum vel surdarum. Hoc verò

verò totum etiam per Newtoni methodum effici posse, certo sciebat; neque candidi erat hominis id dissimulare. Cùm autem hactenus suam methodum exposuisset Leibnitius, addidit, se hîc geometriæ multo sublimioris initia posuisse, pervenientis ad difficillima quæque & utilissima problemata, quæ sine calculo differentiali, AUT SIMILI, vix solvi possint. Quid verò illud AUT SIMILI sibi vellet, quî quæso conterranei ejus, sine Œdipode, poterant intelligere? Enimvero planis disertisque verbis dictum ab eo oportuit, SIMILEM illam, quam innuit, methodum Newtoni fuisse; quàm latè ea pateret, quàm à longo tempore reperta esset, prout ipse ex Angliâ didicisset, narrare; suamque illâ posteriorem esse confiteri. Hoc omnes controversias præcidisset; hoc candidi & honesti viri officium erat. Horum tamen omnium quasi oblitus, suis ille conterraneis, in responso ad Fatium, prædicat, se cùm anno 1684 calculi sui elementa ederet, nihil tum de ullâ SIMILI methodo inaudivisse; nihil de ullâ aliâ nisi ad ducendas tangentes: quod qualis hominis fuerit, aliis dicendum relinquo.

Illud denique Leibnitio est expediendum: quî factum sit, ut in responsis suis ad Wallisium & Fatium, quorum uterque primi ejus methodi inventoris gloriam Newtono detulerat, nihil tum ipse de se, ut priore inventore, dicebat; sed senum geometrarum mortem operiebatur, aliosque, qui superstites adhuc sunt, pro novitiis habebat: quin & ipsum Newtonum adortus, cum quoquam se alio certaturum negabat. Atqui dixerat ei Newtonus, in epistolâ 24 *Octob.* 1676, se tum ante annos quinque, quo quietiùs N° LVII. ætatem ageret, consilium publicandi, quæ de hoc argumento scripserat, abjeciisse: & ex eo quidem tempore studiosè vitavit omnes, de rebus philosophicis ac mathematicis, disputationes; quin & à commercio de his rebus literario, ut disputationibus ansam porrigente, datâ operâ abstinuit; eandemque ob causam, neque de Leibnitio queri priùs sustinuit, quàm in Actis se Lipsiensibus ut plagiarium traduci vidisset; Keilliumque eo tantùm nomine in lites trahi, quòd ab hoc eum crimine vindicare conatus sit.

Insimulatum quidem est, quasi Regia Societas sententiam contra Leibnitium in hâc causâ tulisset, non utrâque parte auditâ. Non ita se res habet. Nondum sententiam tulit Societas. Leibnitius quidem postulabat à Societate, ut Keillium inauditum damnare

nare vellet: ideo ut ipſe jure eodem ſic damnari potuiſſet; cùm idem ſit jus Seio quod Titio, Keillio quod Leibnitio. Cúmque accuſationem ſuam adverſus Keillium diſtituiſſet Leibnitius, jure potuiſſet Societas notam illi inurere. Ea verò certorum tantùm hominum conſeſſum legit, qui ſcrutarentur epiſtolas atque chartas, quæ de his rebus in Archivis Societatis habentur; & ſecundum illas chartas epiſtolaſque rem ipſam, ut erat, Societati narrarent. Non enim ideo lecti erant, ut Leibnitium vel Keillium, ſed ut veteres chartas, examinarent: in eâque re probè ſe & honeſtè geſſerunt. Numeroſus quippe conſeſſus erat; è viris eruditis diverſarum nationum lectus: quorum fidem in epiſtolis chartiſque examinandis, fideliterque edendis, nihil quicquam, ullius hominis gratiâ, addendo, vel omittendo, vel mutando, Societas tota comprobavit. Quin & ipſæ epiſtolæ atque chartæ, Societatis juſſu, conſervantur adhuc; ut ſi quis velit, ibi conſuli, & cum edito Commercio Epiſtolico comparari poſſint. Illud interim ſubmonendus eſt Leibnitius; cùm id Societati impingit, quaſi inauditum eum condemnatum iſſet, id ob eam rem, per ſtatutum ejus quoddam, commeritum ſe eſſe, ut nomen ejus inde expungatur.

Philoſophia porro, quam in Principiis ſuis atque Opticis Newtonus excoluit, eſt experimentalis: illa ſcilicet, quæ cauſas rerum non fidentiùs docet, quàm per experimenta confirmari queant; neque implenda eſt opinationibus, quæ per phænomena nequeunt probari. Et idcirco in Opticis ſuis, res experimentis firmatas, ab illis quæ incertæ adhuc manent, diſtinxit Newtonus: & incertas aliquot ejuſmodi, ſub finem Opticorum, ut quærenda propoſuit. Eandemque ob cauſam, in Principiorum præfatione, cùm memorâſſet motus planetarum, cometarum, lunæ ac maris, ceu in libro illo de Gravitatis theoriâ deductos, hæc addidit: *Utinam cætera naturæ phænomena ex Principiis Mechanicis, eodem argumentandi genere, derivare liceret. Nam multa me movent, ut nonnihil ſuſpicer, ea omnia ex viribus quibuſdam pendere poſſe, quibus corporum particulæ, per cauſas nondum cognitas, vel in ſe mutuò impelluntur, & ſecundum regulares figuras cohærent, vel ab invicem fugantur, & recedunt: quibus viribus ignotis, philoſophi hactenus naturam fruſtrà tentârunt.* Et ſub finem ejus libri, in ſecundâ

cundâ editione, narrat; ut, præ inopiâ experimentorum tanto negotio ſufficientium, non aggreſſus ſit leges actionum illius ſpiritûs, ſive agentis, deſcribere, per quem efficitur hæc attractio. Quin & eandem ob cauſam de Gravitatis cauſâ nihil pronuntiat; quòd nulla experimenta, ſive phænomena, ad manum eſſent, quæ cauſam illam certò indicare poſſent. Atque hoc in Principiis ſuis, ſub ipſo initio, abunde declaraverat, his verbis: *Virium cauſas & ſedes phyſicas jam non expendo.* Et paulo pòſt: *Voces attractionis, Impulſûs, vel Propenſionis cujuſcunque in centrum, indifferenter & pro ſe mutuo promiſcuè uſurpo; has vires, non phyſicè, ſed mathematicè tantùm conſiderando. Unde caveat lector, ne per hujuſmodi voces cogitet me ſpeciem vel modum actionis, cauſamve aut rationem phyſicam alicubi definire; vel centris, quæ ſunt puncta mathematica, vires verè & phyſicè tribuere; ſi fortè aut centra trahere, aut vires centrorum eſſe dixero.* Et ſub finem Optices: *Quâ cauſâ efficiente hæ attractiones* [ſc. gravitas, viſque magnetica & electrica] *peragantur, hîc non inquiro. Quam ego attractionem appello, fieri ſane poteſt, ut ea efficiatur impulſu; vel alio aliquo modo nobis incognito. Hanc vocem attractionis ita hîc accipi velim, ut in univerſum ſolummodo vim aliquam ſignificare intelligatur, quâ corpora ad ſe mutuò tendant; cuicunque demum cauſæ attribuenda ſit illa vis: nam ex phænomenis naturæ illud nos priùs edoctos eſſe oportet, quænam corpora ſe invicem attrahant, & quænam ſint leges & proprietates iſtius attractionis, quam in id inquirere par ſit, quânam efficiente cauſâ peragatur attractio.* Pauloque inferiùs, eaſdem attractiones tamquam vires conſiderat, quas in rerum naturâ exiſtentiam habere, licet cauſæ earum nondum ſint cognitæ, per phænomena conſtat: diſtinguitque eas à qualitatibus occultis, quæ à ſpecificis rerum formis fluere exiſtimantur. Et in ſcholio ſub extremum Principiorum, cùm Gravitatis proprietates memoraſſet, hæc addidit: *Rationem verò harum Gravitatis proprietatum ex phænomenis nondum potui deducere; & hypotheſes non fingo. Quicquid enim ex phænomenis non deducitur, hypotheſis vocanda eſt; & hypotheſes, ſeu metaphyſicæ, ſeu phyſicæ, ſeu qualitatum occultarum, ſeu mechanicæ, in philoſophiâ experimentali locum non habent. ——Satis eſt, quòd Gravitas reverâ exiſtat, & agat ſecundum leges à nobis expoſitas, & ad corporum cæleſtium & maris noſtri motus omnes*

omnes ſufficiat. Jam verò, poſt hæc omnia, quæ conſulto præmonuerat Newtonus, quis non miretur, ideo eum à quóquam ſugillari, quòd cauſas gravitatis aliarumque attractionum non per hypotheſes explicet? quaſi criminis loco eſſet, certis eſſe contentum, incerta verò dimittere. Et tamen Actorum Eruditorum (anno 1714 menſe Martio, p. 141, 142) editores, id Newtono exprobrant, quòd cauſam Gravitatis neget eſſe mechanicam; aſſeruntque, ſi ſpiritus ille, vel agens, quo electrica fit attractio, non ſit æther vel ſubtilis Carteſii materia, quâvis id hypotheſi contemtius eſſe; ut fortaſſe ſit Principium Henrici Mori hylarchium. Quin & ipſe Leibnitius, in tractatu *De bonitate Dei*, & in epiſtolis ad Hartſoekerum atque alibi, Newtono id vitio vertit, quaſi Gravitatem faceret naturalem quandam & eſſentialem corporum proprietatem, immo occultam qualitatem, ac denique miraculum. Atque hujuſmodi cavillationibus, homines hi conterraneis ſuis perſuaſum eſſe cupiunt, judicio eum & acumine parum valere; neque eum eſſe, qui methodum infiniteſimalem, rem tam arduam, invenire potuiſſet.

Illud profecto confitendum eſt; in philoſophiâ tractandâ Newtonum inter & Leibnitium plurimum intereſſe. Prior ille eo uſque progeditur, quo phænomenorum & experimentorum evidentia eum ducit; & ubi illa deficit, pedem ſiſtit: poſterior hypotheſibus ſuis ſcatet totus; eaſque proponit, non experimentis examinandas, ſed clauſis oculis credendas. Ille, inopiâ experimentorum, quæ cauſam Gravitatis certo indicare poſſint, utrum mechanica fuerit necne, non affirmat: hic, ſi mechanica non ſit, perpetuum eſſe miraculum pronuntiat. Ille, atque id quoque non definiens ſed quærens, Creatoris potentiæ tribuit, quòd minimæ quæque materiæ partes ſint duræ: hic illam materiæ duritiem conſpirantibus quibuſdam motibus imputat; &, ſi cauſa ejus alia ponatur quàm mechanica, pro perpetuo eam miraculo deridendam propinat. Ille motum in homine animalem, non audet affirmare, merè eſſe mechanicum: hic purè mechanicum eſſe audacter aſſerit; cum ex hypotheſi ejus de harmoniâ præſtabilitâ, numquam anima vel mens hominis ſic agat in corpus, ut motus hujus vel impediat, vel adjuvet. Ille Deum aſſerit *(Deum in quo vivimus, & movemur, & ſumus)* eſſe omnipræſentem; non tamen

ſamen ut mundi animam: hic, non mundi quidem animam eſſe, ſed INTELLIGENTIAM SUPRAMUNDANAM: ex quo illud conſequi videatur, non poſſe Deum intra mundi limites quicquam efficere, niſi per miraculum prorſus incredibile. Ille philoſophis præcipit, ut à phænomenis & experimentis ad eorum cauſas progrediantur; atque inde ad cauſarum iſtarum cauſas; & ſic deinceps, donec ad primam cauſam perveniatur. Hic omnes cauſæ primæ actiones pro miraculis haberi; omneſque leges per Dei voluntatem naturæ impreſſas pro perpetuis miraculis occultiſque qualitatibus cenſeri; & idcirco ex philoſophiâ exulare jubet. Siccine verò agitur? An perpetuæ & univerſales naturæ leges, ſi ex potentiâ Dei, cauſæve adhuc nobis incognitæ actione deriventur, pro miraculis & qualitatibus occultis, hoc eſt ex ejus ſententiâ, pro monſtris & abſurditatibus, ſunt exſibilandæ? Omnia porro pro Dei exiſtentiâ de naturæ phænomenis ſumpta argumenta, idcircóne ſunt explodenda, quia novis quis ea nominibus & ignominioſis infamet? An, ut ſuperſtitioſa & abſurda, rejicietur Philoſophia Experimentalis, quia neque ultra experimenta definire quicquam vult; neque adhuc per experimenta probare poteſt, naturæ omnia phænomena per cauſas merè mechanicas poſſe ſolvi? Res profecto digna eſt, quæ & maturè & ſeriò conſideretur.

COM-

COMMERCIUM EPISTOLICUM D. JOHANNIS COLLINS, ET ALIORUM, DE ANALYSI PROMOTA: JUSSU SOCIETATIS REGIÆ LONDINENSIS, ANNO 1712, IN LUCEM PRIMUM EDITUM.

AD

LECTOREM.

QUAM ob cauſam editæ ſint hæ epiſtolæ chartulæque collectaneæ, apparebit ex literis D. Leibnitii & D. Keillii in fine ſubjunctis. Offenſionem attulerant D. Leibnitio nonnulla, quæ ſcripto prodidit D. Keillius in Actis Londinenſibus anno 1708, *injuriam D. Newtono oblatam propulſans. Datis igitur ad Societatis Regalis ſecretarium literis, de calumniâ queſtus D. Leibnitius, remedium à Societate petiit; idque eos æquum credidit judicaturos, ut D. Keillius culpam ſuam publicè fateretur. D. Keillio ea eſt pars viſa potior, ut ad illa, quæ queſtus erat D. Leibnitius, literis ſcriptis reſponderet: quibus in literis, quæ antea ediderat, & expoſuit pleniùs, & vindicavit. D. Leibnitius, nequaquam his ſatis ſibi factum arbitratus, literas alteras ad Societatem dedit; in quibus adhuc de D. Keillio queſtus,* novum eum hominem appellat, parumque peritum rerum anteactorum cognitorem; nec mandatum ab eo, cujus intereſſet, habentem; Societatiſque æquitati committit, annon coercendæ ſint vanæ & injuſtæ vociferationes.

Verſabatur in Angliâ D. Leibnitius ineunte anno 1673, *iterumque menſe* Octobri 1676; *& interjecto illo temporis intervallo in Galliâ egit. Quo omni temporis ſpatio, mutuis acceptis datiſque literis, commercium habuit cum D. Oldenburgo; &, Oldenburgi operâ, tandem cum D. Collinio itidem, & nonnunquam etiam cum D. Newtono. Quid autem ille ex Anglis tandem, vel tum cùm Londini eſſet, vel ex literis iſtis mutuò datis, edidicerit, in eo forè vertitur hæc omnis quæſtio. D. Oldenburgus & Collinius jam diù obierunt. D. Newtonus autem tum Cantabrigiæ egit; parumque amplius novit, quàm quod ex literis ipſius à D. Walliſio deinceps editis apparet. D. Newtonus neque à D. Keillii partibus teſtis eſſe poteſt, nec D. Leibnitius ipſe à ſuis: alius autem in vivis teſtis eſt nullus. Societas itaque regalis, à D. Leibnitio bis adverſus Keillium appel-*

lata, ſelectorum ex Societate arbitrorum conſeſſum conſtituit; qui literas, literamque tranſcriptarum libellos, aliaſque chartulas à D. *Oldenburgo penes Societatem relictas, & ſiquid inter* D. *Collinii ſchedas repertum huc faceret, perſcrutarentur, ſententiamque ſuam ad Societatem referrent: juſſitque tandem ut ſententia illa, à ſelectorum arbitrorum conſeſſu relata, unà cum ipſis literarum, aliarumque chartularum excerptis, emitteretur.*

De hâc methodo, ex methodis Serierum et Fluentium compoſitâ, ſcripſit infra Newtonus. N° VI, VII, XXII, XLVII, LIII, LVI.

Cùm D. *Newtonus Analyſin iſtam ſcripto traderet, quæ ſub initium horum collectaneorum impreſſa eſt, habuit jam tum* * *methodum generalem æquationes finitas in infinitas reſolvendi, & æquationes tum finitas tum infinitas applicandi ad problemata ſolvenda, ope proportionum augmentorum momentaneorum quantitatum naſcentium, & augeſcentium. Augmenta hæc appellat* D. *Newtonus particulas, & momenta; D. Leibnitius autem infiniteſimales, indiviſibiles, & differentias. Quantitates augeſcentes appellat* D. *Newtonus Fluentes; D. Leibnitius autem Summas. Et velocitates augmenti appellat* D. *Newtonus Fluxiones; iſtaſque fluxiones exponit per quantitatum fluentium momenta.*

Quæ pars hujus methodi in eo ſita eſt, ut æquationes finitæ in infinitas reſolvantur, eam cum D. *Leibnitio, rogatu ſuo, communicavit* D. *Newtonus, literis ad illum datis* Junii 13, & Octobris 24, 1676. *Reliquam hujus methodi partem, poſtquam eouſque explicuerat, ut eam ſatis* § *obviam factam exiſtimaret; ne ſibi deinceps ſubriperetur, priuſquam eam exponere viſum foret, literis occultis ita celavit, quo modo aliàs Galilæus atque Hugenius fecerant. Hujus poſterioris partis inventionem ſibi vendicat* D. *Leibnitius: D. Keillius autem eam* D. *Newtono adſerit; Keillioque ſuffragatur ſententia ſelectorum è Societate arbitrorum conſeſſûs. Alios tamen exteros, qui methodum iſtam à* D. *Leibnitio acceperint, aut aliter obtinuerint, nihil quidquam in his collectaneis eſt, quod ullo pacto afficiat. Illi, quid inter* D. *Leibnitium & D. Oldenburgum commercii eſſet, ignorabant. Illis, quòd methodum, quam utilem eſſe compererant, in rem ſuam adhibuerint, atque excoluerint; id verò laudi eſt dandum.*

§ Vid. N° LIII.

Subjunctæ ſunt epiſtolis annotationes quædam; quò lectores, quibus minus eſt otii, & epiſtolas inter ſe faciliùs conferre, & ſemel perlectas intelligere queant.

COM-

COMMERCIUM EPISTOLICUM

D. JOHANNIS COLLINS,

ET ALIORUM,

DE ANALYSI PROMOTA:

JUSSU SOCIETATIS REGIÆ

IN LUCEM EDITUM.

Excerpta ex epiſtolâ reverendi viri D. Iſaaci Barrow *ad D.* J. Collins, Cantabrigiæ, 20 Junii 1669 *datâ; cujus habetur Autographon.* N°I.

AMICUS quidam apud nos commorans, qui eximio in his rebus pollet ingenio, nudiuſtertius chartas quaſdam mihi tradidit, in quibus magnitudinum dimenſiones ſupputandi methodos, *Mercatoris* methodo pro hyperbolâ ſimiles, maximè verò generales, deſcripſit; ſimulque æquationes reſolvendi: quæ, ut opinor, tibi placebunt; quas unà cum proximis literis ad te mittam.

Ex epiſtolâ ejuſdem ad eundem, 31 Julii 1669 *datâ, pariterque ipſius* Barrovii *manu ſcriptâ.*

Mitto, quas pollicitus eram, amici chartas; quæ, uti ſpero, haud parum te oblectabunt. Remittas, quæſo, quum eas quantum tibi viſum fuerit perlegeris: id enim poſtulavit amicus meus, cùm primùm eum rogavi, ut eas tecum communicare mihi liceret. Quantocyus igitur, obſecro, te eas recepiſſe fac me certiorem; quòd illis metuo; quippe qui eas per veredarium publicum ad te mittere non dubitaverim, quo tibi morem gererem quàm citiſſimè.

Ex

Ex epiſtolâ ejuſdem ad eundem, 20 Aug. 1669 datâ; cujus etiam comparet autographon.

Amici chartas tibi placuiſſe gaudeo; eſt illi nomen Newtonus, collegii noſtri ſocius, & juvenis; fecundus enim, ex quo artium magiſtri gradum cepit, jam agitur annus; & qui, eximio quo eſt acumine, permagnos in hâc re progreſſus fecit. Illas, ſi vis, cum nobili domino vicecomite Brounkero communica.

N° II. *Exemplar dictarum chartarum, manu D.* Collins *exaratum, & in ſcriniis ejus repertum, quod cum ipſius D.* Newtoni *autographo collatum ad verbum conſentire invenimus. Hujus autem titulus eſt*

DE ANALYSI PER ÆQUATIONES NUMERO TERMINORUM INFINITAS.

METHODUM generalem, quam de Curvarum quantitate——ex utrâque parte lineæ BD. [Vide Tom. I. p. 257.]

Compoſitarum Curvarum Quadratura ex Simplicibus.

N° III. REG. II. *Si valor ipſius y, &c.*——*dabitur tota* βBDδ. [Vide Tom. I. p. 258—263.]

Aliarum Omnium Quadratura.

N° IV. REG. III. *Sit valor ipſius y, &c.*——$\frac{13}{3}x^3$. [Vide Tom. I. p. 263, 264.]

Exempla Radicem extrahendo.

N° V. Si Sit $\sqrt{aa+xx}=y$, *&c.*——ſuperficies cognoſcetur. [Vide Tom. I. p. 264—268.]

Exempla per Reſolutionem Æquationum.

Numeralis Æquationum affectarum Reſolutio.

N° VI. Quia tota difficultas——quæ quærebatur. [Vide Tom. I. p. 268—270.]

Literalis Æquationum affectarum Reſolutio.

N° VII. His in numeris ſic——x ſit minor. [Vide Tom. I. p. 270—272.]

Alius modus eaſdem Reſolvendi.

N° VIII. Sin valor areæ tanto——magis compoſita. [Vide Tom. I. p. 272.]

N° IX. Sed hunc modum——$y=x^{\frac{2}{3}}+2x^{\frac{4}{3}}$, &c. [Vide Tom. I. p. 273, 274.]

N° X. Et hæc de areis——in reliquis. [Vide Tom. I. p. 274—278.]

Applicatio

* N. B. Quadratura Curvarum per Æquationes infinitas, quæ nonnunquam terminantur & finitæ evadunt. Eadem explicatur in Prop. v. Libri de Quadraturis. Et propoſitio illa pendet à quatuor prioribus. Ideoque methodus fluxionum & momentorum, quatenus habetur in propoſitionibus

Applicatio prædictorum ad Curvas Mechanicas.

Et hæc de Curvis geometricis——fiat) * exactè & Geometricè determi- N° XI.
nentur. Sed iſta narrandi non eſt locus. Reſpicienti duo præ reliquis demonſtranda occurrunt. [Vide Tom. I. p. 278—280.]

1. *Demonſtratio Quadraturæ Curvarum Simplicium in Regulâ Primâ.*

Præparatio pro Regulâ Primâ demonſtrandâ.

† Sit itaque Curvæ——‡ Tum $x + o$ [Tom. I. p. 280]——§ poſſunt N° XII.
inveniri [Tom. I. p. 281]——minimarum dimenſionum. [Tom. I. p. 282.]

Excerpta ex epiſtolâ D. Oldenburgh *ad D.* Renatum Franciſcum Sluſium N° XIII.
Canonicum Leodienſem, *anno* 1669, 14 Septembris St. vet. *datâ: cujus apographum conſpicitur in libro Societatis Regiæ, quo conſervantur epiſtolæ, No. III. p.* 174.

Inſuper communicavit ille [*Barrovius*] univerſalem methodum analyticam, ipſi tranſmiſſam à D. Iſaaco Newtono, inſervientem menſurandis areis omnium ejuſmodi Curvarum, & earundem perimetrorum, in quibus ordinatæ eandem habet communem habitudinem ad baſin: hæcque methodus alia non eſt ab illâ, quam particulariter applicuit D. *Mercator* ad inveniendas areas hyperbolæ, univerſalis reddita. Auctor ſic incipit.

" *De Analyſi per Æquationes numero terminorum infinitas.*

" Methodum generalem, quam de Curvarum quantitate, per infinitam " terminorum ſeriem menſurandâ, olim excogitaveram, &c."

Et poſtquam ejus beneficio oſtendit complurium Curvarum quadraturam, accedit ad circulum; & convertendo $\sqrt{aa + bb}$, vel $\sqrt{aa - bb}$ in ſeriem infinitam, oſtendit complures ejuſmodi ſeries applicari poſſe ad circulum: adeo ut datis horum quibuſlibet duobus; radio nempe, ſinu, arcu, & areâ ſegmenti, reliquorum quodvis inveniri poſſit infinitè verum: res ni fallor ab omnibus auctoribus prægreſſis valdè expetita. Ejuſdem etiam adminiculo eximiè facilitavit inventionem radicis æquationis cujuſlibet, & mediarum proportionalium; & ſeriem largitur ad inveniendam lineæ Ellipticæ longitudinem. Similiter, ut oſtenderet methodum ſuam ad Curvas Mechanicas, earumque tangentes, ſe porrigere, quadrat Cycloidem ejuſque portiones; areamque curvæ Quadratricis, ejuſque perimetrum invenit: atque ad calcem ſic ait.

tionibus quinque primis Libri de Quadraturis, Newtono innotuit anno 1669.
† Exemplum luculentum calculi per momenta fluentium.
‡ Leibnitius ſcribit *dx* pro *o* vel $o \times 1$; *dz* pro *ov*, vel *oy*.
§ Hâc propoſitione ex æquatione fluentes involvente inveniuntur fluxiones.

" Nec quicquam hujuſmodi ſcio, ad quod hæc methodus, idque variis
" modis, ſeſe non extendat. Imo tangentes ad Curvas Mechanicas, ſi
" quando id non aliàs fiat, hujus ope ducuntur. Et quicquid vulgaris
" Analyſis per æquationes ex finito terminorum numero conſtantes, quando
" id ſit poſſibile, perficit; hæc per æquationes infinitas ſemper perficiat.
" Et hæc de areis Curvarum inveſtigandis dicta ſufficiant. Imo cùm pro-
" blemata de Curvarum longitudine, de quantitate & ſuperficie Solidorum,
" deque centro Gravitatis, poſſunt eo tandem reduci, ut quæratur quantitas
" ſuperficiei planæ, lineâ curvâ terminatæ; non opus eſt quicquam de iis
" adjungere."

N° XIV. *Ex epiſtolâ D.* Collins *ad D.* Jacobum Gregorium, *anno* 1669, 25 Nov. *datâ. Quæ quidem epiſtola, manu dicti D.* Collins *deſcripta, conſervata eſt.*

Barrovius provinciam ſuam publicè prælegendi remiſit cuidam, nomine Newtono, Cantabrigienſi; cujus, tanquam viri acutiſſimo ingenio præditi, in præfatione Prælectionum Opticarum, meminit: quique antequàm ederetur Mercatoris Logarithmotechnia, eandem methodum adinvenerat; eamque ad omnnes Curvas generaliter, & ad circulum diverſimodè, applicârat.

N° XV. *Ex epiſtola D.* Jacobi Gregorii *ad D.* J. Collins, *ad Fanum Sti.* Andreæ *apud* Scotos *anno* 1670, 20 Aprilis *datâ, prout in autographo ipſius* Gregorii *legitur.*

Seriem à te miſſam de cirogli zonâ intelligere nequeo, nempe $2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5B^9}{576R^7}$ — &c. Si hæc rectè deſcripta ſit, ſeriem legitimam non eſſe ſuſpicor.

N° XVI. *Ex epiſtolâ ejuſdem* Gregorii *ad eundem, anno* 1670, 5 Septemb. *datâ.*

Barrovii [geometricas] lectiones ſummâ cum voluptate & attentione perlegi; atque omnes, qui unquam hiſce de rebus ſcripſerunt, infinito intervallo ſuperâſſe comperio. Ex ejuſdem [Barrovii] methodis tangentes ducendi, cum quibuſdam è propriis collatis, inveni methodum generalem & geometricam * ducendi tangentes ad omnes Curvas ſine calculo; & quæ complectitur non tantùm Barrovii methodos particulares, ſed & ipſius generalem methodum analyticam, quam habes ſub finem lectionis decimæ. Methodus mea haud pluribus quàm duodecim continetur propoſitionibus.

N° XVII. *Ex epiſtolâ ejuſdem ad eundem anno* 1670, 23 Novemb. *datâ; cujus etiam conſervatur autographon.*

Plurimæ approximationes pro circuli ſegmentis ex his facilè elici poſſunt; at vix operæ pretium erit, cùm poteſtates alternas tollere nequeo,

* Hinc innotuit methodum tangentium *Gregorii* & *Sluſii* ex methodo *Barrovii* conſequi.

quod

quod factum est à D. *Newtono* in suâ serie, modò series sit: nam, ut dicam quod sentio, ad nullam mearum reducere possum. Autumo tamen meam pari facilitate & brevitate rem confecturam.

Ex autographo D. Jacobi Gregorii *ad eundem D.* Collins, *de Fano-Stirenæ, 19 Decembris ejusdem anni, missa.* N° XVIII.

Quum postremas ad te dedi literas, nondum animadvertissem D. Newtoni seriem de circuli zonis, quam jam dudum ad me misisti, unà cum infinito istiusmodi serierum numero, consectarium illius esse posse, quam mihi de logarithmis: nempe, dato logarithmo invenire ejus numerum; vel radicem potestatis cujuscunque puræ in infinitam seriem permutare. Me sine tam tardi fuisse ingenii miror; qui tanto temporis spatio hoc non animadverteram, quum tamen multum olei & operæ in istâ serie expiscandâ impenderam. At ut ingenuè fatear, semper in animum induxeram, si modò series esset, me in eam incidere posse, ope aliquarum è seriebus meis pro circulo inter se combinatis, quarum quidem plurimas ad manus habeo; neque ullam aliam desiderâram methodum. Series tua, paululum producta, fit $2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5R^9}{576R^7} - \frac{7B^{11}}{1408R^9} - \frac{21B^{13}}{6656R^{11}} - \frac{11B^{15}}{5126R^{13}}$ (a) &c. Eisdem etiam positis, erit arcus, cujus sinus B, $= B + \frac{B^3}{6R^2} + \frac{3B^5}{40R^4} + \frac{5L^7}{112R^6} + \frac{35B^9}{1152R^8} +$ (b) &c. Plures hujuscemodi series proferre possem; sed tu fortasse plus meipso de his rebus nôsti.

Ex epistolâ D. Collins *ad dictum D.* Gregorium, 24 Decembris, *anno* 1670 *datâ: cujus habetur exemplar manu ipsius D.* Collins *descriptum.* N° XIX.

Quum D. Dary * Miscellanea sua in lucem edidit, exemplar libelli misit ad D. Newtonum; qui dictum D. Dary serie, pro areâ zonæ circuli, quam ad te misi, remuneravit: quæ sine omni dubio series est legitima & eximia. Ope D. Barrovii nonnullas alias series, è methodo Newtoni generali derivatas, obtinui; easque, conferto colloquio, deprehendi analyticè deduci posse è datis cujusvis figuræ proprietatibus; & multas series ad singulas figuras applicari posse. Universalem quoque esse, cujusque ope omnes Quadraturas perficere possis; tam Curvarum quas Cartesius Geometricas esse admittit, quàm earum quas censet Mechanicas.

Hâc itaque methodo Curvæ omnium figurarum, communi proprietate definitarum, rectificantur; earum tangentes & centra gravitatis inveniuntur; item rotunda earum Solida, & segmenta secunda cubantur; & in universis Curvis, longitudine curvilineâ data, ordinatim applicatæ inveniuntur, & vice versâ.

Exempla quædam.

Arcu z dato, invenire sinum x, vel co-sinum y; positâ unitate pro radio.

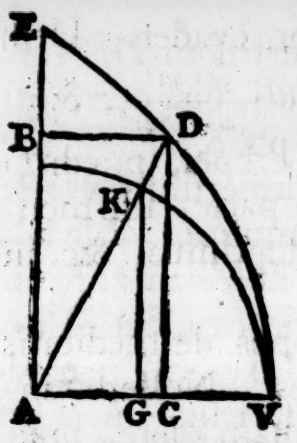
(a) Vide Analysin per Æq. Num. Term. inf. Cap. III. § 6.
(b) Vide Analysin per Æq. Num. Term. inf. Cap. VI. § 3.
* N. B. Miscellanea edidit D. *Michael Dary*, anno 1669.

$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362880}z^9$ (a) – &c.

$y = 1 - \frac{1}{2}z^2 + \frac{1}{24}z^4 - \frac{1}{720}z^6 + \frac{1}{40320}z^8 - \frac{1}{3628800}z^{10}$ (a) + &c.

Et dato ſinu x, invenire z: $z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7$ (b) + &c.

Quadratricem veterum quod attinet, nulla methodus, nullus geometra ejus aream exhibere valuit. Sit igitur AV, radius circuli inſcripti, unitas; & VK, arcus, x; erit area BDVA $= x - \frac{1}{9}x^3 - \frac{1}{225}x^5 - \frac{4}{13230}x^7$ – &c. (c)



Tractatum hâc de re ſcripſit; in quo inventio longitudinis totius, vel datæ partis, curvæ Ellipticæ, & Quadratricis DV, nec non areæ ſupradictæ, eſt inter exempla.

N°XX. *Ex epiſtolâ D.* Jacobi Gregorii *ad D.* Collins, 15 *Feb. anno* 167$\frac{0}{1}$ *datâ, cujus habetur autographon.*

Ex quo epiſtolam ad te dedi, tres à te accepi, unam *Decemb.* 15, alteram *Dec.* 24, tertiam 21 *Januarii* nuper elapſi datam.

Quod attinet Newtoni methodum univerſalem, aliquâ ex parte, ut opinor, mihi innoteſcit; tam quoad Geometricas, quàm Mechanicas curvas. Nihilo tamen minus, ob ſeries ad me miſſas, gratias habeo; quas ut remunerem, mitto quæ ſequuntur.

Sit radius $= r$; arcus $= a$; tangens $= t$; ſecans $= s$.

Et erit $a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8}$, &c. (d)

Eritque $t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8}$, &c. (d)

Et $s = r + \frac{a^2}{2r} + \frac{5a^4}{24r^3} + \frac{61a^6}{720r^5} + \frac{277a^8}{8064r^7}$, &c. (e)

Sit nunc tangens artificialis $= t$; & ſecans artificialis $= s$; & integer quadrans $= q$.

Erit $s = \frac{a^2}{2r} + \frac{a^4}{12r^3} + \frac{a^6}{45r^5} + \frac{17a^8}{2520r^7} + \frac{62a^{10}}{28350r^9}$, (f) &c.

Sit $2a - q = e$, & erit $t = e + \frac{e^3}{6r^2} + \frac{e^5}{24r^4} + \frac{61e^7}{5040r^6} + \frac{277e^9}{72576r^8}$, (g) &c.

Sit nunc ſecans artificialis 45 *gr.* $= s$; ſitque $s + l$ ſecans artificialis ad libitum. Erit ejus arcus $= \frac{1}{2}q + l - \frac{l^2}{r} + \frac{4l^3}{3r^2} - \frac{7l^4}{3r^3} + \frac{14l^5}{3r^4} - \frac{452l^6}{45r^5}$, (h) &c. eritque $2a - q = t - \frac{t^3}{6r^2} + \frac{t^5}{24r^4} - \frac{61t^7}{5040r^6} + \frac{277t^9}{27576r^8}$, (i) &c.

Hîc animadvertendum eſt, radium artificialem eſſe 0; & ubi inveneris q majorem quàm $2a$, ſive artificialem ſecantem 45 *gr.* majorem eſſe datâ ſecante; mutanda eſſe ſigna, & pergendum ſecundum vulgaris Algebræ præcepta.

(a) Vide Analyſin per Æq. Num. Term. inf. Cap. VII. Sect. 2.
(b) Vide Analyſin per Æq. Num. Term. inf. Cap. VI. § 2.
(c) Vide Analyſin per Æq. Num. Term. inf. Cap. IX. § 4.

Sit

Sit ellipſis, cujus alter ſemiaxium $= r$; alter $= c$. Ex quolibet curvæ ellipticæ puncto demittatur in ſemiaxem, r, recta perpendicularis $= a$. Erit curva elliptica perpendiculari a adjacens $= a + \frac{r^2a^3}{6c^4} + \frac{4r^2c^2a^5 - r^4a^5}{40c^8} + \frac{8c^4r^2a^7 + r^6a^7 - 4c^2r^4a^7}{112c^{12}} + \frac{64c^6r^2a^9 - 48c^4r^4a^9 + 24c^2r^6a^9 - 5r^8a^9}{1152c^{16}}$, &c.

Si determinetur ellipſeos ſpecies, ſeries hæc ſimplicior evadet. Ut ſi $c = 2r$, foret Curva prædicta $= a + \frac{a^3}{96r^2} + \frac{3a^5}{2048r^4} + \frac{113a^7}{458752r^6} + \frac{3419a^9}{75497472r^8}$, &c.

Reliquis verò manentibus, ſi Curva prædicta eſſet hyperbola, prædicta quoque ſeries ei inſerviret; ſi modò omnium terminorum partes affirmentur, & negentur totus terminus tertius, totus quintus, ſeptimus, &c. in locis imparibus.

Gratias ago maximas, tam ob benevolentiam, quâ mones de meditatis meis publicandis, quàm ob perhumanas tuas pollicitationes. Nollem tantam moleſtiam tibi creare; neque mihi in animo eſt quicquam edere, præterquam quadraturam meam Circuli recuſam, additis quibuſdam nugamentis. Quod attinet methodum meam inveniendi radices omnium æquationum; una ſeries unam tantùm prodit radicem, at pro quâlibet radice infinitæ ſunt ſeries. Induſtriâ autem aliquâ opus eſt, ad ſeriem ritè incipiendam, & ad quam pertineat radicem dignoſcendam. Verùm hâc de re fuſiùsforſan aliquando ad te ſcribam. Non eſt quod metuas cuiquam, quicquid miſerim, communicare; parum enim ſolicitus ſum, utrumne meo an alieno nomine in publicum prodeat.

Ex epiſtolâ D. Collins *ad D.* Bertet Pariſiis *tum agentem. Data autem eſt* 21 Februarii, *anno* $170\frac{0}{1}$; *ejuſque exemplar, manu ipſius D.* Collins *exaratum, conſervatur.* N° XXI.

Syſtema Algebræ integrum componere, opus eſt eximium, & dignum cui ab omnibus faveatur; præcipuè verò quia quatuor circiter abhinc annis inventa fuit à D. Iſaaco Newtono methodus analytica generalis, pro quadraturâ omnium ſpatiorum curvilineorum, tam in Curvis Geometricis quàm Mechanicis, communi aliquâ proprietate gaudentibus. Hujus ope quicquid à quadraturis pendet peragitur; ut rectificatio Curvarum; inventio tangentium & centrorum gravitatis; rotundorumque Solidorum, & eorundem ſegmentorum ſecundorum, & curvarum ſuperficierum dimenſuratio: non autem ſuperficierum Solidorum quorum axes inclinantur; uti parabolicorum conoidum, &c. hæc manet difficultas poſteris ſuperanda. Hæc omnia peraguntur approximando verum in infinitum, abſque radicum extractione, ope infinitæ ſeriei rationalium, cujuſmodi multæ ad unam eandemque figuram diverſimodè applicari poſſunt: *v. g.* ad Circulum una, ad inveniendam aream totius, vel partis cujuſvis; alia, ad inſcriptas; alia, ad adſcriptas; &c. Ita ut dato ſinu, tangente vel ſecante, inveniri poteſt longitudo arcûs, & vice verſâ, ope diverſarum ſerierum ad eam rem appropriatarum.

(d) Vide Excerp. ex Epiſt. Not. k, § 7.
(e) Vide Excerp. ex Epiſt. Not. k, § 8.
(f) Vide Excerp. ex Epiſt. Not. k, § 14.
(g) Vide Excerp. ex Epiſt. Not. k, § 22.
(h) Vide Excerp. ex Epiſt. Not. k, § 23.
(i) Vide Excerp. ex Epiſt. Not. k, § 26.

Unde fit, ut jam calculo facilior inventu ſit arcus è ſinu dato, & vice verſâ, quàm è ſinu dato ſinus dupli arcûs. Univerſim autem hoc nihil aliud eſt, quàm methodus à Mercatore uſurpata, in ejus Logarithmotechniâ, ad hyperbolam quadrandam, generalis reddita. D. Jacobus Gregorius apud Scotos nuperrimè incidit in eandem methodum.

N° XXII. *Ex epiſtolâ D.* J. Collins *in Italiam ad D.* Alphonſum Borellum *miſſâ; & menſe* Decembri *anno* 1671 *datâ: cujus habetur exemplar, manu ipſius D.* Collins *deſcriptum.*

Kinckhuyſenii introductio ad Analyſim Speciosam, quam *Stel-konſt* vocat, à D. Iſaaco Newtono prælo parata eſt; qui jam Mathematicês Profeſſor apud Cantabrigienſes factus eſt. Huic adjunget ipſius methodum generalem Quadraturarum analyticam; cujus ope calculo eruit omnium curvilinearum figurarum regularium, communi aliquâ proprietate gaudentium, aream; earundem Curvarum rectificationem; inventionem centrorum gravitatis earum; itemque rotunda Solida, & ſuperficies eorum rotatione genitas; & ſecunda iſtorum Solidorum ſegmenta: imo dato quovis logarithmico ſinu, tangente vel ſecante in canone, invenire licet arcum ei competentem, abſque naturali ſinu, tangente vel ſecante prius invento, & vice verſâ; idque generaliter, ſine ullâ radicum extractione.

Hujus ſpecimen pro circulo appoſui.

N. B. In hujus epiſtolæ exemplari, locus vacuus ſeriei interſerendæ hîc relictus fuit.

N° XXIII. *Ex epiſtolâ ejuſdem D.* Collins *ad D.* Franciſcum Vernon *Anglum Pariſiis agentem,* Londini, 26 Decembris, *anno* 1671 *datâ: cujus habetur exemplar manu ipſius D.* Collins *deſcriptum.*

D. Barrovius certiorem me facit, D. Newtonum pene adornâſſe Kinckhuyſenii ad Algebram introductionem; cujus hîc brevi edendæ negotium mihi curæ erit: eamque de propriâ ipſius penu auctiorem reddidiſſe. Huic ſubjiciet generalem * ſuam infinitarum ſerierum methodum analyticam; cujus ope computantur omnium ſpatiorum curvilineorum areæ, tum Geometricorum, tum eorum quæ, ex mente Carteſii, Mechanica ſunt, modo figuræ una aliqua aut pluribus communibus proprietatibus definitæ ſint; ipſarumque Curvarum longitudines; centra gravitatis; rotunda Solida, & ſuperficies eorum rotatione genitæ. Hinc etiam eruuntur multæ pro circulo ſeries; necnon quovis numero dato, tanquam logarithmico ſinu, tangente vel ſecante, calculo perfacili, ſine ullâ radicum extractione, ſine ullis tabulis, inveniri poteſt arcus ei reſpondens, & vice verſâ; idque vero quantum velis proximè, abſque naturali ſinu, tangente, aut ſecante prius invento: tot tantiſque commodis fœta eſt hæc doctrina, de quâ non niſi comperta loquor! Unà cum his mittet viginti lectiones ejus Opticas, quas D. Barrovius opus cenſet, quo majus præſens ætas vix protulit. Admonui maturandam

* Hic tractatus unus idemque eſt ac ille, cujus mentionem fecerat D. Newtonus in epiſtolâ *Oct.* 24, 1676, datâ, per D. Oldenburgum D. Leibnitio communicatâ; & in quo methodi ſe-

rierum

randam ideo esse impressionem, quoniam D. Hugenius tractatum de Dioptricâ, & de Curvarum Evolutione jam molitur. Ille autem contrà, se magis cupere, ut accepto harum rerum nuncio, Hugenius potius excitaretur quàm tardaretur; ratus minimè verisimile utriusque hypotheses vel deductiones easdem esse posse.

Ex epistolâ D. J. Collins *ad D.* Thomam Strode, 26 Julii *anno* 1672 N° XXIV.
datâ: cujus habetur exemplar manu ipsius D. Collins *descriptum.*

Quod geometriam curvarum figurarum spectat; hanc tandem generaliter ad calculum analyticum reduci posse, omnino orbi literato novum atque inauditum est. Hujus æquationes sunt series, terminis numero infinitis conflatæ, quorum tamen pauci sufficiunt communiter, ex notis curvarum proprietatibus erutæ. Auctorem quod attinet, hujusque methodi præstantiam, hæc accipe.

Mense *Septembri* 1668, Mercator Logarithmotechniam edidit suam, quæ specimen hujus methodi, i e. serierum infinitarum, in unicâ tantùm figurâ, nempe quadraturam hyperbolæ continet. Haud multo post quàm in publicum prodierat liber, exemplar ejus Cl. Wallisio Oxoniam misi; qui suum de eo judicium in Actis Philosophicis statim fecit: aliumque Barrovio Cantabrigiam; qui quasdam Newtoni chartas, qui jam Barrovium in mathematicis prælectionibus publicis excipit, extemplo remisit: è quibus & ex aliis, quæ olim ab auctore cum Barrovio communicata fuerant, patet illam methodum à dicto Newtono aliquot annis antea excogitatam, & modo universali applicatam fuisse: ita ut ejus ope, in quâvis figurâ curvilineâ propositâ, quæ unâ vel pluribus proprietatibus definitur, quadratura vel area dictæ figuræ, † accurata, si possibile sit, sin minus infinitè vero propinqua; evolutio, vel longitudo lineæ curvæ; centrum gravitatis figuræ; Solida, ejus rotatione genita, & eorum superficies; sine ullâ radicum extractione obtineri queant.

Postquam intellexerat D. Gregorius hanc methodum, à D. Mercatore in Logarithmotechniâ usurpatam, & Hyperbolæ quadrandæ adhibitam, quamque adauxerat ipse Gregorius, jam universalem redditam esse, omnibusque figuris applicatam; acri studio eandem acquisivit, multumque in eî enodandâ desudavit.

Uterque, D. Newtonus & Gregorius, in animo habet hanc methodum exornare. D. Gregorius autem, D. Newtonum primum ejus inventorem anticipare, haud integrum ducit.

Ex epistolâ ad D. Collins *ad D.* Newtonum 30 Julii *anno* 1672 *datâ, cujus* N° XXV.
habetur exemplar manu ipsius D. Collins *descriptum.*

Parandis seriebus pro extrahendis radicibus in speciebus [Algebraicis], ad modum Vietæ [in numericis], credo D. Gregorium haud modicam impen-

rierum infinitarum & fluxionum simul explicabantur, ut ibi loci memorat.

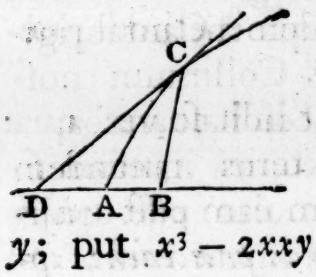
† Testibus igitur Barrovio & Collinio, methodus prædicta quadrandi figuras, accuratè si fieri potest, Newtono innotuit anno 1666 aut antea.

disse

diſſe operam: nihil autem de eâ re ſcribere ſuſcipiet, antequam tu, methodi hujus repertor, proprias de eâ lucubrationes in lucem emiſeris; ſed aliis rebus per interim intentus eſt.

N° XXVI. *Ex epiſtolâ D.* Newtoni *ad D.* Collins, *anno* 1672, 10 Decembris *datâ. Repertum autem eſt ipſius* Newtoni *autographum in ſcriniis D.* Collins, *unà cum ejuſdem exemplari manu D.* Collins *deſcripto.*

Ex animo gaudeo D. Barrovii, amici noſtri reverendi, lectiones Mathematicis exteris adeo placuiſſe; neque parum me juvat intelligere eos [Sluſium & Gregorium] in eandem mecum incidiſſe ducendi tangentes methodum. Qualem eam eſſe conjiciam, ex hoc exemplo percipies. Pone CB applicatam ad AB, in quovis angulo dato, terminari ad quamvis Curvam, AC; & dicatur AB, x; & BC, y; habitudoque inter x & y exprimatur quâlibet æquatione; puta $x^3 - 2xxy + bxx - bbx + byy - y^3 = 0$, quâ ipſa determinatur Curva. Regula ducendi tangentem hæc eſt: Multiplica æquationis terminos per quamlibet progreſſionem arithmeticam juxta dimenſiones y; put $x^3 - 2xxy + bxx - bbx + byy - y^3$: ut & juxta dimenſiones x; puta



$x^{3}_{0} - 2xxy^{1} + bxx^{0} - bbx^{0} + byy^{2} - y^{3}$. Prius productum erit numerator, & poſ-
$3 \quad 2 \quad 2 \quad 1 \quad 0 \quad 0$

terius, diviſum per x, denominator fractionis, quæ exprimet longitudinem BD, ad cujus extremitatem, D, ducenda eſt tangens CD. Eſt ergo longitudo $BD = \frac{-2xxy + 2byy - 3y^3}{3xx - 4xy + 2bx - bb}$.

Hoc eſt unum particulare, vel corollarium potius, methodi generalis; quæ extendit ſe, citra moleſtum ullum calculum, non modo ad ducendum tangentes ad quaſvis Curvas, ſive Geometricas, ſive Mechanicas, vel quomodocunque rectas lineas, aliaſve Curvas, reſpicientes; verùm etiam ad reſolvendum alia abſtruſiora problematum genera de curvitatibus, areis, longitudinibus, centris gravitatis Curvarum, &c. Neque, quemadmodum Huddenii methodus de Maximis & Minimis, ad ſolas reſtringitur æquationes illas, quæ quantitatibus ſurdis ſunt immunes.

Hanc methodum intertexui * alteri iſti, quâ æquationum exegeſin inſtituo, reducendo eas ad ſeries infinitas. Memini me ex occaſione aliquando narrâſſe D. Barrovio, edendis lectionibus ſuis occupato, inſtructum me eſſe hujuſmodi methodo tangentes ducendi: ſed neſcio quo diverticulo, ab eâ ipſi deſcribendâ fuerim avocatus.

Sluſii methodum tangentes ducendi brevi publicè prodituram confido: quamprimum advenerit exemplar ejus, ad me tranſmittere ne grave ducas.

* Sc. in tractatu, quem Newtonus ſcripſit anno 1671. Miſſum autem fuit apographum hujus epiſtolæ ad Tſchurnhauſium menſe Maio 1675, & ad Leibnitium menſe Junio 1676.

Epiſtola

Epiſtola D. Sluſii *ad* Oldenburgh, *anno* 167$\frac{2}{3}$, 17 Januarii Leodii *data, quâ continetur methodus ejus ducendi tangentes, inter epiſtolas Regiæ Societatis aſſervatur Lib. N° VI. pag.* 11. *Legitur autem impreſſa in Tranſactionibus Philoſophicis, N° XC.*

Ex epiſtolâ D. Oldenburgh *ad D.* Sluſium, *anno* 167$\frac{2}{3}$, 29 Januarii *datâ, quâ prædictis* Sluſii *literis reſpondetur. Legitur autem exemplar ejus in libris Regiæ Societatis, N° VI. p.* 27. N° XXVII.

Statui, Deo dante, primâ occaſione methodum ipſam, prout epiſtolâ turâ continetur, Tranſactionibus Philoſophicis inſerere. Non ingratum interea fuerit, accipere quæ doctiſſimus noſter Newtonus, in Academiâ Cantabrigienſi Mathematum Profeſſor, de eodem argumento ad D. Collinium noſtrum, qui te ſummopere & jugiter colit, nuper perſcripſit in hæc verba.

" Non parum me juvat intelligere, mathematicos exteros in eandem
" mecum incidiſſe ducendi tangentes methodum. Qualem eam eſſe conji-
" ciam, ex hoc exemplo percipies. *Atque ita deinceps ut in præcedente ip-*
" *ſius* Newtoni *epiſtolâ habetur.*"

Hactenus Newtonus, quæ ideo nunc perſcribo, ut cum noviſſimis tuis comparare poſſis.

Epiſtola D. Sluſii *ad D.* Oldenburgh, *anno* 1673, 3 Maii Leodii *data, quâ continentur fundamenta methodi tangentium* Sluſianæ, *cujuſque aſſervatur exemplar in libris epiſtolarum Regiæ Societatis, N° VI. p.* 111, *impreſſa legitur in Phil, Tranſact. N° XCV.* N° XXVIII.

Ex epiſtolâ D. Oldenburgh *ad D.* Sluſium, *anno* 1673, 10 Julii *datâ. Legitur autem inter epiſtolas Regiæ Societatis, Lib. VI. p.* 196. N° XXIX.

En tibi, vir illuſtriſſime, impreſſum modum tuum demonſtrandi methodum tuam ducendi tangentes ad quaſlibet Curvas; quemadmodum poſtremis tuis literis eum mihi communicaveras. Subticui viri nomen offenſionis evitandæ cauſâ. Scripſit mihi D. Newtonus in hanc ſententiam.

" Ex priori tuâ epiſtolâ ſubdubitabam, exiſtimaretne celeberrimus Slu-
" ſius per ea, quæ ipſi de me ſcripſeras, me mihi tribuere methodum ip-
" ſius ducendi tangentes; donec intelligerem à D. Collinio, te ipſi ſigni-
" ficâſſe, eam, ex opinione tuâ, ſeriùs hîc inventam fuiſſe. Tibi quippe
" videtur, eam D. Sluſio perſpectam fuiſſe, aliquot annis priuſquam ederet
" Meſolabum ſuum, proindeque antequam ego eam intelligerem. At ſi
" res ſecus ſe haberet, cùm tamen eam primus communicaverit amicis ſuis
" & literato orbi, jure merito ipſi debetur. Quoad methodos illas, eæ-
" dem ſunt, quanquam, crediderim, ex principiis diverſis derivatæ. Neſ-
" cio tamen num ipſius principia eam largiantur adeo generalem, ac mea;

" quæ ad æquationes terminis ſurdis affectas ſe extendunt, abſque eorum
" ad aliam formam reductione."
Hæc ille, quæ in bonam partem à te acceptum iri confido.

N° XXX. *Excerpta ex epiſtolâ D.* Gothofredi Guilielmi Leibnitii *ad D.* Oldenburgh, Londini, *anno* $167\frac{2}{1}$, 3 Feb. *datâ. Hujus autographon in ſcriniis Regiæ Societatis extat, & exemplar ejus in libro epiſtolarum dictæ Societatis, N° VI. p.* 35, *deſcriptum legitur.*

Cùm heri apud illuſtriſſimum Boylium incidiſſem in clariſſimum Pellium, Mathematicum inſignem, ac de numeris incidiſſet mentio; commemoravi ego, ductus occaſione ſermonum, eſſe mihi methodum ex quodam differentiarum genere, quas voco generatrices, colligendi terminos ſeriei cujuſcunque continuè creſcentis, vel decreſcentis. Differentias autem generatrices voco, ſi datæ ſeriei inveniantur differentiæ, & differentiæ differentiarum, & ipſarum ex differentiis differentiarum differentiæ, &c. & ſeries conſtituatur ex termino primo & primâ differentiâ, & primâ differentiâ differentiarum, & primâ differentiâ ex differentiis differentiarum, &c. ea ſeries erit differentiarum generatricium, ut ſi ſeries continuè creſcens vel decreſcens fuit $a, b, c, d,$ poſitâ ∽ differentiæ notâ differentiæ generatrices erunt.

$$1a \,.\, 2a \backsim b \,.\, 3\overline{a \backsim b \backsim b \backsim c} \,.\, 4\overline{\overline{a \backsim b \backsim b \backsim b} \backsim \overline{b \backsim c \backsim c \backsim c}} \,.\, d.$$

$$\begin{array}{lllll}
4 & \overline{a \backsim b \backsim b \backsim c} & \backsim & \overline{b \backsim c \backsim c \backsim d} & \\
3 & a \backsim b \backsim b \backsim c & & b \backsim c \backsim c \backsim d & \\
2 & a \backsim b & & b \backsim c & c \backsim d \\
1 & a & & b & c \qquad d
\end{array}$$

Aut in numeris; ſi ſeries ſit numerorum cubicorum deinceps ab unitate creſcentium, differentiæ generatrices erunt numeri 0, 1, 6, 6. Voco autem generatrices, quia ex iis, certo modo multiplicatis, producuntur termini ſeriei; cujus uſus tum maximè apparet, cùm differentiæ generatrices ſunt finitæ, termini autem ſeriei infiniti; ut in propoſito exemplo numerorum cubicorum.

| | | |

multuariè percurri, & inveni veriſſimè dixiſſe Pellium. Sed & mihi tamen dandam operam credidi, ne qua in animis relinqueretur ſuſpicio, quaſi, tacito inventoris nomine, alienis meditationibus honorem mihi quærere voluiſſem; & ſpero appariturum eſſe, non adeo egenum me meditationum propriarum, ut cogar alienas emendicare. Duobus autem argumentis ingenuitatem meam vindicabo. Primò ſi ipſas ſchedas meas confuſas, in quibus non tantùm inventio mea, ſed & inveniendi modus occaſioque apparet, monſtrem: deinde ſi quædam momenti maximi, Regnaldo Moutonoque indicta, addam, quæ ab heſterno veſpere confinxiſſe me non ſit veriſimile, quæque non poſſunt facilè expectari à tranſcriptore.

Ex ſchedis meis occaſio inventi hæc apparet. Quærebam modum inveniendi differentias omnis generis poteſtatum; quemadmodum conſtat differentias quadratorum eſſe numeros impares: inveneramque regulam generalem ejuſmodi.

Datâ potentiâ gradûs dati præcedente, invenire ſequentem (vel contrà) diſtantiæ datæ, vel radicum datarum; ſeu invenire potentiarum gradûs dati, utcunque diſtantium, differentias. Multiplicetur potentia, gradûs proximè præcedentis, radicis majoris per differentiam radicum; & differentia potentiarum, gradûs proximè præcedentis, multiplicetur per radicem minorem. Productorum ſumma erit quæſita differentia potentiarum, quarum radices ſunt datæ. Eandem regulam ita inflexeram, ut ſufficeret, præter radices cujuſlibet gradûs, etiamſi non proximè præcedentis, potentias datarum radicum dari, ad differentias potentiarum alterius cujuſcunque, licet altioris, gradûs inveniendas. Et oſtendi quod in quadratis obſervatur, numeros impares eſſe eorum differentias, id non niſi regulæ propoſitæ ſubſumptionem eſſe.

His meditationibus defixus, quemadmodum in quadratis differentiæ ſunt numeri impares, ita quoque quæſivi, quales eſſent differentiæ cuborum; quæ cùm irregulares viderentur, quæſivi differentias differentiarum, donec inveni differentias tertias eſſe numeros ſenarios. Hæc obſervatio mihi aliam peperit. Videbam enim ex differentiis præcedentibus generari terminos differentiaſque ſequentes; ac proinde, ex primis, quas ideo voco generatrices, ut hoc loco 0.1.6.6, ſequentes omnes. Hoc concluſo, reſtabat invenire, quo additionis, multiplicationiſve, aut horum complicationis genere, termini ſequentes ex differentiis generatricibus producerentur. Atque ita reſolvendo experiundoque deprehendi primum terminum, 0, componi ex primâ differentiâ generatrice, 0, ſumptâ ſemel, ſeu, vice unâ: ſecundum, 1, ex primâ 0 ſemel, & ſecundâ 1 ſemel: tertium, 8, ex primâ 0 ſemel, ſecundâ 1 bis, & tertiâ 6 ſemel: nam $0\times1+1\times2+6\times1=8$. Quartum, 27, ex primâ 0 ſemel, ſecundâ 1 ter, tertiâ 6 ter, quartâ 6 ſemel: nam $0\times1+1\times3+6\times3+6\times1=27$, &c. idque analyſis mihi univerſale eſſe comprobavit. Hæc fuit occaſio obſervationis meæ, longè alia à Moutonianâ; qui cùm in tabulis condendis laboraret, in hoc calculandi compendium cum Regnaldo incidit. Nec vel illi vel Regnaldo adimenda laus, quòd & Briggius in logarithmicis ſuis jam olim talia quædam, obſervante Pellio, ex parte advertit. Mihi hoc ſupereſt; ut addam nonnulla illis indicta, ad amo-

liendum tranſcriptoris nomen; neque enim intereſt reipublicæ, quis obſervaverit; intereſt, quid obſervetur. Primum ergo illud adjicio, quod apud Moutonium non extat, & caput tamen rei eſt: quinam ſint illi numeri, quorum tabulam ille exhibet in infinitum continuandam, quorum ductu in differentias generatrices, productis inter ſe junctis, termini ſerierum generentur. Vides enim ex ipſo modo, quo tabula ab eo pag. 385 exhibetur, non fuiſſe id ei ſatis exploratum; alioqui enim veriſimile eſt ita tabulam fuiſſe diſpoſiturum, ut ea numerorum connexio atque harmonia appareret; niſi quis de induſtriâ texiſſe dicat: ita enim ſe habet pars tabulæ.

1	1					
2	1	1				
3	1	2	1			
(4)	1	3	3	1		
5	1	4	6	4	1	
6	1	5	10	10	5	1
7	1	6	15	20	15	6
8	1	7	21	35	35	21
9	1	8	28	56	70	56
10	1	9	36	84	126	126
11	1	10	45	120	210	252

Apparet, ex hujus tabulæ conſtructione, ſolam haberi rationem correſponſûs numerorum generantium cum numero termini generati; ut cùm terminus eſt quartus (4) producitur ex primâ differentiâ ſemel, ſecundâ ter 3, tertiâ ter 3, quartâ ſemel 1; ideo in eâdem (4) lineâ tranſverſâ locantur 1.3.3.1. Sed vel non obſervavit, vel diſſimulavit, autor correſponſum numerorum, ſi à ſummo deorſum eundo per columnas diſponantur hoc modo,

1	1					
2	1	1				
3	1	2	1			
4	1	3	3	1		
5	1	4	6	4	1	
6	1	5	10	10	5	1
7	1	6	15	20	15	6
8	1	7	21	35	35	21
9	1	8	28	56	70	56
10	1	9	36	84	126	126
11	1	10	45	120	210	252

Ita enim ſtatim vera genuinaque eorum natura ac generatio apparet; eſſe ſcilicet eos numeros, quos combinatorios appellare ſoleo, de quibus multa dixi in diſſertatiunculâ de arte combinatoriâ; quoſque alii appellant ordines numericos; alii in ſpecie primam columnam, unitatum; ſecundam, numerorum naturalium; tertiam, triangularium; quartam, pyramidalium; quintam, triangulo triangularium &c. de quibus integer extat tractatus Paſchalii ſub titulo Trianguli Arithmetici; in quo tamen proprietatem numerorum

rorum ejuſmodi tam illuſtrem tamque naturalem * non obſervatam ſum miratus. Sed eſt profecto caſus quidam in inveniendo, qui non ſemper maximis ingeniis maxima, ſed ſæpe etiam mediocribus nonnulla offert.

Hinc jam vera numerorum iſtorum natura, & tabulæ conſtructio, ſive à Regnaldo ſive à Moutonio diſſimulata, intelligitur: ſemper enim terminus datus columnæ datæ componitur ex termino præcedente columnæ tam præcedentis quàm datæ: atque illud quoque apparet, non opus eſſe moleſto calculo ad tabulam, à Moutonio propoſitam, continuandam, ut ipſe poſtulat; cùm hæ numerorum ſeries paſſim jam tradantur calculenturque.

Cæterùm Moutonius obſervatione iſtâ, ad interponendas medias proportionales inter duos extremos numeros datos; ego, ad inveniendos ipſos numeros extremos in infinitum, cum eorum differentiis, utendum cenſebam. Hinc ille, non niſi cùm differentiæ ultimæ evaneſcunt, aut pene evaneſcunt, uſum regulæ invenit; ego detexi innumerabiles caſus, regulâ quâdam inobſervatâ comprehendendos, ubi poſſum ex datis numeris finitis, certo modo multiplicatis, producere numeros plurimarum ſerierum in infinitum euntium, etſi differentiæ earum non evaneſcant.

Ex iiſdem fundamentis poſſum efficere in progreſſionibus problemata plurima; aut in numeris ſingularibus, aut in rationibus vel fractionibus. Poſſum enim progreſſiones addere ſubtrahereque, imo multiplicare quoque & dividere, idque compendioſè.

$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{5}$	$\frac{1}{6}$
$\frac{1}{6}$	$\frac{1}{10}$	$\frac{1}{15}$	$\frac{1}{21}$
$\frac{1}{10}$	$\frac{1}{20}$	$\frac{1}{35}$	$\frac{1}{56}$
$\frac{1}{15}$	$\frac{1}{35}$	$\frac{1}{70}$	$\frac{1}{126}$
&c.	&c.	&c.	&c.

Multa alia circa hos numeros obſervata ſunt à me, ex quibus illud eminet; quòd modum habeo ſummam inveniendi ſeriei fractionum in infinitum decreſcentium; quarum numerator unitas, nominatores verò numeri iſti triangulares aut pyramidales, aut triangulo-triangulares, &c.

* *Imo obſervata fuit. Vide Paſchalii Triangulum Arithmeticum, Pariſiis anno* 1665 *editum, pag.* 2, *ubi definitionum antepenultima hæc eſt.*

Le nombre de chaque celiule eſt egal a celuy de la cellule qui la precede, dans ſon rang perpendicularie, plus a celuy de la cellule qui la precede dans ſon rang parallele. Ainſi la cellule F, *c'eſt a dire le nombre de la cellule* F, *egale la cellule* C *plus la cellule* E; *& ainſi des autres.*

N° XXXI. *In ſcriniis etiam Regiæ Societatis aſſervantur autographa quinque epiſtolarum, à D.* Leibnitio *ad D.* Oldenburgum *eodem anno* 1673 *ſcriptarum; prima autem* Londini *data eſt,* Februarii 20; *reliquæ verò* Pariſiis, Martii 30, Aprilis 26, Maii 24, & Junii 8. *Omniumque, ſi ſecundum excipias, exemplaria leguntur in libro Regiæ Societatis N° VI. pag.* 34, 101, 115 & 137.

N° XXXII. *Hactenus D.* Leibnitius *in Arithmeticâ verſabatur: jam ad Geometriam ſe convertit; & anno proximo ad* Oldenburgium *ſcribit epiſtolas duas,* Pariſiis Jul. 15, & Octob. 36 *datas, quæ leguntur in libro epiſtolarum Regiæ Societatis N° VII. pag.* 93 & 110, *eædemque reperiuntur impreſſæ in tomo tertio Operum Mathematicorum D.* J. Wallis, & *in ſcriniis Reg. Societatis aſſervantur earum autographa.*

Ex harum priore 15 Julii *datâ.*

Diu eſt, quod nullas à me habuiſti literas, &c. Alia mihi theoremata ſunt, momenti non paulo majoris. Ex quibus illud imprimis mirabile eſt, cujus ope area circuli, vel ſectoris ejus dati, exactè exprimi poteſt per ſeriem quandam numerorum rationalium continuè productam in infinitum. Sed & methodos quaſdam analyticas habeo, generales admodum & latè fuſas, quas majoris facio quàm theoremata particularia & exquiſita.

Ex poſteriore 26 Octob. *datâ.*

N° XXXII. Porro, in eâ geometriæ parte rem memorabilem mihi eveniſſe nuncio. Scis D. Vicecomitem Brounkerum, & Cl. Nic. Mercatorem exhibuiſſe infinitam ſeriem numerorum rationalium, ſpatio Hyperbolæ æqualem. Sed hoc in Circulo efficere, hactenus potuit * nemo. Etſi enim illi Brounkerus & Walliſius dederint numeros rationales magis magiſque appropinquantes; nemo tamen dedit [*imo uterque dedit; ſed forte non ejus ſenſu,*] progreſſionem numerorum rationalium, cujus, in infinitum continuatæ, ſumma ſit exactè æqualis circulo. Sed verò mihi tandem feliciter ſucceſſit. Inveni enim ſeriem numerorum valdè ſimplicem, cujus ſumma exactè æquatur circumferentiæ circuli; poſito diametrum eſſe unitatem. Et habet ea ſeries id quoque peculiare, quod miras quaſdam circuli & hyperbolæ exhibet harmonias. Itaque tetragoniſmi circularis problema, jam à Geometriâ traductum eſt ad Arithmeticam Infinitorum: quod hactenus fruſtra quærebatur. Reſtat ergo tantùm, ut doctrina de ſerierum, ſeu progreſſionum numericarum, ſummis perficiatur. Quicunque hactenus quadraturam circuli exactam quæſivere, ne viam quidem aperuere, per quam eò pervenire poſſe ſpes ſit: *quod nunc primùm à me factum dicere auſim.* Ratio diametri ad circumferentiam exactè à me exhiberi poteſt per rationem, non quidem numeri ad numerum, id enim foret abſolutè inveniſſe; ſed per rationem numeri

* Collinius jam ante quadriennium ſeries Newtonianas, ante triennium Gregorianas, cum amicis communicare cœpit. Leibnitius in Angliâ diverſabatur anno ſuperiore (1673), & hujuſmodi ſeries nondum communicaverat; nec prius cum amicis in Angliâ communicare cœpit, quàm ab

numeri ad totam quandam ſeriem numerorum rationalium valde ſimplicem & regularem. Eâdem † methodo etiam arcûs cujuſlibet, cujus ſinus datur, geometricè exhiberi, per ejuſmodi ſeriem, valor poteſt; nullo ad integræ circumferentiæ dimenſionem recurſu. Ut adeo neceſſe non ſit, arcûs rationem ad circumferentiam noſſe.

Excerpta ex epiſtolâ D. Oldenburg *ad D.* Leibnitium, *anno* 1674, 8 Decembris *datâ, cujus aſſervatur autographum. Eadem autem legitur inter epiſtolas Regiæ Societatis, lib. N° VII. pag.* 119; *eſtque reſponſum ad literas D.* Leibnitii, 26 Octobris *præcedentis datas.* N° XXXIV.

Quod de profectu in curvilinearum dimenſione memoras, bene ſe habet: ſed ignorare te nolim Curvarum dimetiendarum rationem & methodum à laudato Gregorio, nec non ab Iſaaco Newtono, ad Curvas quaſlibet, tum mechanicas, tum geometricas, quin & circulum ipſum, ſe extendere: ita ſcilicet ut ſi in aliquâ Curvâ ordinatam dederis, iſtius methodi beneficio poſſis lineæ curvæ longitudinem, aream figuræ, ejuſdem centrum gravitatis, Solidum rotundum, ejuſque ſuperficiem, ſive erectam ſive inclinatam, Solidique rotundi ſegmenta ſecunda, horumque omnium converſa invenire: quin & dato quolibet arcu in quadrante, logarithmicum ſinum, tangentem vel ſecantem, non cognito naturali, & converſum computare. Quod verò ais neminem hactenus dediſſe progreſſionem numerorum rationalium, cujus, in infinitum continuatæ, ſumma ſit æqualis circulo; id verò tibi tandem feliciter ſucceſſiſſe, de eo quidem tibi gratulor, &c.

Ex epiſtolâ D. Leibnitii *ad D.* Oldenburg, Pariſiis *anno* 1675, 30 Martii *datâ. Extat autographum ſcriptoris; & reperitur deſcripta inter epiſtolas Reg. Soc. N° VII. pag.* 213. *Hâc autem reſpondetur ad ſupradictas* Oldenburgi *literas,* 8 Decembris *præcedentis datas.* N° XXXV.

Scribis, clariſſimum Newtonum veſtrum habere methodum exhibendi quadraturas omnes, omniumque curvarum ſuperficierum, & Solidorum, ex revolutione genitorum, dimenſiones, & centrorum gravitatis inventiones; per appropinquationes ſcilicet, ita enim interpretor. Quæ methodus, ſi eſt univerſalis & commoda, meretur æſtimari; nec dubito fore ingenioſiſſimo authore dignam. Addis, tale quid *Gregorio* innotuiſſe.

Ex epiſtolâ D. Oldenburg *ad D.* Leibnitium, *anno* 1675, 15 Aprilis *datâ; cujus habetur exemplar inter epiſtolas Reg. Societatis N° VII. pag.* 216. *Hâc reſpondetur ad D.* Leibnitii *literas,* 30 Martii *præcedentis datas: Anglicè autem extat, manu D.* Collins *deſignata ac* 10 Aprilis *data, eamque Latinè tranſtulit D.* Oldenburg & *ad D.* Leibnitium *miſit.* N° XXXVI.

D. Collinius, præmiſſâ ſalute, quæ ſequuntur remittit. Primo Cl. Gre-

ab Oldenburgo acceperat, ut mox patebit; neque alias communicavit, quàm quas acceperat.

† Methodum exhibendi arcum cujus ſinus datur, Leibnitius ab Oldenburgo poſtea quæſivit, *Maii* 12, 1676.

gorium,

gorium, in poſtremâ ſuâ ad illuſtrem Hugenium reſponſione, ſeriem ſuppeditâſſe ad ſemicircumferentiam circuli inveniendam, quæ talis.

Pone radium $= r$; dimidium latus quadrati inſcripti circulo $= d$; & differentiam inter radium & latus quadrati $= e$: ſemicircumferentia æqualis eſt $\frac{4rr}{2d - \frac{e}{3} - \frac{e^2}{90d} - \frac{e^3}{756d^3} - \frac{2e^4}{113400d^4} - \frac{266e^5}{74844000d^5}}$ — &c. in infinitum; quæ ſeries adeo produci poteſt, ut à ſemicircumferentiâ minus differat, quàm ulla quantitas aſſignabilis.

Editum hoc fuit à D. Gregorio, poſtquam D Mercatoris Logarithmotechnia jam extabat; quæ, quàm primùm viderat lucem, ad D. Barrovium à me fuit tranſmiſſa; qui, obſervato in eâ infinitæ ſeriei uſu ad logarithmos conſtruendos, reſcribebat, methodum illam jam aliquandiu excogitatam fuiſſe à ſucceſſore ſuo Newtono; omnibuſque Curvis, earumque portionibus, geometricis æquè ac mechanicis univerſim, applicatam: cujus rei ſpecimina quædam ſubjecit, viz.

Poſitâ pro radio unitate, datoque x pro ſinu, ad inveniendum z arcum ſeries hæc eſt: $z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 + \frac{35}{1152}x^9$ &c. in infinitum. Et extractâ radice hujus æquationis, methodo ſymbolicâ, ſi dederis z pro arcu, ad inveniendum x ſinum ſeries hæc eſt;

$$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362880}z^9 - \text{ &c.}$$

Atque hæc ſeries facilè continuatur in infinitum. Prioris beneficio, ex ſinu 30 grad. Ceulenii numeri facilè ſtruuntur.

Conſimiliter ſi ponas radium R, & B ſinum arcûs; zona, inter diametrum & chordam illi parallelam, eſt

$= 2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} - \frac{7B^{11}}{1408R^9}$ — &c. Atque eadem ſeries, mutatis ſignis termini ſecundi, quarti & ſexti, &c. inſervit aſſignandæ areæ zonæ æquilateris hyperbolæ, viz.

$$AFGB = 2RB + \frac{B^3}{3R} - \frac{B^5}{20R^3} + \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} + \frac{7B^{11}}{1408R^9} - \text{ &c.}$$

Rurſum, dato radio R, & ſinu verſo ſive ſagittâ a, ad inveniendam aream ſegmenti reſecti à chordâ: pone b^2 pro $2Ra$, & erit ſegmentum $= \frac{4ba}{3} - \frac{2a^3}{5b} - \frac{a^5}{14b^3} - \frac{a^7}{36b^5} - \frac{5a^9}{352b^7} - \frac{7a^{11}}{832b^9}$ — &c.

Et arcus integer $= 2b + \frac{a^2}{4b} + \frac{3a^4}{20b^3} + \frac{5a^6}{56b^5} + \frac{35a^8}{576b^7} + \frac{63a^{10}}{1408b^9} +$ &c.

Duæ hæ ſeries D. Gregorio habentur; quas exhibuit ex eo tempore, quo uſus eſt hâc methodo; quod ab ipſo aliquot poſt annis factum: poſtquam ſcilicet intellexerat D. Newtonum generatim eam applicâſſe. Exinde quoque ad nos miſit ſeries ſimiles ad tangentes naturales ex earundem arcubus,

* Hanc ſeriem D. Collins, initio anni 1671, à Gregorio acceperat, ut ſuprà; D. Leibnitius eandem cum amicis in Galliâ hoc anno ut ſuam communicavit, celatâ hâc epiſtolâ.

† His verbis patet ſeries, quas D. Leibnitius ſe ante annos aliquot inveniſſe profeſſus eſt, à communicatis diverſas fuiſſe. Subjungit etiam ipſe verbis diſertis *ſua a communicatis longe diverſa eſſe circa hanc rem meditata.* Vid. epiſt. *Maii* 12, 1676.

&

& converſim, obtinendum. Ex. gr. pone radium $=r$; arcum, a; tangentem, t: erit, $t=a+\frac{a^3}{3r^2}+\frac{2a^5}{15r^4}+\frac{17a^7}{315r^6}+\frac{62a^9}{2835r^8}+$ &c. Et converſim ex tangente invenire arcum ejus, * $a=t-\frac{t^3}{3r^2}+\frac{t^5}{5r^4}-\frac{t^7}{7r^6}+\frac{t^9}{9r^8}-$ &c.

Atque hoc factum cùm vides, facilè credideris, poſſe eâdem methodo æquè facilè ex arcu inveniri ſinum vel tangentem logarithmicum, abſque inventione naturalis, & converſim. Pronum quoque tibi fuerit credere, methodum hanc applicari poſſe ad rectificationem quarumlibet Curvarum; particulatim verò ad lineam quadratricem, & ad inveniendam aream illius figuræ: id quod antehac, nullâ demum cum methodo, fuit præſtitum. Atque ulteriore calculationis labore extendi poteſt ad inveniendas areas ſuperficierum in rotundis Solidis inclinantibus; nec non ad inveniendas Soliditates ſecundorum ſegmentorum in Solidis rotundis. E. G. Si Conoides aliqua ſecetur à plano tranſeunte per baſin ejus, poterit id vocari ſegmentum primum; & ſi hæc portio iterum ſecetur à plano, recto ad planum prius ſecans, portio, eum in modum ſecta, hoc ipſo intenditur, ut ſit ſegmentum [ſecundum].

Porro methodus eadem applicatur inveniendis radicibus purarum poteſtatum, valdè affectarum æquationum; ita ut ex quolibet numero, abſque logarithmorum ope, excitare poſſis quamlibet poteſtatem per ſaltum, & ex quâvis poteſtate, utut affectâ, invenire radicem ejus, vel quodvis medium illud inter & unitatem aſſignatum. D. Gregorius magno labore paravit ſeriem infinitam, generatim reſpectivis poteſtatibus affectis cujuſlibet æquationis propoſitæ adaptandam; ita ut quivis Algebræ cultor, penu ipſius inſtructus, mox aptare poſſit ſeriem aliquam, ad inveniendam quamlibet radicem cujuſvis æquationis propoſitæ; poſtquam innotuit, ad quod latus noti limitis radix ceciderit. Verum id hactenus nobis non communicavit, uti nec nos illum ad id faciendum ſolicitavimus; imprimis cùm ipſe lubens permittat Newtono, ut ille primus novæ hujus methodi dè infinitâ ſerie inventionem orbi mathematico patefaciat, &c.

Ex epiſtolâ D. Leibnitii *ad D.* Oldenburgh, *anno* 1675, 20 Maii, Pariſiis *datâ. Extat autographon ejus, eademque legitur in libro epiſtolarum Regiæ Societatis, N° VII. pag* 235. *Reſponſum autem eſt ad prædictas D.* Oldenburgi *literas* 15 Aprilis *datas.* N° XXXVII.

Literas tuas, multâ fruge algebraicâ refertas, accepi; pro quibus tibi, & doctiſſimo Collinio, gratias ago. Cùm nunc præter ordinarias curas mechanicis imprimis negotiis diſtrahar, non potui examinare ſeries, quas miſiſtis, ac cum † meis comparare. Ubi fecero, ‡ perſcribam tibi ſententiam meam: nam aliquot jam anni ſunt, quod inveni meas, viâ quâdam ſic ſatis ſingulari. Collinium ipſum magni facio; quoniam omnes puræ matheſeos

‡ N. B. Hoc nunquam fecit D. Leibnitius; ſed ubi ſeries duas primas per Mohrum quendam demum accepiſſet, poſtulavit methodum D. Newtoni, perveniendi ad iſtas duas ſeries, ad ſe mitti; quaſi nullas priùs ab Oldenburgo accepiſſet. Et hoc pacto, epiſtolam Oldenburgi oblivioni tradendo, licentiam obtinuit ſerierum ab eo acceptarum ultimam ſibi vindicandi.

partes

partes ab ipso egregiè cultas video. Multa habeo destinata, à quibus me deterrent calculi tantùm; qui nec susci pi facilè ab homine occupato, nec alteri, nisi doctissimo ac sinceriss imo, tutò credi possunt.

N° XXXVIII. *Ex Actis Eruditorum anno* 1691 *mense* Aprili, *pag.* 178, *habentur hæc* D. Leibnitii *verba.*

Jam anno 1675 *compositum habebam* * *opusculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum; sed quod, materiâ sub manibus crescente, limare ad editionem non vacavit, postquam aliæ occupationes supervenere; præsertim cùm nunc prolixius exponere vulgari more, quæ Analysis nostra nova paucis exhibet, non satis operæ pretium videatur. Interim insignes quidam Mathematici, quibus veritas primariæ nostræ propositionis dudum in actis publicatæ innotuit, pro humanitate suâ, nostri qualiscunque inventi candidè meminere.*

N° XXXIX. *Excerpta ex schediasmatis manu D.* Collins *exaratis, & in scriniis ejus repertis, & nonnullis in locis* Oldenburgi *calamo castigatis; quæ quidem D.* Oldenburg *D.* Tschurnhausio *transmittenda acceperat, & Latinè verterat. Extant autem tum autographa D.* Collins, *tum responsum ad eadem D.* Oldenburgo *redditum, cum titulo manu ejus inscripto,* " Responsum ad Scriptum D. *Collinii* de *Cartesii* Inventis," Accep. d. 8 *Junii* 1765."

Nonnulli Cartesium arrogantiæ insimulârunt, asserentem se ex omnibus modis methodisve possibilibus, in optimam simplicissimamque incidisse. An ullibi hoc affirmaverit Cartesius plane nescio: certum tamen, est methodum ducendi tangentes multum promotam fuisse à Newtono & Gregorio. Ita liquet ex Newtoni epistolâ anno 1672, 10 *Decemb.* datâ. Vide N° XXVI.

N. B. In hoc schediasmate habetur apographum epistolæ hujus, ut & apographum epistolæ Gregorii *ad* Collins 5 Sept. 1670, *suprà impressæ.*

N° XL. *Ex epistolâ D* Oldenburg *ad D.* Leibnitium, *anno* 1675, 24 Junii *datâ, & in libro epistolarum Regiæ Societatis, N° VII. pag.* 243 *descriptâ. Responsum autem est ad præcedentes D.* Leibnitii *literas,* 20 Maii *datas.*

Dominus Newtonus, beneficio logarithmorum graduatorum in scalis, παραλλήλως locandis ad distantias æquales, vel circulorum concentricorum eo modo graduatorum adminiculo, invenit radices æquationum. Tres regulæ rem conficiunt pro cubicis, quatuor pro biquadraticis. In harum dispositione respectivæ coefficientes omnes jacent in eâdem lineâ rectâ; à cujus puncto, tam remoto à primâ regulâ ac scalæ graduatæ sunt ab invicem, linea recta iis superextenditur, unà cum præscriptis conformibus genio æquationis; quâ in regularum unâ datur potestas pura radicis quæsitæ.

Ex

* Quadratura Arithmetica, de quâ hîc agitur, ea est, quam Gregorius cum D. Collinio, initio anni 1671, Oldenburgus cum D. Leibnitio, hoc anno, communicavit. De hâc quadraturâ D. Leibnitius opusculum vulgari more composuit, & cum amicis hoc anno communicare cœpit: anno proximo scriptum polivit, ut cum Oldenburgo communicaretur: anno tertio in patriam redux negotiis

Ex epistolâ D. Leibnitii *ad D.* Oldenburg, Parisiis *anno* 1675, 12 Julii *datâ.* N° XLI.
Hujus extat autographum; habeturque exemplar ejus in libro epistolarum Regiæ Societatis, N° VII. pag. 149. *Responsum autem est ad literas præcedentes D.* Oldenburgi, *& impressa legitur inter Opera D.* Wallisii. *In hâc perperam scribitur* Parius *pro* Darius.

Methodum celeberrimi Newtoni, radices æquationum inveniendi per instrumentum, credo differre à meâ. Neque enim video in meâ, quid aut logarithmi aut circuli concentrici conferant. Quoniam tamen rem vobis non ingratam video, conabor absolvere, ac tibi communicare, quamprimum otii sat erit.

Scripsisti aliquoties, vestrates omnium Curvarum dimensiones per appropinquationem dare. Velim nosse, an possint dare geometricè dimensionem Curvæ ellipseos vel hyperbolæ, ex datâ circuli, aut hyperbolæ, quadraturâ.

Ex epistolâ D. Oldenburgi *ad D.* Leibnitium, *anno* 1675, 30 Septemb. N° XLII.
datâ. Cujus extat exemplar manu D. Oldenburg *descriptum. Legitur etiam in libro Regiæ Societatis N° VII. pag.* 159, *& responsum est ad præcedentem.*

Scriptum quoddam Belgicum Belga quidam, Georgius Mohr vocatus, Algebræ & Mechanicæ probè peritus, apud Collinium nostrum reliquit; qui apographum ejus, quale hîc insertum vides, impertire tibi voluit.—— Tschurnhausius nuper Parisios hinc profectus est; & te sine dubio jam salutavit. Scire cupis, an dare nostrates geometricè possint dimensionem Curvæ ellipseos aut hyperbolæ, ex datâ circuli, aut hyperbolæ, quadraturâ. Ait Collinius, illos id præstare non posse geometricâ præcisione; sed dare eos posse ejusmodi approximationes, quæ quâcunque quantitate datâ minùs à scopo aberrabunt. Et speciatim, quod attinet alicujus arcûs circuli rectificationem, impertire tibi poterit laudatus Tschurnhausius methodum, à Gregorio nostro inventam, quam, cùm apud nos esset, Collinius ipsi communicavit.

Ex epistolâ D. Leibnitii *ad D.* Oldenburg, Parisiis 28 Decembris *anno* 1675 N° XLIII.
datâ. Extat autographum ejus, describiturque in libro Regiæ Societatis, N° VII. pag. 189, *& à D.* Wallisio *impressa est.*

Duarum tibi literarum debitor, rogo ne sequius interpreteris silentium meum; soleo enim interrumpi nonnunquam, & hæc studia per intervalla tractare. Quòd Tschurnhausium ad nos misisti, fecisti pro amico: multum enim ejus consuetudine delector; & ingenium agnosco in juvene præclarum & magna promittens. Inventa mihi ostendit non pauca, Analytica & Geometrica, sane perelegantia. Unde facilè judico, quid ab eo expectari possit.

gotiis publicis interesse cœpit; &, materiâ sub manibus crescente, opus ad editionem limare non ampliùs vacavit. Sed neque operæ pretium duxit, subinde prolixius exponere, vulgari more, quæ analysis sua nova paucis exhibet. Inventa est igitur hæc analysis, postquam D. Leibnitius opusculum, vulgari more compositum, polire & limare desiit, & negotiis publicis interesse cœpit.

Habebis & à me inſtrumentum æquationes omnes geometricas conſtruendi unicum: et meam quadraturam circuli, ejuſque partium, per ſeriem numerorum rationalium infinitam; de quâ aliquoties ſcripſi, & quam jam pluſquam biennio abhinc geometris hîc communicavi.

N° XLIV. *Ex epiſtolâ D.* Leibnitii *ad D.* Oldenburg, Pariſiis, 12 Maii, *anno* 1676 *datâ; cujus autographum in ſcriniis Regiæ Societatis aſſervatur, cum notis manu* Oldenburgi *in tergo ſcriptis.*

Cùm Gregorius Mohr Danus [*ſuperiùs Belga*], in Geometrâ & Analyſi verſatiſſimus, nobis attulerit communicatam ſibi à doctiſſimo Collinio veſtro expreſſionem relationis inter arcum & ſinum, per infinitas ſeries ſequentes: poſito ſinu x, arcu z, radio 1.

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 \text{ \&c.}$$

$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 \text{ \&c.}$$

Hæc * INQUAM, cum nobis attulerit ille, quæ mihi valdè ingenioſa videntur; & poſterior imprimis ſeries elegantiam quandam ſingularem habeat; ideo rem gratam mihi feceris, vir clariſſime, ſi demonſtrationem tranſmiſeris. Habebis viciſſim mea ab his longè diverſa, circa hanc rem meditata; de quibus jam aliquotab hinc annis ad te perſcripſiſſe credo; demonſtratione tamen non additâ, quam † nunc polio. Oro ut Cl. Collinio multam à me ſalutem dicas: is facilè tibi materiam ſuppeditabit ſatisfaciendi deſiderio meo.

N° XLV. *Ex epiſtolâ D.* Collins *ad D.* Oldenburgum, *D.* Leibnitio *tum* Pariſiis *agenti tranſmittendâ. Hujus exemplar, anno* 1676, 14 Junii, *manu ipſius D.* Collins *deſcriptum, ac in ſcriniis ejus repertum, etiamnum conſervatum eſt.*

Reſpondeas, ſi placet, ad ea quæ quærit D. Leibnitius, in literis ejus 12 *Maii* datis, ſeriei primæ numeros coefficientes, $\frac{1}{6}, \frac{3}{40}, \frac{5}{112}, \frac{35}{1152}$, hoc modo compoſitos eſſe, $\frac{1\times1}{2\times3} = \frac{1}{6}$; & $\frac{1}{6}\times\frac{3\times3}{4\times5} = \frac{3}{40}$; & $\frac{3}{40}\times\frac{5\times5}{6\times7} = \frac{5}{112}$; & $\frac{5}{112}\times\frac{7\times7}{8\times9} = \frac{35}{1152}$; & $\frac{35}{1152}\times\frac{9\times9}{10\times11} = \frac{63}{2816}$; atque ita deinceps in infinitum. Unde intelligi poſſit, hanc ſeriem elegantiâ minimè cedere converſæ ejuſdem; quæ tamen illi magis arridet. Meditata ejus de eodem argumento, cùm fundamentis planè diverſis innitantur, non poſſunt nobis non eſſe acceptiſſima; atque exoptamus ea fidem noſtram exuperare poſſe. Hujus autem methodi ea eſt præſtantia, ut cùm tam latè pateat, ad nullam hæreat difficultatem. Gregorium autem alioſque in eâ fuiſſe opinione arbitror, ut quicquid uſpiam antea de hâc re innotuit, quaſi dubia diluculi lux fuit, ſi cum meridianâ claritate conferatur.

* Quaſi ante annum eaſdem non acceperit ab Oldenburgo.

Hoc

Hoc anno, cùm D. Gregorius, *anno ſuperiore ad finem vergente, emortuus* N° XLVI.
eſſet, quæ cum amicis communicaverat in unum corpus, ſolicitante D. Leibnitio, *collecta ſunt. Et extat collectio, manu D.* J. Collins *exarata, cum hoc titulo;*

Excerpta ex D. Gregorii *epiſtolis cum D.* Leibnitio *communicandâ; tibique poſtquam perlegerit ille, reddenda. Et ſic orditur.*

D. H. Oldenburg *Armigero.*

Quandoquidem impenſè rogâſti me, permotus ſolicitationibus D. Leibnitii & aliorum ex Academiâ Regiâ Pariſinâ, ut hiſtoriolam aliquam concinnarem, ſtudia & inventa doctiſſimi D. Jacobi Gregorii, nuper defuncti, exhibentem; quoniamque arcta inter nos amicitia, crebraque dum viveret literarum reciprocatio fuit; in honorem nominis ejus, quæcunque majoris momenti in literis ejus occurrunt, ſummâ fide in unum colligere ſtatuo, &c.

In hâc collectione habetur epiſtola ſuperiùs impreſſa Gregorii *ad* Collins 5 Sept. 1670. *Habetur & epiſtola ſuperiùs impreſſa, quâ* Gregorius *quadraturam prædictam arithmeticam, initio anni* 1671, *cum D.* Collins *communicavit. Habetur & epiſtola D.* Newtoni *ad D.* Collins, 10 Decemb. 1672 *data, & ſuperiùs impreſſa; in quâ* Newtonus *ſe methodum generalem habere dicit ducendi tangentes, quadrandi curvilineas, & ſimilia peragendi; & methodum exemplo ducendi tangentes exponit: quam methodum D.* Leibnitius *differentialem poſtea vocavit. Hæc collectio ad D.* Leibnitium *miſſa fuit* 26 Junii 1676.

Ex epiſtolâ D. Collins *ad D.* Davidem Gregorium, *prædicti* Jacobi Gregorii N° XLVII.
nuper defuncti fratrem. Data autem eſt anno 1676, 11 Auguſti, *ejuſque habetur exemplar manu ipſius D.* Collins *deſcriptum.*

Hiſtoriolam compoſui, quâ in unum congeſſi, quæcunque unquam à fratre tuo, de rebus mathematicis, vel literis, aliaſve ſcripto vel colloquio, acceperim: eo fine, ut eandem ſcriniis Regiæ Societatis (cujus erat ſodalis) commiſſam & aſſervatam, amici ejus inſpicere poſſint; vel ſi libuerit, ſoluto pretio, tranſcriptam habere. Conſtat autem duodecim circiter ſchedis. Me verò nihil omiſiſſe, quod alicujus momenti eſſe poterit, ſi nonnulla cum Hugenio aliiſve controverſa excipias, aras ſacras juraturus contingere auſim. Mathematicis Gallis quouſque profecerat, quæque reliquerat frater tuus, ſcire aventibus, me operam dediſſe ut iis ſatisfacerem, ex ſequentibus comperies. *Sub finem autem exemplaris hujus epiſtolæ hæc ſubjunxerat D.* Collins.

Eruditi ex Academiâ Regiâ Pariſienſi, auditâ D. Gregorii morte, cupidè ſciſcitabantur ea, quæ moriens reliquerat; ſimulque narrationem eorum, quæ attinent doctrinam ſerierum infinitarum, apud nos repertam petebant: ſequentem ideo ad eos tranſmittendam curavi, ac deinde ad Davidem Gregorium, fratrem Jacobi ſuperſtitem.

† Opuſculum prædictum de Quadratura Arithmetica D. Leibnitius polire perrexit.

Quod attinet doctrinam ſerierum infinitarum, Mercator, in Logarithmotechniâ ſuâ, primum ſpecimen ejus orbi exhibuit; applicando eam ad hyperbolæ quadraturam tantùm, & ad logarithmorum conſtructionem, abſque radicum extractione. Hanc ipſam ejus doctrinam à D. Walliſio, in Tranſ. Philoſoph. illuſtratam habemus; eamque poſtea adauxit & promovit D. Gregorius in Exercitationibus ejus Geometricis, eodem anno editis.

Paucos poſt menſes quàm editi ſunt hi libri, miſſi ſunt ad D. Barrovium Cantabrigiæ. Ille autem reſponſum dedit, hanc infinitarum ſerierum doctrinam jam ante biennium à D. Iſaaco Newtono inventam fuiſſe; & quibuſvis figuris generaliter applicatam: ſimulque tranſmiſit D. Newtoni opus manuſcriptum, à D. Collins deinde cum D. Vicecomite Brounker, Regiæ Societatis tum Præſide, communicatum. Barrovio autem cathedram mathematicam abdicante, Newtonus, ab eodem commendatus, in ſucceſſorem ejus electus eſt; & de hâc doctrinâ publicè prælegit: lectioneſque ejus in bibliothecâ publicâ Cantabrigienſi aſſervantur.

Collins deinde, mediante D. Barrovio D. Newtono familiaris factus, literarum commercium cum eo habuit; & ab eo epiſtolam obtinuit 10 *Decembris*, anno 1672 datam, quâ docet modum ducendi tangentes ad Curvas geometricas, ope æquationis, quâ relatio inter ordinatim applicatas & abſciſſas exprimitur. Vide epiſtolam hanc, N° XXVI.

Collins etiam in diverſis literis, anno 1669 ad D. Gregorium datis, eidem ſignificavit Newtoni in hâc materiâ ſucceſſus. Gregorius autem contrà, ſe quoque plures habere pro circulo ſeries; ſimulque petiit nonnullas è Newtonianis, quas cum propriis conferre voluit, ad ſe mitti. Miſit igitur aliquas D. Collins; quas Gregorius à ſuis prorſus diverſas, & faciliores calculoque aptiores inveniens, haud levi ſtudio in eandem ipſam Newtoni methodum incidit, circa annum exeuntem 1670: ſicut ipſe apertè in epiſtolâ 19 *Decemb.* teſtatur. N° XVIII.

Cùm D. Leibnitius *methodum perveniendi ad ſeries, anno ſuperiori ſibi miſſas deſideraret, & ut* Gregoriana *omnia* Lutetiam Pariſiorum *mitterentur:* Oldenburgus & Collins, Newtonum *enixè rogârunt, ut ipſe methodum ſuam deſcriberet cum D.* Leibnitio *communicandam.*

N° XLVIII. *Epiſtola prior D.* Iſaaci Newtoni, *Matheſeos Profeſſoris in celeberrimâ Academiâ* Cantabrigienſi, *ad D.* Henricum Oldenburg, *Regalis Societatis* Londini *Secretarium*, 13 Junii 1676; *cum illuſtriſſimo viro D.* Godfredo Gulielmo Leibnitio *(eo mediante) communicanda. Literis* Oldenburgi (26 Junii) *ad* Leibnitium *miſſa.*

NEWTONI AD OLDEN- QUAMQUAM D. Leibnitii modeſtia, in excerptis quæ ex epiſtolâ ejus ad me nuper miſiſti, noſtratibus multum tribuat circa ſpeculationem quandam Infinitarum Serierum, de quâ jam cœpit eſſe rumor: nullus dubito tamen quin ille, non tantùm, quod aſſerit, methodum reducendi quantitates quaſcunque

cunque in ejuſmodi ſeries, ſed & varia compendia, fortè noſtris ſimilia, ſi non & meliora, adinvenerit. BURGUM PRIOR.

Quoniam tamen ea ſcire pervelit, quæ ab Anglis hâc in re inventa ſunt; & ipſe ante annos aliquot in hanc ſpeculationem inciderim: ut votis ejus aliquâ ſaltem ex parte ſatisfacerem, nonnulla eorum, quæ mihi occurrerunt, ad te tranſmiſi.

Fractiones in infinitas ſeries reducuntur per diviſionem; & quantitates radicales, per extractionem radicum; perinde inſtituendo operationes iſtas in ſpeciebus, ac inſtituri ſolent in decimalibus numeris. Hæc ſunt fundamenta harum reductionum.

Sed extractiones radicum multum abbreviantur per hoc * Theorema.

$$\overline{P+PQ}\Big|^{\frac{m}{n}} = P^{\frac{m}{n}} + \frac{m}{n}AQ + \frac{m-n}{2n}BQ + \frac{m-2n}{3n}CQ + \frac{m-3n}{4n}DQ + \&c.$$

Ubi $P+PQ$ ſignificat quantitatem, cujus radix, vel etiam dimenſio quævis, vel radix dimenſionis, inveſtiganda eſt; P, primum terminum quantitatis ejus; Q, reliquos terminos diviſos per primum. Et $\frac{m}{n}$, numeralem indicem dimenſionis ipſius $P+PQ$: ſive dimenſio illa integra ſit; ſive, ut ita loquar, fracta; ſive affirmativa, ſive negativa. Nam, ſicut analyſtæ, pro *aa*, *aaa*, &c. ſcribere ſolent a^2, a^3, &c. ſic ego, pro $\sqrt{a}$, $\sqrt{a^3}$, $\sqrt{c.a^5}$, &c. ſcribo $a^1 a^{\frac{3}{2}} a^{\frac{5}{3}}$; & pro $\frac{1}{a}$, $\frac{1}{aa}$, $\frac{1}{aaa}$, ſcribo a^{-1}, a^{-2}, a^{-3}. Et ſic pro $\frac{aa}{\sqrt{c.a^3+bbx}}$, ſcribo $aa \times \overline{a^3+bbx}\Big|^{-\frac{1}{3}}$; & pro $\frac{aab}{\sqrt{c:a^3+bbx} \times a^3+bbx}$: ſcribo $aab \times \overline{a^3+bbx}\Big|^{-\frac{2}{3}}$. In quo ultimo caſu, ſi $\overline{a^3+bbx}^{-\frac{2}{3}}$ concipiatur eſſe $\overline{P+PQ}\Big|^{\frac{m}{n}}$ in regulâ: erit $P=a^3$; $Q=\frac{bbx}{a^3}$; $m=-2$; $n=3$. Denique, pro terminis inter operandum inventis in quoto, uſurpo A, B, C, D, &c. Nempe A, pro primo termino $P^{\frac{m}{n}}$; B, pro ſecundo $\frac{m}{n}AQ$; & ſic deinceps. Cæterùm uſus regulæ patebit exemplis.

Exemplum 1. Eſt $\sqrt{cc+xx}$ (ſeu $\overline{cc+xx}\Big|^{\frac{1}{2}}$) $= c + \frac{xx}{2c} - \frac{x^4}{8c^3} + \frac{x^6}{16c^5} - \frac{5x^8}{128c^7} + \frac{7x^{10}}{256c^9}$ — &c. Nam, in hoc caſu, eſt $P=cc$; $Q=\frac{xx}{cc}$; $m=1$; $n=2$; A $(=P^{\frac{m}{n}}=cc^{\frac{1}{2}})=c$. B $(=\frac{m}{n}AQ)=\frac{xx}{2c}$. C $(=\frac{m-n}{2n}BQ)=-\frac{x^4}{8c^3}$. Et ſic deinceps.

Exemplum 2. Eſt $\sqrt{5:c^5+c^4x-x}$: (id eſt, $\overline{c^5+c^4x-x^5}\Big|^{\frac{1}{5}}$) $= c + \frac{c^4x-x^5}{5c^4} - \frac{2c^8xx+4c^4x^6-2x^{10}}{25c^9}$ + &c. Ut patebit ſubſtituendo in allatam regulam, 1 pro *m*, 5 pro *n*, c^5 pro P, & $\frac{c^4x-x^5}{c^5}$ pro Q. Poteſt etiam $-x^5$ ſubſtitui pro P, & $\frac{c^4x+c^5}{-x^5}$ pro Q. Et tunc evadet $\sqrt{5:c^5+c^4x-x^5}$: $= -x +$

* Reſolutionem binomii in hujuſmodi ſeriem anno 1669 Newtono innotuiſſe patet, ex analyſi ſuprà impreſſâ. Vid. ſuprà, N° XII.

c^4x

NEWTONI AD OLDEN- $\frac{c^4x+c^5}{5x^4}+\frac{2c^8xx+4c^9x+2c^{10}}{25x^9}$ + &c. Prior modus eligendus eſt, ſi x valdè parvum ſit; poſterior, ſi valdè magnum.

Exem. 3. Eſt $\frac{N}{\sqrt[3]{c:y^3-a^2y}}$ (hoc eſt, $N\times\overline{y^3-a^2y}^{-\frac{1}{2}}$) æqualis $N\times\frac{1}{y}+\frac{aa}{3y^3}+\frac{2a^4}{9y^5}+\frac{14a^6}{81y^7}$ &c. Nam $P=y^3$. $Q=-\frac{aa}{yy}$. $m=-1$, $n=3$. $A\left(=P^{\frac{m}{n}}=y^3\times-\frac{1}{3}\right)=y^{-1}$, hoc eſt $\frac{1}{y}$. $B\left(=\frac{m}{n}AQ=-\frac{1}{3}\times\frac{1}{y}\times-\frac{aa}{yy}\right)=\frac{aa}{3y^3}$ &c.

Exem. 4. Radix cubica ex quadrato-quadrato ipſius $d+e$, hoc eſt $\overline{d+e}^{\frac{4}{3}}$, eſt $d^{\frac{4}{3}}+\frac{4ed^{\frac{1}{3}}}{3}+\frac{2ee}{9d^{\frac{2}{3}}}-\frac{4e^3}{81d^{\frac{5}{3}}}$ + &c. Nam $P=d$. $Q=\frac{e}{d}$. $m=4$. $n=3$. $A\left(=P^{\frac{m}{n}}\right)=d^{\frac{4}{3}}$, &c.

Exem. 5. Eodem modo ſimplices etiam poteſtates eliciuntur. Ut, ſi quadrato-cubus ipſius $d+e$, (hoc eſt, $\overline{d+e}^5$, ſeu $\overline{d+e}^{\frac{5}{1}}$) deſideretur. Erit juxta regulam, $P=d$. $Q=\frac{e}{d}$. $m=5$. & $n=1$. Adeoque $A\left(=P^{\frac{m}{n}}\right)=d^5$. $B\left(=\frac{m}{n}AQ\right)=5d^4e$. & ſic $C=10d^3ee$. $D=10dde^3$. $E=5de^4$. $F=e^5$. & $G\left(=\frac{m-5n}{6n}FQ\right)=0$. Hoc eſt, $\overline{d+e}^5=d^5+5d^4e+10d^3ee+10dde^3+5de^4+e^5$.

Exem. 6. Quinetiam diviſio, ſive ſimplex ſit, ſive repetita, per eandem regulam perficitur. Ut ſi $\frac{1}{d+e}$, hoc eſt, $\overline{d+e}^{-1}$, ſive $\overline{d+e}^{-\frac{1}{1}}$, in ſeriem ſimplicium terminorum reſolvendum ſit: erit, juxta regulam, $P=d$. $Q=\frac{e}{d}$. $m=-1$. $n=1$. & $A\left(=P^{\frac{m}{n}}=d^{-\frac{1}{1}}\right)=d^{-1}$, ſeu $\frac{1}{d}$. $B\left(=\frac{m}{n}AQ=-1\times\frac{1}{d}\times\frac{e}{d}\right)=-\frac{e}{dd}$. Et ſic $C=\frac{ee}{d^3}$. $D=-\frac{e^3}{d^4}$, &c. Hoc eſt, $\frac{1}{d+e}=\frac{1}{d}-\frac{e}{dd}+\frac{ee}{d^3}-\frac{e^3}{d^4}$, &c.

Exem. 7. Sic & $\overline{d+e}^{-3}$, hoc eſt unitas ter diviſa per $d+e$, vel ſemel per cubum ejus, evadit $\frac{1}{d^3}-\frac{3e}{d^4}+\frac{6ee}{d^5}-\frac{10e^3}{d^6}$ + &c.

Exem. 8. Et $N\times\overline{d+e}^{-\frac{1}{3}}$, hoc eſt N diviſum per radicem cubicam ipſius $d+e$, evadit $N\times\frac{1}{d^{\frac{1}{3}}}-\frac{e}{3d^{\frac{4}{3}}}+\frac{2ee}{9d^{\frac{7}{3}}}-\frac{14e^3}{81d^{\frac{10}{3}}}$ + &c.

Exem. 9. Et $N\times\overline{d+e}^{-\frac{3}{5}}$, hoc eſt N diviſum per radicem quadrato-cubicam ex cubo ipſius $d+e$, ſive $\frac{N}{\sqrt[5]{c:d^3+3ddc+3dee+e^3}}$, evadit $N\times\frac{1}{d^{\frac{3}{5}}}-\frac{3e}{5d^{\frac{8}{5}}}+\frac{12ee}{25d^{\frac{13}{5}}}-\frac{52e^3}{125d^{\frac{18}{5}}}$ + &c.

Per eandem regulam, geneſes poteſtatum, diviſiones per poteſtates aut per quantitates radicales, & extractiones radicum altiorum in numeris, etiam commodè inſtituuntur.

Extractiones

Extractiones radicum æquationum affectarum in ſpeciebus imitantur earum extractionem in numeris. Sed methodus Vietæ & Oughtredi noſtri huic negotio minùs idonea eſt. Quapropter aliam excogitare adactus ſum, *cujus ſpecimina, ne repetantur, vide in Tractatu de Analyſi*, pag. &c. &c. BURGUM PRIOR.

Dicam tantùm in genere, quòd radix cujuſvis æquationis ſemel extracta, pro regulâ reſolvendi conſimiles æquationes aſſervari poſſit; quódque ex pluribus ejuſmodi regulis, regulam generaliorem plerumque efformare liceat; & quòd radices omnes, ſive ſimplices ſint, ſive affectæ, modis infinitis extrahi poſſint; de quorum ſimplicioribus itaque ſemper conſulendum eſt.

Quomodo ex æquationibus, ſic ad infinitas ſeries reductis, areæ & longitudines Curvarum, contenta & Superficies ſolidorum, vel quorumque ſegmentorum figurarum quarumvis, eorumlibet centra gravitatis determinentur; & quomodo etiam Curvæ omnes Mechanicæ ad ejuſmodi æquationes infinitarum ſerierum reduci poſſint; indeque problemata circa illas reſolvi, perinde ac ſi Geometricæ eſſent; nimis longum foret deſcribere. Sufficiat ſpecimina quædam talium problematum recenſuiſſe: inque iis, brevitatis gratiâ, literas A, B, C, D, &c. pro terminis ſeriei, ſicut ſub initio, nonnunquam uſurpabo. N° XLIX.

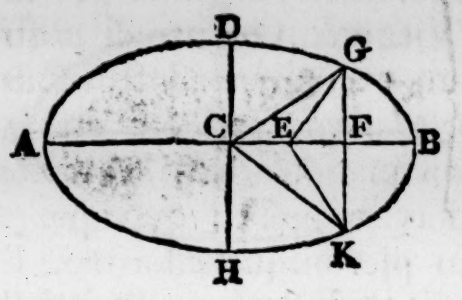
1. Si ex dato ſinu recto, vel ſinu verſo, arcus deſideretur: ſit radius r, & ſinus rectus x. Eritque arcus $= x + \frac{x^3}{6rr} + \frac{3x^5}{40r^4} + \frac{5x^7}{112r^6} +$ &c. Hoc eſt, $= x + \frac{1\times1\times xx}{2\times3\times rr}\mathrm{A} + \frac{3\times3xx}{4\times5rr}\mathrm{B} + \frac{5\times5xx}{6\times7rr}\mathrm{C} + \frac{7\times7xx}{8\times9rr}\mathrm{D} +$ &c. Vel ſit d diameter, ac x ſinus verſus; & erit arcus $= d^{\frac{1}{2}}x^{\frac{1}{2}} + \frac{x^{\frac{3}{2}}}{6d^{\frac{1}{2}}} + \frac{3x^{\frac{5}{2}}}{40d^{\frac{3}{2}}} + \frac{5x^{\frac{7}{2}}}{112d^{\frac{5}{2}}}$ &c. Hoc eſt, $= \sqrt{dx}$ in $1 + \frac{x}{6d} + \frac{3xx}{40dd} + \frac{5x^3}{112d^3} +$ &c.

2. Si viciſſim ex dato arcu deſideretur ſinus: ſit radius r, & arcus z. Eritque ſinus rectus $= z - \frac{z^3}{6rr} + \frac{z^5}{120r^4} - \frac{z^7}{5040r^6} + \frac{z^9}{362880r^8} -$ &c. Hoc eſt, $= z - \frac{zz}{2\times3rr}\mathrm{A} - \frac{zz}{4\times5rr}\mathrm{B} - \frac{zz}{6\times7rr}\mathrm{C} -$ &c. Et ſinus verſus $= \frac{zz}{2r} - \frac{z^4}{24r^3} + \frac{z^6}{720r^5} - \frac{z^8}{40320r^7} +$ &c. Hoc eſt, $\frac{zz}{1\times2r} - \frac{zz}{3\times4rr}\mathrm{A} - \frac{zz}{5\times6rr}\mathrm{B} - \frac{zz}{7\times8rr}\mathrm{C} -$ &c.

3. Si arcus capiendus ſit in ratione datâ ad alium arcum: eſto diameter $= d$; chorda arcûs dati $= x$; & arcus quæſitus ad arcum illum datum ut n ad 1. Eritque arcûs quæſiti chorda $= nx + \frac{1-nn}{2\times3dd}xx\mathrm{A} + \frac{9-nn}{4\times5dd}xx\mathrm{B} + \frac{25-nn}{6\times7dd}xx\mathrm{C} + \frac{49-nn}{8\times9dd}xx\mathrm{D} + \frac{81-nn}{10\times11dd}xx\mathrm{E} +$ &c.

Ubi nota, quòd cùm n eſt numerus impar, ſeries deſinet eſſe infinita; & evadet eadem, quæ prodit per vulgarem Algebram, ad multiplicandum datum angulum per iſtum numerum n.

NEWTONI AD OLDEN-



4. Si in axe alterutro, AB, ellipſeos ABD, cujus centrum C, & axis alter DH, detur punctum aliquod, E; circa quod recta, EG, occurrens ellipſi in G, motu angulari feratur; & ex datâ areâ ſectoris elliptici BEG, quæratur recta GF, quæ à puncto G ad axem normaliter demittitur: eſto BC $= q$; DC $= r$; EB $= t$; ac duplum areæ BEG $= z$.

Et erit GF $= \frac{z}{t} - \frac{q}{6rrt^4}z^3 + \frac{10qq-9qt}{120r^4t^7}z^5 - \frac{280q^3+504qq-225qtt}{5040r^6t^{10}}z^7 +$ &c. Sic itaque aſtronomicum illud Kepleri problema reſolvi poteſt.

5. In eâdem ellipſi, ſi ſtatuatur CD $= r$, $\frac{CB^2}{DC} = c$, & CF $= x$: erit [arcus] ellipticus DG $= x + \frac{1}{6cc}x^3 + \frac{1}{10rc^3}x^5 + \frac{1}{14rrc^4}x^7 + \frac{1}{18r^3c^5}x^9 + \frac{1}{22r^4c^6}x^{11} +$ &c.

$$
\begin{array}{llll}
 & -\frac{1}{40c^4} & -\frac{1}{28rc^5} & -\frac{1}{24rrc^6} \\
 & +\frac{1}{112c^6} & +\frac{1}{48rc^7} & +\frac{3}{88rrc^8} \\
 & & -\frac{5}{1152c^8} & -\frac{5}{352rc^9} \\
 & & & +\frac{7}{2816c^{10}}
\end{array}
$$

Hîc numerales coefficientes ſupremorum terminorum ($\frac{1}{6}$, $\frac{1}{10}$, $\frac{1}{14}$, &c.) ſunt in muſicâ progreſſione: et numerales coefficientes omnium inferiorum, in unâquâque columnâ, prodeunt multiplicando continuò numeralem coefficientem ſupremi termini per terminos hujus progreſſionis, $\frac{\frac{1}{2}n-1}{2}$, $\frac{\frac{3}{4}n-3}{4}$, $\frac{\frac{5}{4}n-5}{6}$, $\frac{\frac{7}{5}n-7}{8}$, $\frac{\frac{9}{6}n-9}{10}$, &c. Ubi n ſignificat numerum dimenſionum ipſius c, in denominatore iſtius ſupremi termini. *E. g.* Ut terminorum infra $\frac{1}{22r^4c^6}$ numerales coefficientes inveniantur; pono $n = 6$, ducoque $\frac{1}{2\,2}$, numeralem coefficientem ipſius $\frac{1}{22r^4c^6}$, in $\frac{\frac{1}{2}n-1}{2}$, hoc eſt in 1; & prodit $\frac{1}{2\,2\,8}$, numeralis coefficiens termini proximè inferioris. Dein duco hunc $\frac{1}{2\,2\,8}$ in $\frac{\frac{3}{4}n-3}{4}$, ſive in $\frac{n-3}{4}$, hoc eſt, in $\frac{3}{4}$: & prodit $\frac{3}{8\,8\,8}$, numeralis coefficiens tertii termini in iſtâ columnâ. Atque iſta $\frac{3}{8\,8\,8} \times \frac{\frac{5}{4}n-5}{6}$ facit $\frac{5}{3\,5\,2\,2}$, numeralem coefficientem quarti termini. Et $\frac{5}{3\,5\,2\,2} \times \frac{\frac{7}{5}n-7}{8}$ facit $\frac{7}{2\,8\,1\,2\,6}$, numeralem coefficientem infimi termini. Idem in aliis, ad infinitum uſque, columnis præſtari poteſt: adeoque valor ipſius DG, per hanc regulam, pro lubitu produci.

Adhæc, ſi BF dicatur x, ſitque r latus rectum ellipſeos, & $e = \frac{r}{AB}$; erit arcus ellipticus BG $=$

$\sqrt{rx}$

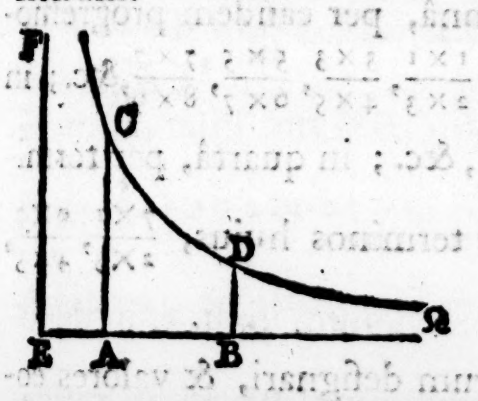
$$\sqrt{rx}\ \text{in}\ \frac{1+\frac{2}{3}e-\frac{3}{3r}}{}\Big\}x\ \frac{-2+3e-\frac{5}{8}ee}{5rr}\Big\}x^2\ \frac{+4-9e+\frac{23}{4}e^2-\frac{7}{16}e^3}{7r^3}\Big\}x^3\ \frac{-10+30e-\frac{123}{4}e^2+\frac{91}{8}e^3-\frac{45}{128}e^4}{9r^4}\Big\}x^4+\&c.$$

Quare, ſi ambitus totius ellipſeos deſideretur: biſeca CB in F, & quære arcum DG per prius theorema; & arcum BG, per poſterius.

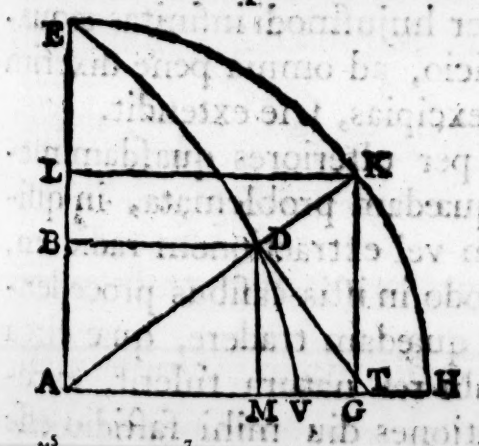
6. Si vice verſâ, ex dato arcu elliptico, DG, quæratur ſinus ejus CF; tum dicto $CD = r$, $\frac{CB^2}{CD} = c$, & arcu illo $DG = z$; erit

$$CF = z - \frac{1}{6cc}z^3 - \frac{1}{10rc^3}z^5 - \frac{1}{14rrc}z^7 - \&c.$$
$$+\frac{13}{120c^4} \quad +\frac{71}{420rc^5}$$
$$-\frac{493}{5040c^6}$$

Quæ autem de ellipſi dicta ſunt, omnia facilè accommodantur ad hyperbolam; mutatis tantùm ſignis ipſorum c & e, ubi ſunt imparium dimenſionum.



7. Præterea, ſi ſit CD hyperbola, cujus aſymptoti, EB, EF, rectum angulum, BEF, conſtituant; & ad EB erigantur utcunque perpendicula, AC, BD, occurrentia hyperbolæ in C & D: & EA dicatur a; AC, b; & area CADB, z; erit $AB = \frac{z}{b}+\frac{zz}{2abb}+\frac{z^3}{6aab^3}+\frac{z^4}{24a^3b^4}+\frac{z^5}{120a^4b^5}+\&c.$ Ubi coefficientium denominatores prodeunt, multiplicando terminos hujus arithmeticæ progreſſionis 1, 2, 3, 4, 5, &c. in ſe continuò. Et hinc ex logarithmo dato poteſt numerus ei competens inveniri.



8. Eſto VDE quadratrix, cujus vertex eſt V; exiſtente A centro, & AE ſemidiametro circuli ad quem aptatur, & angulo VAE recto: demiſſoque ad AE perpendiculo quovis, DB, & actâ quadratricis tangente, DT, occurrente axi ejus, AV, in T: dic $AV = a$, & $AB = x$; eritque $DB = a - \frac{xx}{3a} - \frac{x^4}{45a^3} - \frac{2x^6}{945a^5} - \&c.$ Et $VT = \frac{xx}{3a} + \frac{x^4}{15a^3} + \frac{2x^6}{189a^5} + \&c.$ Et area $AVDB = ax - \frac{x^3}{9a} - \frac{x^5}{225a^3} - \frac{2x^7}{6615a^5} - \&c.$ Et arcus $VD = x + \frac{2x^3}{27aa} + \frac{14x^5}{2025a^4} + \frac{604x^7}{893025a^6} + \&c.$ Unde viciſſim, ex dato BD, vel VT, aut areâ AVDB, arcuve VD, per reſolutionem affectarum æquationum, erui poteſt x, ſeu AB.

9. Eſto denique AEB ſphæroides, revolutione ellipſeos, AEB, circa axem AB genita, & ſecta planis quatuor; AB, per axem tranſeunte; DG, parallelo AB;

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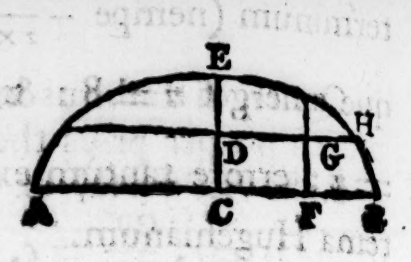
CDE, perpendiculariter biſecante axem; & FG, parallelo CE: ſitque recta CB $= a$, CE $= c$, CF $= x$, & FG $= y$: et ſphæroideos ſegmentum CDGF, dictis quatuor planis comprehenſum, erit

$$+2cxy - \frac{x}{3c}y^3 - \frac{x}{20c^3}y^5 - \frac{x}{56c^5}y^7 - \frac{5x}{576c^7}y^9 - \&c.$$
$$- \frac{cx^3}{3aa} - \frac{x^3}{18caa} - \frac{x^3}{40c^3aa} - \frac{5x^3}{336c^5aa} - \&c.$$
$$+ \frac{cx^5}{20a^4} - \frac{x^5}{40ca^4} - \frac{3x^5}{16cc^3a^4} - \&c.$$
$$- \frac{cx^7}{56a^6} - \frac{5x^7}{336ca^6} - \&c.$$
$$- \frac{5cx^9}{576a^8} - \&c.$$
$$- \&c.$$



Ubi numerales coefficientes ſupremorum terminorum $(2, +\frac{1}{3}, -\frac{1}{20}, -\frac{1}{56}, -\frac{5}{576}$, &c.) in infinitum producuntur, multiplicando primum coefficientem, 2, continuò per terminos hujus progreſſionis; $-\frac{1\times1}{2\times3}, \frac{1\times3}{4\times5}, \frac{3\times5}{6\times7}, \frac{5\times7}{8\times9}, \frac{7\times9}{10\times11}$, &c. Et numerales coefficientes terminorum, in unâquâque columnâ deſcendentium in infinitum, producuntur multiplicando continuò coefficientem ſupremi termini, in primâ columnâ, per eandem progreſſionem; in ſecundâ autem, per terminos hujus, $\frac{1\times1}{2\times3}, \frac{3\times3}{4\times5}, \frac{5\times5}{6\times7}, \frac{7\times7}{8\times9}$, &c.; in tertiâ, per terminos hujus, $\frac{3\times1}{2\times3}, \frac{5\times3}{4\times5}, \frac{7\times5}{6\times7}, \frac{9\times7}{8\times9}$, &c.; in quartâ, per terminos hujus, $\frac{5\times1}{2\times3}, \frac{7\times3}{4\times5}, \frac{9\times5}{6\times7}$, &c.; in quintâ, per terminos hujus, $\frac{7\times1}{2\times3}, \frac{9\times3}{4\times5}, \frac{11\times5}{6\times7}$, &c. Et ſic in infinitum.

Et eodem modo ſegmenta aliorum Solidorum deſignari, & valores eorum aliquando commodè, per ſeries quaſdam numerales, in infinitum produci poſſunt.

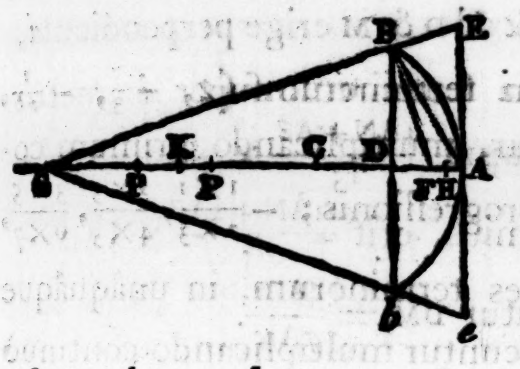
Ex his videre eſt, quantum fines Analyſeos per hujuſmodi infinitas æquationes ampliantur: quippe quæ, earum beneficio, ad omnia pene dixerim problemata, ſi numeralia Diophanti & ſimilia excipias, ſeſe extendit.

Non tamen omnino univerſalis evadit, niſi per ulteriores quaſdam methodos eliciendi ſeries infinitas. Sunt enim quædam problemata, in quibus non liceat ad ſeries infinitas per diviſionem vel extractionem radicum, ſimplicium affectarumve, pervenire. Sed quomodo in iſtis caſibus procedendum ſit, jam non vacat dicere; ut neque alia quædam tradere, quæ circa reductionem infinitarum ſerierum in finitas, ubi rei natura tulerit, excogitavi. Nam parciùs ſcribo, quòd hæ ſpeculationes diu mihi faſtidio eſſe cœperunt; adeo ut ab iiſdem jam per quinque fere annos abſtinuerim.

Unum tamen addam: quòd poſtquam problema aliquod ad infinitam æquationem deducitur, poſſint inde variæ approximationes, in uſum Mechanicæ, nullo ferè negotio formari; quæ, per alias methodos quæſitæ, multo labore temporiſque diſpendio conſtare ſolent.

Cujus rei exemplo eſſe poſſunt tractatus Hugenii aliorumque de quadratură

turâ circuli. Nam, ut ex datâ arcûs chordâ, A, & dimidii arcûs chordâ, B, arcum illum proximè aſſequaris: finge arcum illum eſſe z, & circuli radium r; juxtaque ſuperiora erit A (nempe duplum ſinûs dimidii z) $= z - \frac{z^3}{4\times 6rr} + \frac{z^5}{4\times 4\times 120r^4} -$ &c. Et B $= \frac{1}{2}z - \frac{z^3}{2\times 16\times 6rr} + \frac{z^5}{2\times 16\times 16\times 120r^4} -$ &c. Duc jam B in numerum fictitium n, & à producto auſer A, & reſidui ſecundum terminum (nempe $-\frac{nz^3}{2\times 16\times 6r} + \frac{z^5}{4\times 6rr}$), eo ut evaneſcat, pone $= 0$; indeque emerget $n = 8$: & erit $8B - A = 3z$ * $- \frac{3z^5}{64\times 120r^4} +$ &c. hoc eſt, $\frac{8B-A}{3} = z$; errore tantùm exiſtente $\frac{z^5}{7680r^4} -$ &c. in exceſſu (a). Quod eſt theorema Hugenianum. BURGUM PRIOR.



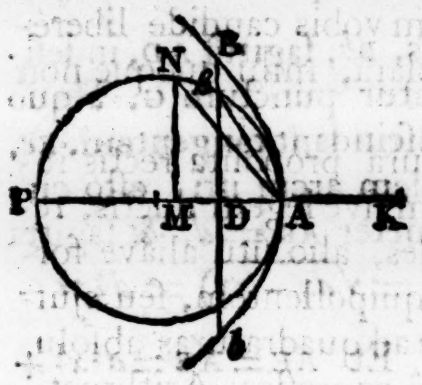
Inſuper, ſi in arcûs, Bb, ſagittâ AD, indefinitè productâ, quæratur punctum G, à quo actæ rectæ, GB, Gb, abſcindant tangentem, Ee, quàm proximè æqualem arcui iſti: eſto circuli centrum C, diameter AK $= d$, & ſagitta AD $= x$. Et erit DB $(= \sqrt{dx - xx}) = d^{\frac{1}{2}}x^{\frac{1}{2}} - \frac{x^{\frac{3}{2}}}{2d^{\frac{1}{2}}} - \frac{x^{\frac{5}{2}}}{8d^{\frac{3}{2}}} - \frac{x^{\frac{7}{2}}}{16d^{\frac{5}{2}}} -$ &c. Et AE $=$ AB $= d^{\frac{1}{2}}x^{\frac{1}{2}} + \frac{x^{\frac{3}{2}}}{6d^{\frac{1}{2}}} + \frac{3x^{\frac{5}{2}}}{40d^{\frac{3}{2}}} + \frac{5x^{\frac{7}{2}}}{112d^{\frac{5}{2}}} +$ &c. Et AE $-$ DB : AD : : AE : AG. Quare AG $= \frac{3}{2}d - \frac{1}{5}x - \frac{12xx}{175d} -$ vel $+$ &c. Finge ergo AG $= \frac{3}{2}d - \frac{1}{5}x$: & viciſſim erit DG $(\frac{1}{4}d - \frac{6}{5}x)$ AG : DB : : DA : AE $-$ DB. Quare AE $-$ DB $= \frac{2x^{\frac{3}{2}}}{3d^{\frac{1}{2}}} + \frac{x^{\frac{5}{2}}}{5d^{\frac{1}{2}}} + \frac{23x^{\frac{7}{2}}}{300d^{\frac{5}{2}}} +$ &c. Adde DB; & prodit AE $= d^{\frac{1}{2}}x^{\frac{1}{2}} + \frac{x^{\frac{3}{2}}}{6d^{\frac{1}{2}}} + \frac{3x^{\frac{5}{2}}}{40d^{\frac{3}{2}}} - \frac{17x^{\frac{7}{2}}}{1200d^{\frac{5}{2}}} +$ &c. Hoc aufer de valore ipſius AE ſuprà habito, & reſtabit error $\frac{16x^{\frac{7}{2}}}{525d^{\frac{5}{2}}} +$ vel $-$ &c. Quare in AG cape AH quintam partem DA, & KG $=$ HC, & actæ GBE, Gbe abſcindent tangentem, Ee, quàm proximè æqualem arcui BAb; errore tantùm exiſtente $\frac{2\times 16x^3}{525d^2}\sqrt{dx} +$ vel $-$ &c. multo minore ſcilicet quàm in theoremate Hugenii. Quòd ſi fiat 7AK : 3AK : : DH : n; & capiatur KG $=$ CH $- n$, erit error adhuc multo minor.

Atque ita, ſi circuli ſegmentum aliquod, BAb, per Mechanicam deſignandum eſſet: primò reducerem aream iſtam in infinitam ſeriem; puta hanc; B$bA = \frac{4}{3}d^{\frac{1}{2}}x^{\frac{3}{2}} - \frac{2x^{\frac{5}{2}}}{5d^{\frac{1}{2}}} - \frac{x^{\frac{7}{2}}}{14d^{\frac{3}{2}}} - \frac{x^{\frac{9}{2}}}{36d^{\frac{5}{2}}} -$ &c. Dein quærerem conſtructiones mechanicas, quibus hanc ſeriem proximè aſſequerer; cujuſmodi ſunt hæ: age rectam AB, & erit ſegmentum B$bA = \frac{2}{3}$AB $+$ BD $\times \frac{4}{5}$AD proximè: exiſtente ſcilicet errore tantùm $\frac{x^3}{70d^2}\sqrt{dx} +$ &c. in defectu (b): vel proximiùs, erit ſegmentum illud (biſecto AD in F, & actâ rectâ BF) $= \frac{4BF + AB}{15} \times 4$AD; exiſtente

(a) Vide Tom. I. p. 323, not. y. (b) Vide Tom. I. p. 324, not. ee.

istente errore solummodo $\frac{x^3}{5bb} \sqrt{dx}$ + &c (a); qui semper minor erit quàm $\frac{1}{1500}$ totius segmenti, etiamsi segmentum illud ad usque semicirculum augeatur.

Sic & in ellipsi BA*b* [vid. fig. precedent.] cujus vertex A, axis alteruter AK, & latus rectum AP; cape PG $= \frac{1}{2}$ AP + $\frac{19AK - 21AP}{10AK}$ × AD. In hyperbolâ vero, cape PG $= \frac{1}{2}$ AP + $\frac{19AK + 21AP}{10AK}$ × AD. Et acta recta GBE abscindet tangentem AE, quàm proximè æqualem arcui elliptico vel hyperbolico AB; dummodo arcus ille non sit nimis magnus.



Et pro areâ segmenti hyperbolici B*b*A; in DP cape MD $= \frac{5AD^2}{4AK}$; & ad D & M erige perpendicula, Dβ, MN, occurrentia semicirculo super diametro AP descripto; eritque $\frac{44AN + 4Aβ}{15}$ × 4AD = B*b*A proximè: vel proximiùs, erit $\frac{21AN + 4Aβ}{75}$ × 4AD = B*b*A; si modò capiatur DM $= \frac{5AD^2}{7AK}$.

N° LI. *Epistola D.* Leibnitii *ad D.* Oldenburgum, 27 Augusti 1676 *data, cum D.* Newtono *communicanda.*

Clarissimo Viro, D. Henrico Oldenburgio, Godefredus Guilielmus Leibnitius.

Literæ tuæ, die 26 *Julii* datæ, plura ac memorabiliora circa rem analyticam continent, quàm multa volumina spissa de his rebus edita. Quare tibi pariter ac Clarissimis Viris Newtono ac Collinio gratias ago, qui nos participes tot meditationum egregiarum esse voluistis.

Inventa Newtoni ejus ingenio digna sunt; quod ex opticis experimentis & tubo catadioptrico abunde eluxit.

Ejusque methodus inveniendi radices æquationum & areas figurarum, per series infinitas, prorsus differt à meâ: ut mirari libeat diversitatem itinerum, per quæ eodem pertingere licet.

Mercator figuras rationales, seu in quibus ordinatarum valor ex datis abscissis rationaliter exprimi potest (ut scilicet indeterminata quantitas in vinculum non ingrediatur) quadravit; & ad infinitas series reducere docuit, per divisiones: Newtonus autem, per radicum extractiones. Mea methodus corollarium est tantùm doctrinæ generalis de Transformationibus; cujus ope figura proposita quælibet, quâcunque æquatione explicabilis, transmutatur in aliam analyticam æquipollentem; talem ut, in ejus æquatione, ordinatæ dimensio non ascendat ultra cubum aut quadratum, aut etiam simplicem dignitatem, seu infimum gradum. Ita fiet ut quælibet

(a) Vide Tom. I. p. 325, not. ff.

* Hic modus transmutandi figuras curvilineas in alias ipsis æquales, ejusdem est generis cum transmutationibus Barrovianis & Gregorianis. Et conicæ sectiones hâc methodo semper ad series infinitas reduci possunt, per divisiones. Generalis tamen non est. Nam si Curva sit secundi generis,

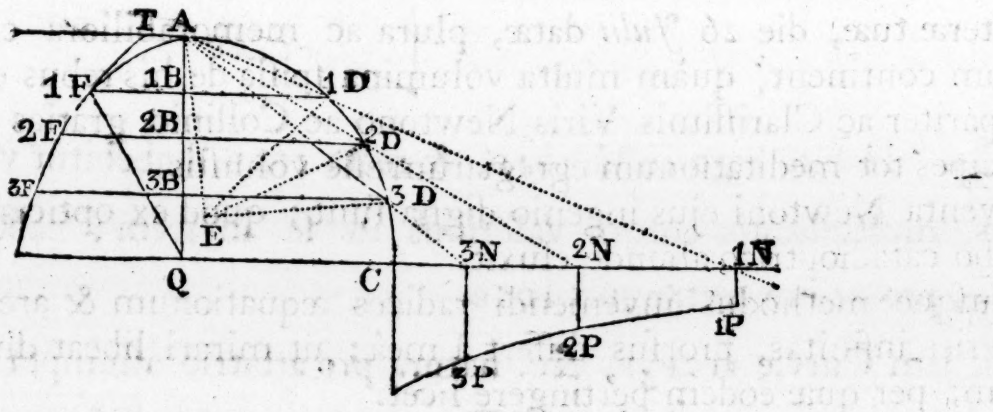
bet figura, vel per extractionem radicis cubicæ vel quadraticæ, Newtoni more; vel etiam, methodo Mercatoris, per ſimplicem diviſionem; ad ſeries infinitas reduci queat.

Ego verò ex his tranſmutationibus ſimpliciſſimam ad rem præſentem delegi. Per quam ſcilicet * unaquæque figura transformatur in aliam æquipollentem rationalem; in cujus æquatione, ordinata in nullam prorſus aſcendit poteſtatem: ac proinde ſolâ Mercatoris diviſione per infinitam ſeriem exprimi poteſt.

Ipſa porro * generalis tranſmutationum methodus mihi inter potiſſima analyſeos cenſenda videtur. Neque enim tantùm ad ſeries infinitas, & ad approximationes; ſed & ad ſolutiones geometricas, aliaque innumera vix alioqui tractabilia, inſervit. Ejus verò fundamentum vobis candidè liberéque ſcribo; perſuaſus quæ apud vos habentur præclara, mihi quoque non denegatum iri.

Transformationis fundamentum hoc eſt: ut figura propoſita rectis innumeris utcunque, modo ſecundum aliquam regulam ſive legem ductis, reſolvatur in partes; quæ partes, aut aliæ ipſis æquales, alio ſitu aliâve formâ reconjunctæ, aliam componant figuram priori æquipollentem, ſeu ejuſdem areæ, etſi aliâ longè figurâ conſtantem. Unde ad quadraturas abſolutas, vel hypotheticas, Geometricas, vel ſerie infinitâ expreſſas, Arithmeticas, jamjam multis modis perveniri poteſt.

Ut intelligatur; ſit figura AQCDA. Ea, ductis rectis BD parallelis, reſolvi poteſt in trapezia 1B2D, 2B3D, &c. Sed, ductis rectis convergentibus ED, reſolvi poteſt in triangula E1D2D, E2D3D, &c.



Si jam alia ſit Curva A1F2F3F; cujus trapezia, 1B2F, 2B3F, ſint triangulis E1D2D, E2D3D, ordine reſpondentibus æqualia; tota figura AE3D2D1DA, toti figuræ A1F2F3F3BA erit æqualis.

Quinetiam trapezia trapeziis conferendo, fieri poteſt ut 1N2P, vel quod eodem redit, rectangulum 1N2P, ſit æquale trapezio reſpondenti 1B2D, ſive rectangulo 1B2D, tametſi recta 1N1P non ſit æqualis rectæ 1B1D; modò ſit 1N2N ad 1B2B ut 1B1D ad 1N1P; quod infinitis modis fieri poteſt.

Quæ omnia talia ſunt, ut cuivis ſtatim ordine progredienti, ipſâ naturâ duce, in mentem veniant; contineantque indiviſibilium methodum generaliſſimè conceptam, nec, quod ſciam, hactenus ſatis univerſaliter explica-

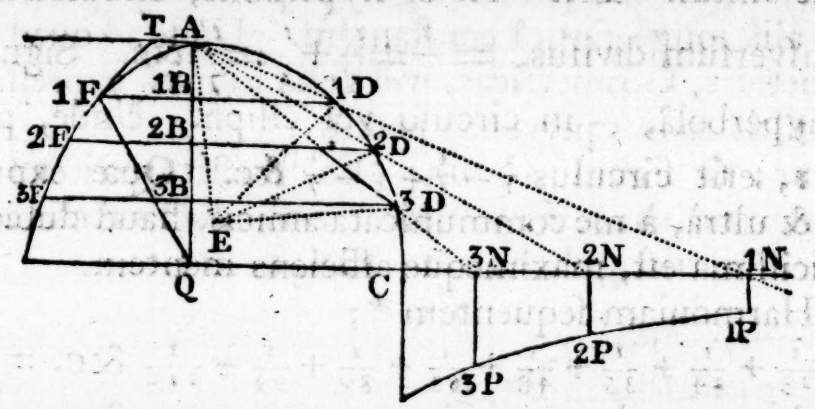
* ris, incidetur in æquationem quadraticam; ſi tertii generis, in cubicam; ſi quarti, in quadrato-quadraticam; ſi quinti, in quadrato-cubicam, &c. præterquam in caſibus quibuſdam valdè particularibus. Per extractiones verò radicum problemata faciliùs ſolvuntur, abſque tranſmutationibus.

tam.

tam. Non tantùm enim parallelæ & convergentes, ſed & aliæ quæcunque certâ lege ductæ, rectæ vel curvæ, adhiberi poſſunt ad reſolutionem. Quanta autem, & quàm abſtruſa, hinc duci poſſint, judicabit, qui methodi univerſalitatem animo erit complexus. Certum enim eſt omnes quadraturas, hactenus notas, abſolutas vel hypotheticas, nonniſi exigua ejus ſpecimina eſſe.

Sed nunc quidem ſuffecerit applicationem oſtendere ad id, de quo agitur; ſeries ſcilicet infinitas, & modum transformandi figuram datam in aliam æquipollentem rationalem, Mercatoris methodo tractandam.

AQCA ſit quadrans circuli: radius AQ $= r$: abſciſſa AIB $= x$: ordinata IBID $= y$; æquatio pro circulo $2rx - x^2 = y^2$. Ducatur recta AID; producaturque, donec ipſi QC etiam productæ occurrat in IN: et QIN vocetur z. Et* erit AIB, ſeu x, $= \frac{2r^3}{r^2+z^2}$: et IBID, ſive y, $= \frac{2zr^2}{r^2+z^2}$. Eodem modo, ductâ A2D2N; ſi Q3N $= z - \beta$, poſita ſcilicet I N2N $= \beta$: erit A2B $= \frac{2r^3}{r^2+z^2-2z\beta+\beta^2}$; & A2B − AIB, ſive recta IB2B, erit $\frac{2r^3}{r^2+z^2-2z\beta+\beta^2} - \frac{2r^3}{r^2+z^2}$. Sive, poſitâ β infinitè parvâ, poſt deſtructiones & diviſiones, erit IB2B $=$



$\frac{4r^3z\beta}{r^2+z^2}$. Habitâ ergo rectâ IBID, & recta IB2B, habebitur valor rectanguli ID2B, multiplicatis eorum valoribus in ſe invicem; habebitur inquam $\frac{8r^3z^2\beta}{r^2+z^2}$, pro valore rectanguli ID2B.

Sit jam Curvæ IP2P3P, &c. natura pro arbitrio aſſumpta talis, ut ordinata ejus INIP, ex datâ abſciſſâ QIN ſive z, ſit $\frac{8r^3z^3}{r^2+z^2}$. Ideo quoniam IN2N $= \beta$, erit rectangulum IP2V, etiam $\frac{8r^3z^2\beta}{r^2+z^2}$; ac proinde æquale rectangulo ID2B: & ſpatium IPIN3N3P2P1P æquale ſpatio circulari reſpondenti IDIB3B3D2DID. Eſt autem quælibet ordinata NP rationalis, ex datâ abſciſſâ QN; quia, poſitâ QN $= z$, ordinata NP eſt $\frac{8r^3z^3}{r^2+z^2}$, ſive $\frac{8r^3z^2}{r^2+3r^2z^2+z^4}$. Ergo ipſa per infinitam ſeriem integrorum exprimi poteſt, dividendo. Et ſpatium talibus ordinatis comprehenſum, æquipollens circulari, infinitâ ſerie numerorum rationalium, methodo Mercatoris qua-

* N. B. Leibnitius hanc methodum vulgari more prolixiùs hic exponit, quam analyſis ejus nova paucis exhibere potuiſſet; ideoque analyſin illam novam nondum invenerat.

drari

drari poteſt. Quod cum facillimum ſit facere, hîc omitto. Neque enim elegantiæ ſuæ, ſed methodi generalis explicandæ cauſâ, hoc exemplum aſſumpſi.

Ita ſiquis loco circuli mihi dediſſet Curvam, in quâ ordinata aſcendiſſet ad gradum cubicum; potuiſſem eam reducere ad Curvam, in quâ ordinata non aſſurrexiſſet ultra quadratum, vel etiam ne quidem ad quadratum.

Itaque ſemper, ſive extractionibus radicum Newtonianis, gradûs cujuſlibet dati, vel diviſionibus Mercatoris, poterit cujuſlibet figuræ ſpatium inveniri, interventu alterius æquipollentis. Multum autem ad ſimplicitatem intereſt, quid eligas.

Omnium verò poſſibilium circuli, & ſectoris conici centrum habentis cujuſlibet, per ſeries infinitas quadraturarum, ſimpliciſſimam hanc eſſe dicere auſim, quam nunc ſubjicio.

Sit QAIF, [vid. fig. præcedent.] ſector, duabus rectis in centro Q con- N° LII.
currentibus, & curvâ conicâ AIF, ad verticem A ſive axis extremum perveniente, comprehenſus. Tangenti verticis, AT, occurrat tangens IFT. Ipſum AT vocemus t; & rectangulum ſub ſemi-latere recto in ſemi-latus tranſverſum ſit unitas. Erit * ſector hyperbolæ, circuli vel ellipſeos, per ſemi-latus tranſverſum diviſus, $= \frac{t}{1} \pm \frac{t^3}{3} + \frac{t^5}{5} \pm \frac{t^7}{7}$ &c. Signo ambiguo $\pm$ valente + in hyperbolâ, − in circulo vel ellipſi. Unde, poſito quadrato circumſcripto 1, erit circulus $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7}$ &c. Quæ expreſſio, jam triennio abhinc, & ultrà, à me communicata amicis, haud dubiè omnium poſſibilium ſimpliciſſima eſt, maximeque afficiens mentem.

Unde duco Harmoniam ſequentem *;

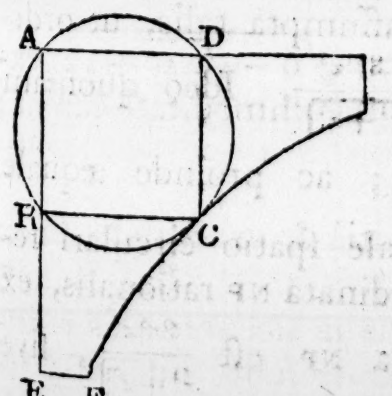
$$\frac{1}{3} + \frac{1}{8} + \frac{1}{15} + \frac{1}{24} + \frac{1}{35} + \frac{1}{48} + \frac{1}{63} + \frac{1}{80} + \frac{1}{99} + \frac{1}{120} \text{ &c.} = \frac{3}{4}$$

$$\frac{1}{3} \qquad \frac{1}{15} \qquad \frac{1}{35} \qquad \frac{1}{63} \qquad \frac{1}{99} \qquad \text{&c.} = \frac{2}{4}$$

$$\frac{1}{8} \qquad \frac{1}{24} \qquad \frac{1}{48} \qquad \frac{1}{80} \qquad \frac{1}{120} \text{ &c.} = \frac{1}{4}$$

$$\frac{1}{3} \qquad \frac{1}{35} \qquad \frac{1}{99} \qquad \text{&c.}$$

$$\frac{1}{8} \qquad \frac{1}{48} \qquad \frac{1}{120} \text{ &c.}$$



Ubi $\frac{1}{3} + \frac{1}{35} + \frac{1}{99}$ &c. exprimit aream circuli ABCD; & $\frac{1}{8} + \frac{1}{48} + \frac{1}{120}$ &c. aream hyperbolæ æquilateræ BCEF, cùm ſit BC dupla ipſius EF, & quadratum inſcriptum $= \frac{1}{4}$. Numeri 3, 8, 15, 24, &c. ſunt quadrati unitate minuti.

Viciſſim, † ex ſeriebus regreſſuum, pro hyperbolâ hanc inveni. Si ſit numerus aliquis unitate minor $1 - m$, ejuſque logarithmus hyperbolicus l: erit $m = \frac{l}{1} - \frac{l^2}{1 \times 2} + \frac{l^3}{1 \times 2 \times 3} - \frac{l^4}{1 \times 2 \times 3 \times 4}$ &c. Si numerus ſit major unitate, ut $1 + n$; tunc pro eo inveniendo mihi etiam ‡ prodiit regula, quæ in Newtoni epiſtolâ expreſſa eſt; ſcilicet erit $n = \frac{l}{1} +$

* Vid. pag. 506, lin. 20, & pag. 519, lin. 3.
† Vide Acta Lipſica *Feb.* 1682.
‡ Vid. not. pag. 528.

$\frac{l^2}{1\times2}+\frac{l^3}{1\times2\times3}+\frac{l^4}{1\times2\times3\times4}$ &c. Prior tamen celeriùs appropinquat. Ideoque efficio, ut eâ poſſim uti, etiam cùm major eſt unitate numerus $1+n$. Nam idem eſt logarithmus pro $1+n$ & pro $\frac{1}{1+n}$. Unde, ſi $1+n$ ſit major unitate, erit $\frac{1}{1+n}$ minor unitate. Fiat ergo $1+m=\frac{1}{1+n}$, ac inventâ m, habebitur & $1+n$ numerus quæſitus.

Quod regreſſum ex arcubus attinet *, incideram ego directè in regulam, quæ ex dato arcu ſinum complementi exhibet. Nempe, ſinus complementi $=1-\frac{a^2}{1\times2}+\frac{a^4}{1\times2\times3\times4}$ &c. Sed poſtea quoque deprehendi, ex eâ illam nobis communicatam, pro inveniendo ſinu recto, qui eſt $\frac{a}{1}+\frac{a^3}{1\times2\times3}+\frac{a^5}{1\times2\times3\times4\times5}$ &c. poſſe demonſtrari.

Quod tribus verbis ſic fit. Summa ſinuum complementi ad arcum, ſeu omnium $1-\frac{a^2}{1\times2}+\frac{a^4}{1\times2\times3\times4}$ &c. eſt $\frac{a}{1}-\frac{a^3}{1\times2\times3}+\frac{a^5}{1\times2\times3\times4\times5}$ &c. Porro, ſumma ſinuum complementi ad arcum, ſeu arcui in locis debitis inſiſtentium, æquatur ſinui recto ducto in radium, ut notum eſt geometris; id eſt, æquatur ipſi ſinui recto; quia radius hic eſt unitas. Ergo ſinus rectus $\frac{a}{1}-\frac{a^3}{1\times2\times3}+\frac{a^5}{1\times2\times3\times4\times5}$, &c. Hinc etiam, ex dato arcu & radio, ſine ullâ prorſus aliorum notitiâ, haberi poteſt area ſegmenti circularis duplicati: quæ eſt $\frac{a^3}{1\times2\times3}-\frac{a^5}{1\times2\times3\times4\times5}+\frac{a^7}{1\times2\times3\times4\times5\times6\times7}$ &c. Unde optimè ſegmentorum tabula ad gradus & minuta, &c. calculabitur.

Pro trigonometricis autem operationibus, percommoda mihi videtur hæc expreſſio: ut ſinus complementi, c, ponatur $=1-\frac{a^2}{1\times2}+\frac{a^4}{1\times2\times3\times4}$; quoniam ſola, memoriâ retenta, omnibus caſibus & operationibus, directis ſcilicet ſimul & reciprocis, ſufficit; quod ideo fit, quoniam æquatio $c=1-\frac{a^2}{2}+\frac{a^4}{24}$ eſt plana. Unde ſi viciſſim quæras arcum ex ſinu complementi, radix extrahi poteſt; adeoque fiet arcus $a=\sqrt{6-\sqrt{24c+12}}$ exactè ſatis ad uſum eorum, qui in itineribus tabularum commoditate carent; quia error æquationis non eſt $\frac{a^6}{720}$.

Innumera alia poſſent dici, quæ his fortaſſe elegantiâ & exactitudine non cederent. Sed ego ita ſum comparatus, ut plerumque, methodis generalibus detectis, rem in poteſtate habere contentus, reliqua libenter aliis relinquam. Neque enim iſta omnia magnopère æſtimanda ſunt, niſi quòd artem inveniendi perficiunt, mentemque excolunt. Si quæ obſcuriora videbuntur, ea libenter elucidabo: et illud quoque explicabo, quomodo hâc methodo

* N. B. Methodum perveniendi ad has ſeries Leibnitius à Newtono jam modo acceperat, idque ex ipſius rogatu. Imo ſeries ipſas à Newtono, unà cum methodo perveniendi ad eaſdem, jam modo

methodo æquationum quoque utcunque affectarum radices per infinitam ſeriem dari poſſint, ſine ullâ extractione; quod mirum fortaſſe videbitur.

Sed deſideraverim, ut Clariſſimus Newtonus nonnulla quoque ampliùs explicet; ut originem theorematis, quod initio ponit: item, modum quo quantitates p, q, r, in ſuis operationibus invenit: ac denique, quomodo in methodo regreſſuum ſe gerat; ut cùm ex logarithmo quærit numerum. Neque enim explicat, quomodo id ex methodo ſuâ derivetur.

Nondum mihi licuit, ejus literas, quâ merentur diligentiâ, legere: quoniam tibi è veſtigio reſpondere volui. Unde non ſatis nunc quidem affirmare auſim, an nonnulla eorum quæ ſuppreſſit, ex ſolâ earum lectione conſequi poſſum. Sed optandum tamen foret, ipſum ea potius ſupplere Newtonum: quia credibile eſt, non poſſe eum ſcribere, quin aliquid ſemper præclari nos doceat, ut apparet, egregiarum meditationum plenus.

Ad alia tuarum literarum venio; quæ doctiſſimus Collinius communi- N° LII
care gravatus non eſt. Vellem adjeciſſet appropinquationis Gregorianæ linearis demonſtrationem. Credo tamen aliam haberi ſimpliciorem, etiam in infinitum euntem; quæ fiat ſine ullâ biſectione anguli; imo ſine ſuppoſitâ circuli conſtructione; ſolo rectarum ductu.

Vellem Gregoriana omnia conſervari. Fuit enim his certè ſtudiis promovendis aptiſſimus: cæterum ejus demonſtrationi editæ, de impoſſibilitate quadraturæ abſolutæ circuli & hyperbolæ, multa haud dubiè deſunt.

De æquationum radicibus ſurdis generalibus inveniendis; ſive, quod idem eſt, tollendis æquationum poteſtatibus intermediis, multa & ego meditatus ſum; & jam vere anni ſuperioris, ſpecimina Hugenio communicaveram regularum Cardanicis ſimilium. Seriem enim habebam ejuſmodi regularum in infinitum euntem; in quibus & Cardanica continebatur. Sed ultra gradum cubicum non erant generales. Perſpexi tamen inde veram methodum progrediendi longiùs. Quamquam multis adhuc opus ſit artibus, quas excutiendas libenter ingenioſiſſimo Tſchurnhauſio relinquo; qui hîc ad eadem quæ ego habebam ſpecimina, imo & alia præterea, etiam de ſuo pervenit.

Ex iis quæ Collinius ait de Gregorianâ methodo, difficile non fuit nobis certo divinare, in quo conſiſtat ejus ſubſtantia.

Imaginariarum quantitatum, in realium radicum expreſſiones ingredientium, ſublationem fruſtra putem ſperari, imo quæri. Neque enim illæ ullo modo vel calculis vel conſtructionibus obſunt: et veræ realeſque ſunt quantitates, ſi inter ſe conjunguntur, ob deſtructiones virtuales. Quod multis elegantibus exemplis, & argumentis, deprehendi.

Exempli gratiâ, $\sqrt{1+\sqrt{-3}}+\sqrt{1-\sqrt{-3}}=\sqrt{6}$. Tametſi enim neque ex binomio $\sqrt{1+\sqrt{-3}}$, neque ex binomio $\sqrt{1-\sqrt{-3}}$ radix extrahetur; nec proinde ſic deſtruetur imaginaria $\sqrt{-3}$: ſupponenda tamen eſt deſtructa eſſe virtualiter; quod actu appareret, ſi fieri poſſet extractio. Aliâ tamen viâ hæc ſumma reperitur eſſe $\sqrt{6}$. Unde in cubicis binomiis, ubi realitas ejuſmodi formularum, tunc cùm extractio ex ſingulis binomiis fieri

modo acceperat, & pro hyperbolâ ſignum tantùm mutavit; pro circulo, ſinum verſum à Newtono acceptum ſubduxit à radio, ut haberet ſinum complementi.

nequit, ad oculum oſtendi non poteſt, mente tamen intelligitur. Quare fruſtra Carteſius aliique expreſſiones Cardanicas pro particularibus habuere. Siquis poſſet invenire quadraturam circuli & ejus partium, ex datâ hyperbolæ & ejus partium quadraturâ, is poſſet eas tollere; modò in ipſam quadraturam imaginariæ illæ rurſus ingrediantur.

Cæterum ex illis, quas habeo, meditationibus circa radices æquationum irrationales, neceſſario ſequitur res ſatis paradoxa: ſcilicet omnes æquationes gradûs octavi, noni, decimi, poſſe ad gradum ſeptimum reduci. Itaque & omnia problemata, ad decimum gradum uſque occurrentia, poſſunt ad ſeptimum deprimi.

Horribiles calculi ſubeundi erunt illi, qui in hoc argumentum velut per vim irrumpet; ſed facilimi, ipſi qui antè meditabitur: cùm, ut prævideo, ipſa natura rei ducat ad compendia quædam, per quæ ſpes eſt calculi magnam partem abſcindi; remque elegantibus artificiis, ingenii potius vi quàm calculi labore, tranſigi poſſe.

Sed ſiquis laborem non ſubterfugeret, eum docere poſſum methodum analyticam generalem infallibilem, per quam omnium æquationum radices generales invenire liceret.

Verùm meliora illis proponerem agenda, qui calculo delectarentur. Conſilium enim habeo tabularum analyticarum, quæ non minoris futuræ eſſent uſûs in Analyſi, quàm tabulæ ſinuum in Geometriâ Practicâ; imo, arbitror, qui paulum in iis calculandis verſatus ſit, eum progreſſiones reperturum in infinitum, quarum ope magna tabulæ pars ſine labore continuari poſſit. Nihil eſt, quod nôrim, in totâ analyſi momenti majoris. Nam in his tabulis pleraque problemata ſtatim ſoluta haberentur, aut levi operâ poſſint inde deduci.

Pendet negotium ex re longè majore; arte ſcilicet combinatoriâ generali ac verâ. Cujus vim ac poteſtatem neſcio, an quiſquam hactenus ſit conſequutus. Ea verò nihil differt ab analyſi illâ ſupremâ, ad cujus intima, quantum judicare poſſum, Carteſius non pervenit. Eſt enim, ad eam conſtituendam, opus alphabeto cogitationum humanarum. Et ad inventionem ejus alphabeti, opus eſt analyſi axiomatum. Sed non miror iſta nemini ſatis conſiderata: quia plerumque facilia negligimus; & multa, quæ clara videntur, aſſumimus. Quam quamdiu faciemus, nunquam ad illud perveniemus, quod mihi videtur in rebus intellectualibus ſummum; nec genus calculi, etiam non-mathematicis accommodati, obtinebimus.

Optârim Cl. Pellium generalia ſua meditata, & illud ſpeciatim, quod memoras, Cribrum Eratoſthenis non ſupprimere. Nam etſi omnia fortè, quæ deſtinârat, non abſolverit; meditata tamen ipſa, & conſilia, egregiorum virorum non perire, publici intereſt. Utilia quoque futura ſunt, quæ de ſinuum tabulâ ad æquationes accommodandâ habet. Item de limitibus, & radicibus.

Quod dicere videmini, pleraſque difficultates, exceptis problematibus Diophantæis, ad ſeries infinitas reduci; id mihi non videtur. Sunt enim multa uſque adeo mira & implexa, ut neque ab æquationibus pendeant, neque

neque ex quadraturis. Qualia ſunt, ex multis aliis, problemata * methodi tangentium inverſæ; quæ etiam Carteſius in poteſtate non eſſe faſſus eſt.

In Tomo III Epiſtolarum, una habetur ad Beaunium; in quâ, ad propoſitas à Beaunio, Curvas quaſdam invenire conatur; quarum una eſt ludus naturæ: ut intervallum inter tangentem, ad [axem] directricem uſque productam, & ordinatim-applicatam ex Curvâ ad directricem, ſit ſemper idem; recta ſcilicet conſtans. Hanc Curvam nec Carteſius, nec Beaunius, nec quiſquam alius, quod ſciam, invenit. Ego verò quâ primum die, imo horâ, cæpi quærere, ſtatim certâ analyſi ſolvi. Fateor tamen nondum me, quicquid in hoc genere deſiderari poteſt conſecutum: quamquam maximi momenti eſſe ſciam. Ac de his quidem nunc ſatis.

Ego id agere conſtitui, ubi primùm otium nactus ero, ut rem omnem mechanicam reducam ad puram geometriam; problemataque circa elateria, & aquas, & pendula, & projecta, & Solidorum reſiſtentiam, & fractiones, &c. definiam. Quæ hactenus attigit nemo. Credo autem rem omnem nunc eſſe in poteſtate; ex quo circa regulas motuum mihi penitus perfectis demonſtrationibus ſatisfeci; neque quicquam amplius in eo genere deſidero. Tota autem res, quod mireris, pendet ex axiomate metaphyſico pulcherrimo; quod non minoris eſt momenti circa motum, quam hoc, *totum eſſe majus parte*, circa magnitudinem.

De centro-baricis quoque, ſingularem quendam aditum reperi ad novas ac plane à prioribus diverſas contemplationes, in Geometriâ pariter ac Mechanicâ magno uſui futuras. Hæc ubi (Deo volente) abſolvero, reliquum temporis, quod ſcilicet philoſophicis meditationibus deſtinare fas erit, naturæ indagationi debeo.

Tſchurnhauſius proximo tabellione ſcribet.

Excerpta ex epiſtolâ D. Ehrenfried de Tſchurnhauſe *ad D.* Oldenburgum, Pariſiis 1° Septemb. 1676 *datâ*; *cujus extat exemplar manu D.* Collinis *deſcriptum.* N° LIV.

Expectabam cum deſiderio reſponſum, cùm aliquot abhinc menſibus ad te literas meas tranſmiſeram; ſed nec ex modò datis colligere licet, has receptas fuiſſe. Interim admodum oblectatus fui, hiſce conſpectis quæ ad D. Leibnitium exarâſti; maximéque me tibi devinxiſti, quòd me participem volueris facere tam ingenioſarum inventionum, & promotionis geometriæ tam pulchræ quâm utilis. Statim curſim eas pervolvi, ut viderem, num forte inter haſce ſeries infinitas exiſteret † ea, quâ ingenioſiſſimus D. Leibnitius circulum, imo quamvis ſectionem conicam (centro in finitâ diſtantiâ gaudentem) quadravit; tali ratione, ut mihi perſuadeam ſimpliciorem viam, nec quoad linearum conſtructionem, nec numeralem expreſſio-

* Si æquationes differentiales D. Leibnitio jam innotuiſſent, haud dixiſſet, problemata methodi tangentium inverſæ ab æquationibus non pendere.

† Annon D. Tſchurnhauſe viderat excerpta ex Gregorii epiſtolis, cum D. Leibnitio communicata; ubi habetur ſeries Gregorii, quam Leibnitio hîc tribuit? Vide pag. 521.

nem, nunquam viſum iri; quique hiſce porro inſiſtens, generalem adinvenit methodum figuram quamvis datam in talem rationalem tranſmutandi, quæ per ſolum inventum (admodum præſtans meo judicio) D. Mercatoris, ad ſeriem infinitam poſſet reduci. Sed hâc de materiâ, cùm ipſe non ita pridem mentem ſuam declaravit, non opus eſt ut prolixior ſim. Verùm ut ad ſpecimina perquam ingenioſa D. Newtoni revertar; hæc non potuere non mihi placere; tam ob utilitatem, quâ ſe tam latè ad quarumvis quantitatum dimenſiones, ac alia difficilia enodanda in mathematicis extendunt, quàm ob deductionem harum, à fundamentis non minus generalibus, quàm ingenioſis, derivatam: non obſtante quod exiſtimem, ad quantitatem quamvis ad infinitam ſeriem æquipollentem reducendam, fundamenta adhuc dari & ſimpliciora & univerſaliora, quàm ſunt fractionum & irrationalium reductio ad tales ſeries, ope diviſionis aut extractionis; quæ mihi tale quid non niſi per accidens præſtare videntur: cùm hæc ſucceſſum quoque habeant, licet non adſint fractiones aut irrationales quantitates. Similia porro, quæ in hâc re præſtitit eximius ille geometra Gregorius, memoranda certè ſunt; & quidem optimè famæ ipſius conſulturi, qui ipſius relicta manuſcripta luci publicæ ut exponantur operam navabunt.

N° LV. *Epiſtola D.* Newtoni *poſterior, ad D.* Oldenburgum, Octob. 24, 1676 *data, cum D.* Leibnitio *communicanda.*

Vir Digniſſime,

QUANTA cum voluptate legi epiſtolas clariſſimorum virorum D. Leibnitii & D. Tſchurnhauſii, vix dixerim.

Perelegans ſane eſt Leibnitii methodus perveniendi ad ſeries convergentes: & ſatis oſtendiſſet ingenium authoris, etſi nihil aliud ſcripſiſſet. Sed quæ alibi per epiſtolam ſparſit, ſuo nomine digniſſima, efficiunt etiam, ut ab eo ſperemus maxima. Diverſitas modorum, quibus eodem tenditur, eo magis placuit, quòd mihi tres methodi perveniendi ad ejuſmodi ſeries innotuerant; adeo ut novam nobiſcum communicandam vix expectarem.

Unam è meis prius deſcripſi: jam addo aliam; illam ſcilicet quâ primùm incidi in has ſeries. Nam incidi in eas, antequam ſcirem diviſiones & extractiones radicum, quibus jam utor. Et hujus explicatione pandendum eſt fundamentum theorematis, ſub initio epiſtolæ prioris poſiti, quod D. Leibnitius à me deſiderat.

Sub initio ſtudiorum meorum mathematicorum, ubi incideram in * Opera celeberrimi Walliſii noſtri: conſiderando ſeries, quarum intercalatione ipſe exhibet aream circuli & hyperbolæ; utpote quòd in ſerie Curvarum, quarum baſis ſeu axis communis ſit x, & ordinatim applicatæ $\overline{1-xx}^{\frac{0}{2}}$. $\overline{1-xx}^{\frac{1}{2}}$. $\overline{1-xx}^{\frac{2}{2}}$. $\overline{1-xx}^{\frac{3}{2}}$. $\overline{1-xx}^{\frac{4}{2}}$. $\overline{1-xx}^{\frac{5}{2}}$. &c. ſi areæ alternarum, quæ ſunt x, $x-\frac{1}{3}x^3$, $x-\frac{2}{3}x^3+\frac{1}{5}x^5$, $x-\frac{3}{3}x^3+\frac{3}{5}x^5-\frac{1}{7}x^7$ &c. interpolari poſſent; ha-

* Vide D. Walliſii arithmeticam infinitorum, Prop. CXVIII, CXXI, ejuſque algebram, Cap. LXXXII.

beremus

beremus areas intermediarum, quarum prima, $\overline{1-xx}^{\frac{1}{2}}$, eſt circulus: ad has interpolandas notabam, quòd in omnibus, primus terminus eſſet x; quodque ſecundi termini, $\frac{0}{3}x^3$, $\frac{1}{3}x^3$, $\frac{2}{3}x^3$, $\frac{3}{3}x^3$, &c. eſſent in arithmeticâ progreſſione; & proinde quòd duo primi termini ſerierum intercalandarum deberent eſſe $x-\frac{\frac{1}{2}x^3}{3}$, $x-\frac{\frac{3}{2}x^3}{3}$, $x-\frac{\frac{1}{2}x^3}{3}$, &c. BURGUM POSTERIOR.

Ad reliquas intercalandas conſiderabam, quòd denominatores, 1, 3, 5, 7, &c. erant in arithmeticâ progreſſione; adeoque ſolæ numeratorum coefficientes numerales eſſent inveſtigandæ. Hæ autem in alternis datis areis erant figuræ poteſtatum numeri undenarii; nempe 11^0, 11^1, 11^2, 11^3, 11^4. Hoc eſt, primo 1; deinde 1, 1; tertio 1, 2, 1; quarto 1, 3, 3, 1; quinto 1, 4, 6, 4, 1, &c.

Quærebam itaque, quomodo in his ſeriebus, ex datis duabus primis figuris, reliquæ derivari poſſent. Et inveni, quòd poſitâ ſecundâ figurâ m, reliquæ producerentur per continuam multiplicationem terminorum hujus ſeriei, $\frac{m-0}{1}\times\frac{m-1}{2}\times\frac{m-2}{3}\times\frac{m-3}{4}\times\frac{m-4}{5}$ &c.

Exempli gratiâ. Sit terminus ſecundus $m=4$; & erit $4\times\frac{m-1}{2}$, hoc eſt 6, tertius terminus; & $6\times\frac{m-2}{3}$, hoc eſt 4, quartus; & $4\times\frac{m-3}{4}$, hoc eſt 1, quintus; & $1\times\frac{m-4}{5}$, hoc eſt 0, ſextus; quo ſeries in hoc caſu terminatur.

Hanc regulam itaque applicui ad ſeries interſerendas. Et cùm pro circulo ſecundus terminus eſſet $\frac{\frac{1}{2}x^3}{3}$, poſui $m=\frac{1}{2}$: & prodierunt termini $\frac{1}{2}\times\frac{\frac{1}{2}-1}{2}$, ſive $-\frac{1}{8}$; $-\frac{1}{8}\times\frac{\frac{1}{2}-2}{3}$ ſive $+\frac{1}{16}$; $+\frac{1}{16}\times\frac{\frac{1}{2}-3}{4}$, ſive $-\frac{5}{128}$; & ſic in infinitum. Unde cognovi, deſideratam aream ſegmenti circularis eſſe $x-\frac{\frac{1}{2}x^3}{3}-\frac{\frac{1}{8}x^5}{5}-\frac{\frac{1}{16}x^7}{7}-\frac{\frac{5}{128}x^9}{9}$ &c.

Et eâdem ratione prodierunt etiam interſerendæ areæ reliquarum Curvarum: ut & area hyperbolæ, & cæterarum alternarum in hâc ſerie; $\overline{1+xx}^{\frac{0}{2}}$, $\overline{1+xx}^{\frac{1}{2}}$, $\overline{1+xx}^{\frac{2}{2}}$, $\overline{1+xx}^{\frac{3}{2}}$, &c.

Et eadem eſt ratio intercalandi alias ſeries; idque per intervalla duorum pluriumve terminorum ſimul deficientium.

Hic fuit primus meus ingreſſus in has meditationes: qui è memoriâ ſane exciderat, niſi oculos in adverſaria quædam, ante paucas ſeptimanas, retuliſſem.

Ubi verò hæc didiceram; mox conſiderabam terminos $\overline{1-xx}^{\frac{0}{2}}$, $\overline{1-xx}^{\frac{2}{2}}$, $\overline{1-xx}^{\frac{4}{2}}$, $\overline{1-xx}^{\frac{5}{2}}$, &c. hoc eſt, 1, $1-xx$, $1-2xx+x^4$, $1-3xx+3x^4-x^6$, &c. eodem modo interpolari poſſe, ac areas ab ipſis generatas: & ad hoc nihil aliud requiri, quàm omiſſionem denominatorum 1, 3, 5, 7, &c. in terminis exprimentibus areas; hoc eſt, coefficientes terminorum quantitatis intercalandæ $\overline{1-xx}^{\frac{1}{2}}$, vel $\overline{1-xx}^{\frac{3}{2}}$, vel generaliter $\overline{1-xx}^{m}$, prodire per continuam

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Adeoque (exempli gratiâ) $\overline{1 - xx}|^{\frac{1}{2}}$, valeret $1 - \frac{1}{2}x^2 - \frac{1}{8}x^4 - \frac{1}{16}x^6$ &c. Et $\overline{1 - xx}|^{\frac{3}{2}}$ valeret $1 - \frac{3}{2}x^2 + \frac{3}{8}x^4 + \frac{1}{16}x^6$ &c. Et $\overline{1 - xx}|^{\frac{1}{3}}$ valeret $1 - \frac{1}{3}xx - \frac{1}{9}x^4 - \frac{5}{81}x^6$ &c.

Sic itaque innotuit mihi generalis reductio radicalium in infinitas ſeries, per regulam quam poſui initio epiſtolæ prioris, antequam ſcirem extractiones radicum.

Sed hâc cognitâ, non potuit altera me diu latere. Nam ut probarem has operationes, multiplicavi $1 - \frac{1}{2}x^2 - \frac{1}{8}x^4 - \frac{1}{16}x^6$ &c. in ſe; & factum eſt $1 - xx$, terminis reliquis in infinitum evaneſcentibus per continuationem ſeriei. Atque ita $1 - \frac{1}{3}xx - \frac{1}{9}x^4 - \frac{5}{81}x^6$ &c. bis in ſe ductum produxit $1 - xx$. Quod, ut certa fuerit harum concluſionum demonſtratio, ſic me manuduxit ad tentandum è converſo, num hæ ſeries, quas ſic conſtitit eſſe radices quantitatis $1 - xx$, non poſſent inde extrahi more arithmetico. Et res bene ſucceſſit. Operationis forma in quadraticis radicibus hæc erat.

$$
\begin{array}{l}
1 - xx \;(1 - \frac{1}{2}xx - \frac{1}{8}x^4 - \frac{1}{16}x^6 \text{ &c.} \\
\underline{1} \\
0 - xx \\
\quad \underline{- xx + \frac{1}{4}x^4} \\
\qquad - \frac{1}{4}x^4 \\
\qquad \underline{- \frac{1}{4}x^4 + \frac{1}{8}x^6 + \frac{1}{64}x^8} \\
\qquad\qquad - \frac{1}{8}x^6 - \frac{1}{64}x^8
\end{array}
$$

His perſpectis, neglexi penitus interpolationem ſerierum; & has operationes, tanquam fundamenta magis genuina, ſolummodo adhibui. Nec latuit reductio per diviſionem; res utique facilior.

Sed & reſolutionem affectarum æquationum mox aggreſſus ſum; eamque obtinui. Unde ſimul ordinatim applicatæ, ſegmenta axium, aliæque quælibet rectæ, ex areis Curvarum, vel arcubus, datis innotuere. Nam regreſſio ad hæc nihil indigebat præter reſolutionem æquationum, quibus areæ, vel arcus, ex datis rectis dabantur.

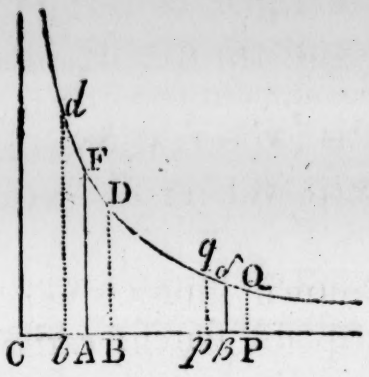
N° LVI. Eo tempore peſtis ingruens [*quæ contigit annis* 1665, 1666] coegit me hinc fugere, & alia cogitare. Addidi tamen ſubinde condituram quandam logarithmorum, ex areâ hyperbolæ, quam hîc ſubjungo.

Sit

* Mathematici priores invenerunt hoc theorema, quòd *ſumma terminorum progreſſionis geometricæ, in infinitum pergentis, eſt ad terminorum primum & maximum, ut hic terminus ad differentiam duorum terminorum primorum* (a). Idem demonſtratur arithmeticè, multiplicando extrema & media. Demonſtravit Walliſius, dividendo rectangulum ſub mediis per extremum ultimum. Vide Walliſii opus arithmeticum anno 1657 editum Cap. XXXIII. § 36. Per Walliſii diviſionem Mercator demonſtravit, & auxit quadraturam hyperbolæ, à D. Brounker priùs inventam. Et Gregorius

(a) Vide Tom. I. p. 261, not.

idem



Sit *d*FD hyperbola, cujus centrum C, vertex F, & quadratum interjectum CAFE = 1. In AC cape AB, A*b* hinc inde = $\frac{1}{10}$ ſeu 0.1 : et, erectis perpendiculis, BD, *bd*, ad hyperbolam terminatis, erit ſemi-ſumma ſpatiorum AD & A*d* = $0.1 + \frac{0.001}{3} + \frac{0.00001}{5} + \frac{0.0000001}{7}$ &c. & ſemi-differentia = $\frac{0.01}{2} + \frac{0.0001}{4} + \frac{0.000001}{6} + \frac{0.00000001}{8}$ &c. Quæ reductæ ſic ſe habent, BURGUM POSTERIOR.

0.1000000000000	0.0050000000000
3333333333	2500000000
20000000	1666666
142857	12500
1111	100
9	1
0.1003353477310	0.0050251679267

Horum ſumma 0.1053605156577 eſt A*d*; & differentia 0.0953101798043 eſt AD. Et eâdem ratione poſitis AB, A*b* hinc inde = 0.2, obtinebitur A*d* = 0.2231435513142; & AD = 0.1823215567939. Habitis ſic logarithmis hyperbolicis numerorum quatuor decimalium 0.8, 0.9, 1.1, & 1.2; cum ſit $\frac{1.2}{0.8} \times \frac{1.2}{0.9} = 2$; & 0.8 & 0.2, ſint minores unitate : adde logarithmos eorum ad duplum logarithmi 1.2, & habebis 0.6931471805597 logarithmum hyperbolicum numeri 2. Cujus triplo adde log. 0.8 (ſiquidem ſit $\frac{2 \times 2 \times 2}{0.8} = 10$), & habebis 2.3025850929933 logarithmum numeri 10: indeque per additionem ſimul prodeunt logarithmi numerorum 9 & 11: adeoque omnium primorum horum, 2, 3, 5, 11, logarithmi in promptu ſunt. Inſuper, ex ſolâ depreſſione numerorum ſuperioris computi per loca decimalia, & additione; obtinentur logarithmi decimalium 0.98, 0.99, 1.01, 1.02; ut & horum 0.998, 0.999, 1.001, 1.002. Et inde, per additionem & ſubductionem, prodeunt logarithmi primorum, 7, 13, 17, 37, &c. Qui unà cum ſuperioribus, per logarithmum numeri 10 diviſi, evadunt veri logarithmi in tabulam inſerendi. Sed hos poſtea propiùs obtinui (a).

Pudet dicere, ad quot figurarum loca has computationes, otioſus eo tempore, produxi. Nam tunc ſane nimis delectabar inventis hiſce. Sed ubi prodiit ingenioſa illa * Nicolai Mercatoris Logarithmotechnia (quem ſuppono ſua primum inveniſſe) cœpi ea minus curare; ſuſpicatus, vel eum nôſſe extractionem radicum, æquè ac diviſionem fractionum; vel alios ſaltem, diviſione patefactâ, inventuros reliqua, prius quàm ego ætatis eſſem maturæ ad-ſcribendum.

idem demonſtravit geometricè. Sed horum nemo methodum generalem quadrandi Curvas per diviſionem invenit. Mercator hoc nunquam profeſſus eſt. Gregorius ejuſmodi methodum, licet vir acutiſſimus & literis Collinii admonitus, vix tandem invenit. Newtonus invenit per interpolationem ſerierum, & poſtea diviſionibus & extractionibus radicum, ut notioribus, uſus eſt.

(a) Vide Geometriam Analyticam, Cap. IX § 36—48.

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Eo ipſo tamen tempore quo liber iſte prodiit, communicatum eſt per amicum D. Barrow, tunc Matheſeos profeſſorem Cantabrigiæ, cum D. Collinio *, compendium quoddam methodi harum ſerierum; in quo ſignificaveram areas & longitudines Curvarum omnium, & Solidorum ſuperficies & contenta, ex datis rectis, & vice verſâ, ex his datis rectas determinari poſſe: & methodum ibi indicatam illuſtraveram diverſis ſeriebus.

Subortâ deinde inter nos epiſtolari conſuetudine; D. Collinius, vir in rem mathematicam promovendam natus, non deſtitit ſuggerere, ut hæc publici juris facerem. Et ante annos quinque [1671], cùm ſuadentibus amicis conſilium ceperam edendi tractatum de refractione lucis, & coloribus, quem tunc in promptu habebam; cœpi de his ſeriebus iterum cogitare: & tractatum † de iis etiam conſcripſi, ut utrumque ſimul ederem.

Sed, ex occaſione teleſcopii catadioptrici, epiſtolâ ad te miſſâ, quâ breviter explicui conceptus meos de naturâ lucis; inopinatum quiddam effecit, ut mei intereſſe ſentirem, ad te feſtinanter ſcribere de impreſſione iſtius epiſtolæ. Et ſubortæ ſtatim, per diverſorum epiſtolas objectionibus aliiſque refertas, crebræ interpellationes me prorſus à conſilio deterruerunt; & effecerunt, ut me arguerem imprudentiæ, quòd umbram captando, eatenus perdideram quietem meam, rem prorſus ſubſtantialem.

Sub eo tempore Jacobus Gregorius, ex unicâ quâdam ſerie è meis, quam D. Collinius ad eum tranſmiſerat, poſt multam conſiderationem, ut ad Collinium reſcripſit, pervenit ad eandem methodum; & tractatum de eâ reliquit, quem ſperamus ab amicis ejus editum iri. Siquidem, pro ingenio quo pollebat, non potuit non adjicere de ſuo nova multa; quæ rei mathematicæ intereſt, ut non pereant.

Ipſe autem tractatum meum non penitus abſolveram, ubi deſtiti à propoſito; neque in hunc diem mens rediit ad reliqua adjicienda. Deerat quippe pars ea, quâ decreveram explicare modum ſolvendi problemata, quæ ad quadraturas reduci nequeunt; licet aliquid de fundamentis ejus poſuiſſem. Cæterùm in tractatu iſto, ſeries infinitæ non magnam partem obtinebant.

Alia haud pauca congeſſi; inter quæ erat methodus ducendi tangentes, quam ſolertiſſimus Sluſius ante annos duos treſve tecum communicavit; de quâ tu, ſuggerente Collinio, reſcripſiſti, eandem ‡ mihi etiam innotuiſſe. Diverſâ ratione in eam incidimus. Nam res non eget demonſtratione, prout ego operor. Habito meo fundamento, nemo potuit tangentes aliter ducere, niſi volens de rectâ viâ deviaret.

Quinetiam non hîc hæretur ad æquationes radicalibus, unam vel utramque indefinitam quantitatem involventibus, utcunque affectas; ſed abſque aliquâ

* Analyſin intelligit per æquationes infinitas ſuprà impreſſam, de quâ vid. pag. 502, 503.

† Hujus tractatûs meminit D. Collins in epiſtolis duabus ſuprà impreſſis, pag. 508. Et Newtonus in epiſt. ſupra impreſſa, pag. 510.

‡ Vide epiſtolam Newtoni ſuprà impreſſam, pag. 510.

§ Hoc eſt, *datâ æquatione quotcunque fluentes quantitates involvente, fluxiones invenire; & vice verſâ.* Prior pars problematis ſolvitur per regulam binomii, initio epiſtolæ ſuperioris Newtonianæ traditam, & initio hujus demonſtratam. Nam ſi terminus ſecundus binomii ſit momentum termini

aliquâ talium æquationum reductione, quæ opus plerumque redderet immenſum, tangens confeſtim ducitur. Et eodem modo ſe res habet in quæſtionibus de Maximis & Minimis; aliiſque quibuſdam, de quibus jam non loquor. BURGUM POSTERIOR.

Fundamentum harum operationum, ſatis obvium quidem, quoniam jam non poſſum explicationem ejus proſequi, ſic potius celavi § 6*accdæ*13-*eff*7*i*3*l*9*n*4*o*4*qrr*4*ſ*9*t*12*vx*.

Hoc fundamento conatus ſum etiam reddere || ſpeculationes de quadraturâ Curvarum ſimpliciores; pervenique ad theoremata quædam generaliora. Et, ut candidè agam, ecce primum theorema.

Ad Curvam aliquam ſit $dz^{\theta} = \overline{e+fz^{\eta}}^{\lambda}$ ordinatim applicata, termino abſciſſæ, ſeu baſis, z normaliter inſiſtens: ubi literæ d, e, f denotant quaſlibet quantitates datas; & θ, η, λ indices poteſtatum, ſive dignitatum, quantitatum quibus affixæ ſunt. Fac $\frac{\theta+1}{\eta} = r$; $\lambda + r = s$; $\frac{d}{\eta f} \times \overline{e+fz^{\eta}}^{+1} = \mathrm{Q}$; & $r\eta - \eta = \varpi$. Et area Curvæ erit Q in $\frac{z^{\varpi}}{s} - \frac{r-1}{s-1} \times \frac{eA}{fz^{\eta}} + \frac{r-2}{s-2} \times \frac{eB}{fz^{\eta}} - \frac{r-3}{s-3} \times \frac{eC}{fz^{\eta}} + \frac{r-4}{s-4} \times \frac{eD}{fz^{\eta}}$ &c. literis A, B, C, D, &c. denotantibus terminos proximè antecedentes; nempe A terminum $\frac{z^{\varpi}}{s}$, B terminum $-\frac{r-1}{s-1} \times \frac{eA}{fz^{\eta}}$ &c. Hæc ſeries, ubi r fractio eſt vel numerus negativus, continuatur in infinitum; ubi verò r integer eſt & affirmativus, continuatur ad tot terminos tantùm, quot ſunt unitates in eodem r; & ſic exhibet geometricam quadraturam Curvæ. Rem exemplis illuſtro. N° LVIII.

Exemplum 1. Proponatur Parabola, cujus ordinatim applicata ſit $\sqrt{ax}$. Hæc, in formam regulæ reducta, fit $z^{0} \times \overline{0+az^{1}}^{\frac{1}{2}}$. Quare $d=1$; $\theta=0$; $e=0$; $f=a$; $\eta=1$; $\lambda=\frac{1}{2}$. Adeoque $r=1$; $s=1\frac{1}{2}$; $\mathrm{Q}=\frac{1}{a} \times az^{\frac{1}{2}}$; $\varpi=0$. Et erit area quæſita $\frac{1}{a} \times az^{\frac{3}{2}}$ in $\frac{1}{1\frac{1}{2}}$; hoc eſt, $\frac{2}{3}z\sqrt{az}$. Et ſic in genere ſi cz^{n} ponatur ordinatim applicata, prodibit area $\frac{c}{n+1}z^{n+1}$.

Exem. 2. Sit ordinatim applicata $\frac{a^4z}{\overline{c^4-2cczz+z^4}^{2}}$. Hæc per reductionem fit $a^4z \times \overline{cc-zz}^{-2}$; vel etiam $a^4z^{-3} \times \overline{-1+ccz^{-2}}^{-2}$. In priori caſu eſt $d=a^4$; $\theta=1$; $e=cc$; $f=-1$; $\eta=2$; $\lambda=-2$. Adeoque $r=1$; $s=-1$; $\mathrm{Q}=-\frac{a^4}{2} \times \overline{cc-zz}^{-1}$, hoc eſt $-\frac{a^4}{2cc-2zz}$; $\varpi=0$. Et area curvæ Q in

mini primi; terminus ſecundus ſeriei, in quam dignitas Binomii per regulam illam reſolvitur, erit momentum dignitatis Binomii. Poſterior pars problematis ſolvitur regrediendo à momentis ad fluentes: quod ubi hæretur, fieri ſolet quadrando figuras; & ubi ad quadraturas hæretur, extrahendo fluentes per regulas quatuor; quarum duas Newtonus in epiſtolâ priore explicuit; duas alias, ſub finem hujus epiſtolæ, literis tranſpoſitis occultavit; ut mox dicetur.

|| Hujuſmodi theoremata Newtono ante annum 1669 innotuiſſe patet, per analyſin ſuprà impreſſam pag. 503, lin. 2, & per epiſtolam Collinii ad Thomam Strode 26 *Julii* 1672 datâ, pag. 509, lin. 24, 25, 26, ut & per hanc epiſtolam.

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$-\frac{z^0}{1}$, id est $= \frac{a^4}{2cc-2zz}$. In secundo autem casu, est $d = a^4$; $\theta = -3$; $e = -1$; $f = cc$; $\eta = -2$; $\lambda = -2$; $r = 1$; $s = -1$; $q = -\frac{a^4}{2cc} \times \overline{-1+ccz^{-2}}^{-1}$, id est $-\frac{a^4zz}{2c^4-2ccxz}$; $\varpi = 0$. Et area $= q$ in $-\frac{z^0}{1}$, hoc est $\frac{a^4zz}{2c^4-2cczz}$. Area his casibus diversimodè exhibetur, quatenus computatur à diversis finibus; quorum assignatio, per hos inventos valores arearum, facilis est.

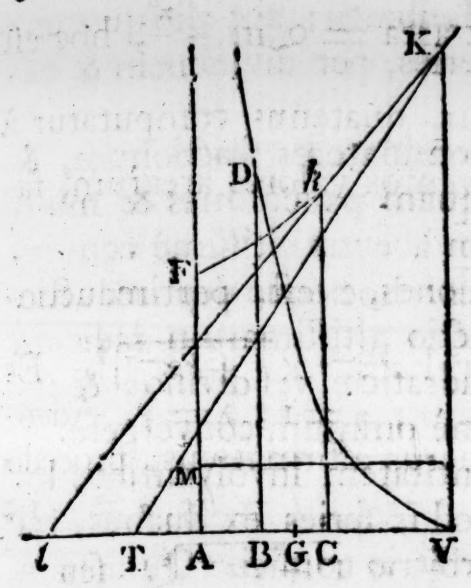
Exem. 3. Sit ordinatim applicata $\frac{a^5}{z^5}\sqrt{bz+zz}$: hoc est, per reductionem ad debitam formam; vel $a^5z^{-\frac{9}{2}} \times \overline{b+z}^{\frac{1}{2}}$; vel $a^5z^{-4} \times \overline{1+bz^{-1}}^{\frac{1}{2}}$. Et erit, in priori casu, $d = a^5$; $\theta = -\frac{9}{2}$; $e = b$; $f = 1$; $\eta = 1$; $\lambda = \frac{1}{2}$. Adeoque $r = -\frac{7}{2}$, &c. Quare, cùm r non sit numerus affirmativus, procedo ad alterum casum. Hic est $d = a^5$; $\theta = -4$; $e = 1$; $f = b$; $\eta = -1$; $\lambda = \frac{1}{2}$. Adeoque $r = 3$; $s = 3\frac{1}{2}$; $q = -\frac{a^5}{b} \times \overline{1+bz^{-1}}^{\frac{1}{2}}$, seu $-\frac{a^5z+a^5b}{bzz}\sqrt{zz+bz}$; $\pi = -2$. Et area, q in $\frac{z^{-2}}{3\frac{1}{2}} - \frac{2}{2\frac{1}{2}} \times \frac{z^{-1}}{3\frac{1}{2}b} + \frac{1}{1\frac{1}{2}} \times \frac{2}{2\frac{1}{2}} \times \frac{z^0}{3\frac{1}{2}bb}$; hoc est $\frac{-30bb+24bz-16zz}{105bbzz}$ in $\frac{a^5z+a^5b}{bzz}\sqrt{zz+bz}$.

Exem. 4. Sit denique ordinatim applicata $\frac{bz^{\frac{2}{3}}}{\sqrt{5ac^3-3accz^{\frac{2}{3}}+3aacz^{\frac{4}{3}}-a^3zz}}$. Hæc, ad formam regulæ reducta, fit $bz^{\frac{2}{3}} \times \overline{c-az^{\frac{2}{3}}}^{-\frac{1}{2}}$. Indeque est $d = b$; $\theta = \frac{1}{3}$; $e = c$; $f = -a$; $\eta = \frac{2}{3}$; $\lambda = -\frac{3}{5}$; $r = 2$; $s = \frac{7}{5}$; $q = -\frac{3b}{2a} \times \overline{c-az^{\frac{2}{3}}}^{\frac{2}{5}}$; $\varpi = \frac{2}{3}$. Et area $q \times \frac{5z^{\frac{2}{3}}}{7} - \frac{5}{2} \times -\frac{5c}{7a}$, id est $-\frac{30abz^{\frac{2}{3}}+75bc}{28aa} \times \overline{c-az^{\frac{2}{3}}}^{\frac{2}{5}}$. Quòd si res non successisset in hoc casu, existente r vel fractione vel numero negativo: tunc tentâssem alterum casum, purgando terminum $-az^{\frac{2}{3}}$ in ordinatim applicatâ à coefficiente $z^{\frac{2}{3}}$; hoc est reducendo ordinatim applicatam ad hanc formam; $bz^{-\frac{1}{15}} \times \overline{a+cz^{-\frac{2}{3}}}^{-\frac{3}{5}}$. Et si r in neutro casu fuisset numerus integer & affirmativus; concluſissem Curvam ex earum numero esse, quæ non possunt geometricè quadrari. Nam, quantum animadverto, hæc regula exhibet in finitis æquationibus areas omnium, geometricam quadraturam admittentium, Curvarum; quarum ordinatim applicatæ constant ex potestatibus, radicibus, vel quibuslibet dignitatibus binomii cujuscunque, licet non directè, ubi index dignitatis est numerus integer.

At quando hujusmodi Curva aliqua non potest geometricè quadrari; sunt ad manus alia theoremata pro comparatione ejus cum conicis sectionibus, vel saltem cum aliis figuris simplicissimis, quibuscum potest comparari: ad quod sufficit etiam hoc ipsum unicum jam descriptum theorema, si debitè concinnetur.

Pro

Pro trinomiis etiam, & aliis quibuſdam *, regulas quaſdam concinnavi. BURGOM POSTERIOR.
Sed in ſimplicioribus vulgoque celebratis figuris, vix aliquid relatu dignum reperi, quod evaſit aliorum conatus; niſi fortè Longitudo Ciſſoidis ejuſmodi cenſeatur. Ea ſic conſtruitur.



Sit VD ciſſois; AV, diameter circuli ad quem aptatur; V, vertex; AF, aſymptotos ejus; ac DB, perpendiculare quodvis ad AV demiſſum. Cum ſemi-axe AF = AV, & ſemi-parametro AG = ¼ AV, deſcribatur hyperbola F*k*K; & inter AB & AV ſumptâ AC mediâ proportionali, erigantur, ad C & V, perpendicula C*k*, VK, hyperbolæ occurrentia in *k* & K; et agantur rectæ, KT, *kt*, tangentes hyperbolam in eiſdem K & *k*, & occurrentes AV in T & *t*; & ad AV conſtituatur rectangulum VM æquale ſpacio TK*kt*. Et ciſſoidis VD longitudo erit ſextupla altitudinis AM. Demonſtratio perbrevis eſt (*). Sed ad infinitas ſeries redeo.

Quamvis multa reſtent inveſtiganda, circa modos approximandi, & circa diverſa ſerierum genera, quæ poſſunt ad id conducere: tamen vix cum D. Tſchurnhauſio ſperaverim, dari poſſe aut ſimpliciora, aut magis generalia fundamenda reducendi quantitates ad hoc genus ſerierum, de quo agimus, quàm ſunt diviſiones, & extractiones radicum; quibus Leibnitius & ego utimur; ſaltem non generaliora: quia pro quadraturâ & Εὐθύνσει Curvarum ac ſimilibus, nullæ poſſunt dari ſeries ex hiſce ſimplicibus terminis algebraicis (unicam tantùm indefinitam quantitatem involventibus) conſtantes, quas non licet hâc methodo colligere.

Nam non poſſunt eſſe plures convergentes ſeries ad idem determinandum, quàm ſunt indefinitæ quantitates, ex quarum poteſtatibus ſeries conflentur: & ego quidem, ex adhibitâ quâcunque indefinitâ quantitate, ſeriem novi colligere; & idem credo Leibnitio in poteſtate eſſe.

Nam quamvis meâ methodo liberum ſit eligere, pro conflandâ ſerie, quantitatem quamlibet indefinitam, à quâ quæſitum dependeat; & methodus, quam ipſe nobiſcum communicavit, determinata videatur ad electionem talium indefinitarum quantitatum, quibus opus commodè deduci poteſt ad fractiones, quæ per ſolam diviſionem evadant ſeries infinitæ; tamen aliæ quæcunque indefinitæ quantitates pro ſeriebus conflandis adhiberi poſſunt; per methodum iſtam, quâ affectæ æquationes reſolvuntur; dummodo reſolvantur in propriis terminis; hoc eſt, conficiendo ſeriem ex ſolis terminis, quos æquatio involvit.

Præterea non video, cur dicatur his diviſionibus & extractionibus problemata reſolvi per accidens: ſiquidem hæ operationes eodem modo ſe habeant ad hoc genus algebræ, ac vulgares operationes arithmeticæ ad algebram vulgo notam.

* Hæ omnes regulæ propoſitionem quintam ſextam ſeptimam & octavam Libri de Quadraturis conſtituunt.

(*) Conſule Geometriam Analyticam, Cap. XII, § 43, 44.

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Quod autem ad ſimplicitatem methodi attinet; nolim fractiones & radicales, abſque prævia reductione, ſemper reſolvi in ſeries infinitas: ſed, ubi perplexæ quantitates occurrunt, tentandæ ſunt omnimodæ reductiones; ſive fiat augendo, minuendo, multiplicando, vel dividendo quantitates indefinitas; ſive per methodum tranſmutatoriam Leibnitii; aut alio quocunque modo, qui occurrat. Et tunc reſolutio in ſeries, per diviſionem & extractionem, opportunè adhibebitur.

Hîc autem præcipue nitendum eſt, ut denominatores fractionum, & quantitates in vinculo radicum, reducantur ad quàm pauciſſimas & minimè compoſitas; & ad tales etiam, quæ in ſeriem abeunt citiſſimè convergentem, etſi radices neque convertantur in fractiones, neque deprimantur. Nam, per regulam initio alterius epiſtolæ, extractio altiſſimarum radicum æquè ſimplex & facilis eſt, ac extractio radicis quadraticæ, vel diviſio: & ſeries, quæ per diviſionem eliciuntur, ſolent minimè omnium convergere.

N° LIX. Hactenus de ſeriebus unicam indefinitam quantitatem involventibus locutus ſum. Sed poſſunt etiam, perſpectâ methodo, ſeries ex duabus, vel pluribus, aſſignatis indefinitis quantitatibus pro arbitrio confici. Quinetiam beneficio ejuſdem methodi poſſunt ſeries ad omnes figuras efformari, Gregorianis, ad circulum & hyperbolam editis, affines; hoc eſt, quarum ultimus terminus exhibebit quæſitam aream. Sed calculum hîc oneroſiorem nolim lubens ſubire.

Poſſunt denique ſeries ex terminis compoſitis eâdem methodo conſtitui. Quemadmodum, ſi ſit $\sqrt{aa-ax+\frac{x^3}{a}}$ ordinatim applicata Curvæ alicujus; pono $aa-ax=zz$; & ex binomio $zz+\frac{x^3}{a}$ extractâ radice, prodibit $z+\frac{x^3}{2az}-\frac{x^6}{8aaz^3}$ &c. Cujus ſeriei omnes termini quadrari poſſunt per theorema jam antè deſcriptum. Sed hæc minoris facio; quòd ubi ſeries ſimplices non ſunt ſatis tractabiles, aliam nondum communicatam methodum habeo, quâ pro lubitu acceditur ad quæſitum.

Ejus fundamentum eſt commoda, expedita, generalis ſolutio hujus problematis: *Curvam geometricam deſcribere, quæ per data quotcunque puncta tranſibit* (a).

Docuit Euclides deſcriptionem circuli per tria data puncta. Poteſt etiam conica ſectio deſcribi per quinque data puncta; & Curva trium dimenſionum per ſeptem data puncta: adeo ut in poteſtate habeam deſcriptionem omnium Curvarum iſtius ordinis, quæ per ſeptem tantum puncta determinantur. Hæc ſtatim geometricè fiunt, nullo calculo interpoſito. Sed ſuperius problema eſt alterius generis: & quàmvis primâ fronte intractabile videatur, tamen res aliter ſe habet. Eſt enim ferè ex pulcherrimis, quæ ſolvere deſiderem.

Seriei à D. Leibnitio pro quadraturâ conicarum ſectionum propoſitæ, affinia ſunt theoremata quædam, quæ, pro comparatione Curvarum cum conicis ſectionibus, in catalogum * dudum retuli.

(a) Vide Methodum Differentialem, Tom. I. p. 521—528.

* Ex his patet propoſitiones Newtoni de Quadraturâ Curvarum diu ante annum 1676 inventas fuiſſe.

Poſſum

Possum utique cum sectionibus conicis geometricè comparare Curvas omnes (numero infinities infinitas) quarum ordinatim applicatæ sunt BURGUM POSTERIOR.

$$\frac{dz^{\eta-1}}{e+fz^{\eta}+gz^{2\eta}} \text{ vel } \frac{dz^{2\eta-1}}{e+fz^{\eta}+gz^{2\eta}} \text{ &c.}$$

$$\text{Aut } \frac{dz^{\frac{1}{2}\eta-1}}{e+fz^{\eta}+gz^{2\eta}} \text{ vel } \frac{dz^{\frac{3}{2}\eta-1}}{e+fz^{\eta}+gz^{2\eta}} \text{ &c.}$$

$$\text{Aut } \frac{d}{z}\sqrt{e+fz^{\eta}+gz^{2\eta}} \text{ vel } dz^{\eta-1}\times\sqrt{e+fz^{\eta}+gz^{2\eta}} \text{ &c.}$$

$$\text{Aut } \frac{dz^{\eta-1}}{\sqrt{e+fz^{\eta}+gz^{2\eta}}} \text{ vel } \frac{dz^{2\eta-1}}{\sqrt{e+fz^{\eta}+gz^{2\eta}}} \text{ &c.}$$

$$\text{Aut } \frac{dz^{\eta-1}\times\sqrt{e+fz^{\eta}}}{g+hz^{\eta}} \text{ vel } \frac{dz^{2\eta-1}\times\sqrt{e+fz^{\eta}}}{g+hz^{\eta}} \text{ &c.}$$

$$\text{Aut } \frac{dz^{\eta-1}}{g+hz^{\eta}\times\sqrt{e+fz^{\eta}}} \text{ vel } \frac{dz^{2\eta-1}}{g+hz^{\eta}\times\sqrt{e+fz^{\eta}}} \text{ &c.}$$

$$\text{Aut } \frac{d}{z}\sqrt{\frac{e+fz^{\eta}}{g+hz^{\eta}}} \text{ vel } dz^{-1}\times\sqrt{\frac{e+fz^{\eta}}{g+hz^{\eta}}} \text{ &c.}$$

Hîc d, e, f, g significant quasvis datas quantitates, cum suis signis + & − affectas; z, axem vel basem Curvæ; & η, 2η, $\frac{1}{2}\eta-1$, $\frac{3}{2}\eta-1$, $\eta-1$, $2\eta-1$, indices potestatum vel dignitatum z, sive sint affirmativi vel negativi, sive integri vel fracti: & singula bina theoremata sunt duo primi termini seriei in infinitum progredientis. In tertio & quarto, $4g$ debet esse non majus quàm ff, nisi e & g sint contrarii signi. In cæteris nulla est limitatio. Horum aliqua (nempe, secundum, tertium, quartum, quintum, & decimum tertium) ex areis duarum conicarum sectionum conjunctis constant. Alia quædam (ut nonum, decimum, & duodecimum) sunt aliter satis composita. Et omnia quidem, in continuatione progressionum, citò evadunt compositissima; adeo ut vix per transmutationem figurarum, quibus Jacobus Gregorius & alii usi sunt, absque ulteriori fundamento inveniri posse putem.

Ego quidem haud quicquam generale in his obtinere potui, antequam abstraherem à contemplatione figurarum, & rem totam ad simplicem considerationem solarum ordinatim applicatarum reducerem. Sed, cùm hæc, & hisce generaliora, sint in potestate; non dubitabitur, credo, de binomialibus longè facilioribus, quæ in his continentur; & prodeunt, ponendo literam aliquam, e vel f vel g, $=0$; & $\eta=1$ vel 2; etsi series, in quas ista resolvantur, non posuerim in epistolâ priori, nedum fortè computaverim; intentus, non in omnia particularia enumeranda, sed in illustrandam methodum per unam & alteram, in singulis rerum generibus, instantiam; quæ ad ostendendam ejus generalitatem sufficere videbatur.

CÆTERUM hæc theoremata dant series plusquam uno modo. Nam primum, si ponatur $f=0$, & $\eta=1$, evadit $\frac{d}{e+gzz}$; unde prodit series nobis communicata. Sed si ponatur $2eg=ff$, & $\eta=1$, inde tandem obtinemus hanc N° LX.

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hanc ſeriem * $1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9} + \frac{1}{11} - \frac{1}{13} - \frac{1}{15}$ &c. pro longitudine quadrantalis arcûs, cujus chorda eſt unitas: vel, quod perinde eſt, hanc $\frac{1}{2} + \frac{1}{15} - \frac{1}{63} + \frac{1}{143} - \frac{1}{255}$ &c. pro longitudine dimidii ejus. Et has fortè, quia æquè ſimplices ſunt ac alteræ, & magis convergunt, non repudiabitis.

Sed ego rem aliter æſtimo. Illud enim melius quod utilius eſt, & problema minori labore ſolvit. Sic, quamvis hæc æquatio $x^3 - x = 1$ appareat ſimplicior hâcce, $yy - 2y\sqrt{\frac{81}{25} - \sqrt{20}} = \sqrt{20}$; tamen in confeſſo eſt poſteriorem reverâ ſimpliciorem eſſe; propterea quòd radicem ejus, y, geometra faciliùs eruit.

Et ob hanc rationem ſeries pro obtinendis arcubus circuli, vel, quod eodem recidit, pro obtinendis ſectoribus conicarum ſectionum, pro optimis habeo, quæ componuntur ex poteſtatibus ſinuum.

Nam ſiquis vellet per ſimplex computum hujus ſeriei $1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$ &c. colligere longitudinem quadrantis ad viginti figurarum loca decimalia, opus eſſet 5 000 000 000 terminis ſeriei circiter; ad quorum calculum milleni anni requirerentur. Et res tardiùs obtineretur per tangentem 45 graduum. Sed, adhibito ſinu recto 45 graduum, quinquaginta quinque vel ſexaginta termini hujus ſeriei, $\sqrt{\frac{1}{2} \times 1 + \frac{1}{12} + \frac{3}{160} + \frac{5}{896}}$ &c. ſufficerent: quorum computatio tribus, ut opinor, vel quatuor diebus abſolvi poſſet.

Et tamen hic non eſt optimus modus computandi totam peripheriam. Nam ſeries ex ſinu recto 30 graduum, vel ſinu verſo 60 graduum conflata, multo citiùs dabit arcum ſuum; cujus ſextuplum, vel duodecuplum, eſt tota peripheria. Neque majori labore eruitur area totius circuli, ex ſegmento, cujus ſagitta eſt quadrans diametri. Ejus computi ſpecimen, ſiquidem ad manus eſt, viſum fuit apponere; & unà adjungere aream hyperbolæ, quæ eodem calculo prodit.

Poſito axe tranſverſo $= 1$, & ſinu verſo, ſeu ſegmenti ſagittâ, $= x$; erit ſemi-ſegmentum $\left.\begin{matrix}\text{hyperbolæ}\\ \text{circuli}\end{matrix}\right\} = x^{\frac{1}{2}}$ in $\frac{2}{3}x + \frac{xx}{-5} - \frac{x^3}{28} + \frac{x^4}{-72}$ &c. Hæc autem ſeries ſic in infinitum producitur, ſit $2x^{\frac{3}{2}} = a$; $\frac{ax}{2} = b$; $\frac{bx}{4} = c$; $\frac{3cx}{6} = d$; $\frac{5dx}{8} = e$; $\frac{7ex}{10} = f$; &c. Et erit ſemi-ſegmentum $\left.\begin{matrix}\text{hyperbolæ}\\ \text{circuli}\end{matrix}\right\} = \frac{a}{3} + \frac{b}{-5} - \frac{c}{7} + \frac{d}{-9} - \frac{e}{11} + \frac{f}{-13}$ &c. Eorumque ſemi-ſumma $\frac{a}{3} - \frac{c}{7} - \frac{e}{11} -$ &c. & ſemi-differentia $\frac{b}{5} + \frac{d}{9} + \frac{f}{13} +$ &c. His ita præparatis; ſuppono $x = \frac{1}{4}$, quadrantem nempe axis; & prodit $a\ (= \frac{1}{4}) = 0.25$; $b\ (= \frac{ax}{2} = \frac{0.25}{1 \times 8}) = 0.03125$; $c\ (= \frac{bx}{4} = \frac{0.03125}{2 \times 8}) = 0.001953125$; $d\ (= \frac{3cx}{6} = \frac{0.001953125}{8}) = 0.000244140625$. Et ſic procedo, uſque dum venero ad terminum depreſſiſſimum, qui poteſt ingredi opus. Deinde hos terminos per 3, 5, 7, 9, 11, &c.

* D. Vicecomes Brounker hyperbolam per hanc ſeriem $\frac{1}{1 \times 2} + \frac{1}{3 \times 4} + \frac{1}{5 \times 6} + \frac{1}{7 \times 8} +$ &c. id eſt

&c. respectivè divisos, dispono in duas tabulas: ambiguos cum primo in unam; & negativos in aliam; & addo, ut hîc vides, BURGUM POSTERIOR.

0.083333333333333	0.000279017857142
625000000000	34679066051
271267361111	8344650 27
51351693 96	2628535 4
14462891 7	96129 6
4954581	3867 6
190948	1663
7963	75
352	4
16	
1	
0.089610988564661 8	0.000282571938957 5

Tunc à priori summâ aufero posteriorem; & restat 0.089328416625704 3 area semi-segmenti hyperbolici. Addo etiam eas summas, & aggregatum aufero à primo termino duplicato, 0.166666666666666; & restat 0.076773106163047 3 area semi-segmenti circularis. Huic addo triangulum istud, quo completur in sectorem, hoc est $\frac{1}{3}\sqrt{3}$, seu 0.054126587736527 4; & habeo sectorem 60 graduum, 0.130899693899574 7; cujus sextuplum, 0.785398163397448 2, est area totius circuli: quæ divisa per $\frac{1}{4}$, sive quadrantem diametri, dat totam peripheriam 3.141592653589792 8. Si alias artes adhibuissem; potui, per eundem numerum terminorum seriei, perveniss ad multo plura loca figurarum, puta viginti quinque, aut amplius: sed animus fuit hîc ostendere, quid, per simplex seriei computum, præstari posset. Quod sane haud difficile est; cùm in omni opere multiplicatores ac divisores, magnâ ex parte, non majores quàm 11, & nunquam majores quàm 41, adhibere opus sit.

Per seriem Leibnitii etiam, si ultimo loco dimidium termini adjiciatur, & alia quædam similia artificia adhibeantur, potest computum produci ad multas figuras. Ut & ponendo summam terminorum $1 - \frac{1}{7} + \frac{1}{9} - \frac{1}{15} + \frac{1}{17} - \frac{1}{23} + \frac{1}{25} - \frac{1}{31} + \frac{1}{33}$ &c. esse ad totam seriem $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11}$ + &c. ut $1 + \sqrt{2}$ ad 2. Sed optimus ejus usus videtur esse, quando vel conjungitur cum duabus aliis persimilibus, & citissimè convergentibus, seriebus; vel sola adhibetur ad computandum arcum 30 graduum, positâ tangente $\sqrt{\frac{1}{3}}$. Tunc enim series illa evadit $1 - \frac{1}{3\times3} + \frac{1}{5\times9} - \frac{1}{7\times27} + \frac{1}{9\times81}$ &c. quæ cito convergit. Vel, si conjunges cum aliis seriebus, pone circuli diametrum = 1, & $a = \frac{1}{2}$; & area totius circuli erit summa harum trium serierum $\frac{a}{1} - \frac{a^3}{3} + \frac{a^5}{5} - \frac{a^7}{7} + \frac{a^9}{9} - \frac{a^{11}}{11}$ + &c. $\frac{aa}{1} + \frac{a^5}{3} - \frac{a^8}{5} - \frac{a^{11}}{7} + \frac{a^{14}}{9} + \frac{a^{17}}{11}$ &c. $\frac{a^4}{1} - \frac{a^{10}}{3} + \frac{a^{16}}{5} - \frac{a^{22}}{7} + \frac{a^{28}}{9}$ &c.

est per hanc $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8}$ + &c. (conjunctis binis terminis) primus omnium quadravit. Mercator hanc quadraturam aliter demonstravit. Gregorius communicavit hanc seriem pro circulo $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11}$ &c. & Newtonus hanc $1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9} + \frac{1}{11} - \frac{1}{13} - \frac{1}{15}$ + &c.

Hîc

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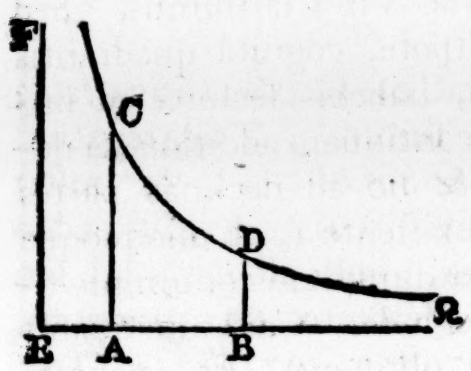
Hîc conſideravimus ſeries, quatenus adhibentur ad computandum totum circulum. Sed quando computandæ ſunt partes ejus, tunc quælibet ſeries habet proprium uſum, & in ſuo genere optima eſt. Si datur tangens ſatis parva, vel ſatis magna; non recurrendum erit ad ſinum aliquem ut inde computetur arcus, neque vice verſâ. Series, dato congruens, eſt æquatio pro ſolvendo proprio problemate.

N° LXI.

Credo Cl. Leibnitium, dum poſuit ſeriem pro determinatione coſinûs ex arcu dato, vix animadvertiſſe ſeriem meam pro determinatione ſinûs verſi ex eodem arcu; ſiquidem hæc idem ſunt.

Neque obſervâſſe videtur morem meum generaliter uſurpandi literas, pro quantitatibus cum ſignis ſuis, + & −, affectis, dum dividit hanc ſeriem $\frac{z}{b}+\frac{zz}{2abb}+\frac{z^3}{6aab^3}+\frac{z^4}{24a^3b^4}+$ &c. Nam cùm area hyperbolica AD, hîc ſignificata per z, ſit affirmativa vel negativa, prout jaceat ex unâ vel alterâ parte ordinatim applicatæ AC; ſi area illa in numeris data ſit l, & l ſubſtituatur in ſerie pro z, orietur vel $\frac{l}{b}+\frac{ll}{2abb}+\frac{l^3}{6aab^3}+\frac{l^4}{24a^3b^4}$ &c. vel $-\frac{l}{b}+\frac{ll}{2abb}-\frac{l^3}{6aab^3}+\frac{l^4}{24a^3b^4}$ &c; prout l ſit affirmativa vel negativa.



Hoc eſt poſito $a=1=b$, & l logarithmo hyperbolico; numerus, ei correſpondens, erit $1+\frac{l}{1}+\frac{ll}{2}+\frac{l^3}{6}+\frac{l^4}{24}$ &c. ſi l ſit affirmativus; & $1-\frac{l}{1}+\frac{ll}{2}-\frac{l^3}{6}+\frac{l^4}{24}$ &c. ſi l ſit negativus. Hoc modo fugio multiplicationem theorematum, quæ aliâs in nimiam molem creſcerent. Nam v. g. illud unicum theorema, quod ſuprà poſui pro quadraturâ Curvarum, reſolvendum eſſet in 32 theoremata, ſi pro ſignorum varietate multiplicaretur.

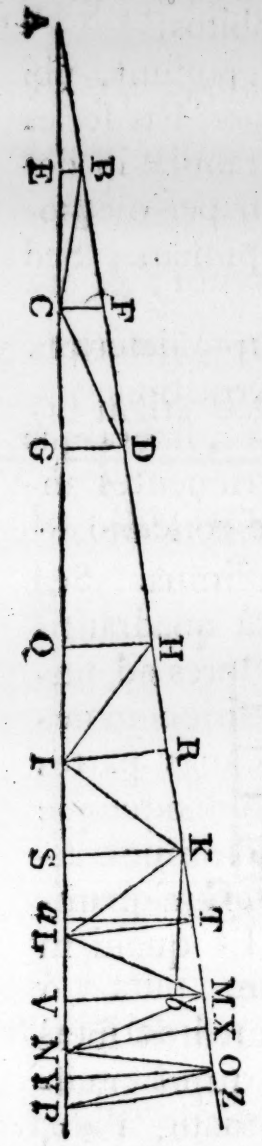
Præterea, quæ habet Vir Clariſſimus de inventione numeri, unitate majoris, per datum logarithmum hyperbolicum, ope ſeriei $\frac{l}{1}-\frac{ll}{1\times2}+\frac{l^3}{1\times2\times3}-\frac{l^4}{1\times2\times3\times4}$ &c. potius quàm ope ſeriei $\frac{l}{1}+\frac{ll}{1\times2}+\frac{l^3}{1\times2\times3}+\frac{l^4}{1\times2\times3\times4}$ &c. nondum percipio. Nam ſi unus terminus adjiciatur amplius ad ſeriem poſteriorem, quàm ad priorem, poſterior magis appropinquabit. Et certè minor eſt labor, computare unam, vel duas primas figuras adjecti hujus termini, quàm dividere unitatem per numerum prodeuntem ex logarithmo hyperbolico, ad multa figurarum loca extenſum, ut inde habeatur numerus quæſitus unitate major. Utraque igitur ſeries, ſi duas dicere fas ſit, officio ſuo fungatur. Poteſt tamen $\frac{l}{1}+\frac{l^3}{1\times2\times3}+\frac{l^5}{1\times2\times3\times4\times5}$ &c. ſeries, ex dimidiâ parte terminorum conſtans, optimè adhiberi; ſiquidem hæc dabit ſemi-differentiam duorum numerorum, ex quâ, & rectangulo dato, uterque datur. Sic & ex ſerie $1+\frac{ll}{1\times2}+\frac{l^4}{1\times2\times3\times4}$ &c. datur ſemi-ſumma numerorum,

rum, indeque etiam numeri. Unde prodit relatio ſerierum inter ſe, quâ, ex unâ datâ, dabitur altera. BURGUM POSTERIOR.

Theorema de inventione arcûs ex dato coſinu, ponendo radium 1, coſinum c, & arcum $\sqrt{6-\sqrt{24c+12}}$, minus appropinquat, quàm primâ fronte videtur. Poſito quidem ſinu verſo v, error erit $\frac{v^3}{90}+\frac{v^4}{194}+$&c. Poteſt fieri ut $120-27v$ ad $120-17v$, ita chorda $(\sqrt{2v})$ ad arcum; & error erit tantùm $\frac{61v^3\sqrt{2v}}{44800}$ circiter; qui ſemper minor eſt quàm $5\frac{1}{4}$ minuta ſecunda, dum arcus non ſit major quàm 45 grad. Et ſingulis etiam biſectionibus diminuitur 128 vicibus.

Series $\frac{a^3}{1\times2\times3}-\frac{a^5}{1\times2\times3\times4\times5}+\frac{a^7}{1\times2\times3\times4\times5\times6\times7}$ &c. applicari poſſet ad computationem tabulæ ſegmentorum, ut obſervat Vir Clariſſimus. Sed res optimè abſolvitur per canonem ſinuum. Utpote, cognitâ quadrantis areâ, per continuam additionem nonæ partis ejus, habebis ſectores ad ſingulos decem gradus in ſemicirculo: deinde, per continuam additionem decimæ partis hujus, habebis ſectores ad gradus; & ſic ad decimas partes graduum, & ultrà, procedi poteſt. Tunc, radio exiſtente 1, ab unoquoque ſectore, & ejus complemento ad 180 gradus, aufer dimidium communis ſinûs recti, & relinquentur ſegmenta in tabulam referenda. Cæterùm quamvis ſeries hîc non proſint, in aliis tamen locum obtinent. Et quoniam hoc ad earum uſum ſpectat, non gravabor in aliquibus attingere.

Conſtructionem logarithmorum non aliunde peti debere, credetis fortè ex hoc ſimplici proceſſu, qui ab iſtis pendet. Per methodum ſuprà traditam quærantur logarithmi hyperbolici numerorum 10, 0.98, 0.99, 1.01, 1.02: id quod fit ſpatio unius & alterius horæ. Dein, diviſis logarithmis quatuor poſteriorum per logarithmum numeri 10, & addito indice 2, prodibunt veri logarithmi numerorum 89, 99, 100, 101, 102, in tabulam referendi. Hi per dena intervalla interpolandi ſunt; & exibunt logarithmi omnium numerorum inter 980 & 1020: & omnibus inter 980 & 1000 iterum per dena intervalla interpolatis, habebitur tabula eatenus conſtructa. Tunc ex his colligendi erunt logarithmi omnium primorum numerorum, & eorum multiplicium, minorum quàm 100: ad quod nihil requiritur, præter additionem & ſubtractionem. Siquidem fit $\sqrt[10]{\frac{9984\times1020}{9945}}=2$. $\sqrt[4]{\frac{8\times9963}{984}}=3$. $\frac{10}{2}=5$. $\sqrt{\frac{98}{2}}=7$. $\frac{99}{9}=11$. $\frac{1001}{7\times11}=13$. $\frac{102}{6}=17$. $\frac{988}{4\times13}=19$. $\frac{9936}{16\times27}=23$. $\frac{986}{2\times17}=29$. $\frac{992}{32}=31$. $\frac{999}{27}=37$. $\frac{984}{24}=41$. $\frac{989}{23}=43$. $\frac{987}{21}=47$. $\frac{9911}{11\times17}=53$. $\frac{9971}{13\times13}=59$. $\frac{9882}{2\times81}=61$. $\frac{9949}{3\times49}=67$. $\frac{994}{14}=71$. $\frac{9928}{8\times17}=73$. $\frac{9954}{7\times18}=79$. $\frac{996}{12}=83$. $\frac{9968}{7\times16}=98$. $\frac{9894}{6\times17}=97$. Et habitis ſic logarithmis omnium numerorum minorum quàm 100; reſtat tantùm hos etiam, ſemel atque iterum, per dena intervalla interpolare. N° LXII.



Conſtructionis tabulæ ſinuum, à quâ pendet tota res trigonometrica, fundamentum optimum eſt continua additio dati anguli ad ſeipſum, vel ad alium datum. Utpote in angulo addendo BAE; inſcribantur HI, IK, KL, LM, MN, NO, OP, &c. æquales radio AB: & ad oppoſita latera demittantur perpendiculares, BE, HQ, IR, KS, LT, MV, NX, OY, &c. Et angulorum HIQ, IKH, KLI, LMK, &c. differentiæ erunt angulus A; ſinus HQ, IR, KS, &c; & coſinus IQ, KR, LS, &c. Detur jam aliquis eorum LMK, & cæteri ſic eruentur. Ad SV & MV demitte perpendicula, T*a* & K*b*; & (propter ſimilia triangula ABE, TL*a*, KM*b*, ALT, AMV, &c.) erit AB.BE:: TL.L*a* $(=\frac{SL-LV}{2})$::KT $(=\frac{1}{2}KM)$.$\frac{1}{2}$M*b* $(=\frac{MV-KS}{2})$. Et AB.AE::KT.S*a* $(=\frac{SL+LV}{2})$::TL.T*a* $(=\frac{KS+MV}{2})$. Unde dantur ſinus & coſinus KS, MV, SL, LV. Et ſimul patet ratio continuandi progreſſiones. Nempe AB.2AE::LV.TM+MX::MX.VN + NY &c.::MV.TL + XN.::XN.MV + OY &c. Vel AB 2BE::LV.XN − TL::MV.TM − MX::MX.OY − MV:: XN.VN − NY &c. Et retro, AB.2AE::LS.KT + RK &c. Pone ergo AB = 1; & fac BE × TL = L*a*; AE × KT = S*a*; S*a* − L*a* = LV; $\overline{2AE \times LV} - TM = MX$ &c. Sed nodus eſt inventio ſinûs & coſinûs anguli A. Et hîc ſubveniunt ſeries noſtræ. Utpote cognitâ, ex ſuperioribus, quadrantalis arcûs longitudine 1.57079 &c; & ſimul quadrato ejus, 2.4694 &c; divide quadratum hoc per quadratum numeri exprimentis rationem 90 graduum ad angulum A: &, quoto dicto z, tres vel quatuor termini hujus ſeriei, $1 - \frac{z}{2} + \frac{zz}{24} - \frac{z^3}{720} + \frac{z^4}{40320}$ &c. dabunt coſinum iſtius anguli A. Sic primò quæri poteſt angulus 5 graduum, & inde tabula computari ad quinos gradus; ac deinde interpolari ad gradus, vel dimidios gradus, per eandem methodum. Nam non convenit progredi per nimios ſaltus. Duæ tertiæ partes tabulæ, ſic computatæ, dant reliquam tertiam partem per additionem vel ſubtractionem, more noto. Siquidem poſito KT coſinu 60 graduum; fit AE = SV, & BE = M*b*. Tunc ad decimas & centeſimas partes graduum pergendum eſt per aliam methodum; ſubſtitutis tamen priùs logarithmis ſinuum inventorum, ſi ejus generis tabula deſideretur.

Ad computum tabularum aſtronomicarum Kepleri; poſui fundamentum aliquod in alterâ epiſtolâ. Ejus ſeriei tres primi termini, & aliquando duo, ſufficiunt. Sed ad diverſas partes ellipſeos diverſæ ejuſmodi ſeries aptari debent. Vel potius tales ſeries computandæ ſunt, quæ ex datâ areâ ſectoris elliptici BGE, immediatè exhibeant aream ſectoris circuli, cujus angulus eſt BEG, radius CB. Et habitis hiſce, computum earum ad duos, tres, aut fortè quatuor terminos, beneficio logarithmorum, haud gravius erit, quàm ſolita reſolutio tot triangulorum in aliis hypotheſibus: imo fortè minus grave, ſi ſeries prius debitè concinnentur; ſiquidem unus logarithmus, è tabulâ

tabulâ petitus, determinet omnes iſtos terminos, addendo ipſum, & ejus multiplices, ad logarithmos datarum coefficientium in promptu habitos. BURGUM POSTERIOR.

Quæ de hoc genere tabularum dicuntur, ad alias transferri poſſunt, ubi ratiocinia geometrica locum non obtinent. Sufficit autem per has ſeries computare triginta, vel viginti, aut fortè pauciores terminos tabulæ in debitis diſtantiis; ſiquidem termini intermedii facilè interſeruntur per methodum quandam, quam in uſum calculatorum ferè hîc deſcripſiſſem. Sed pergo ad alia.

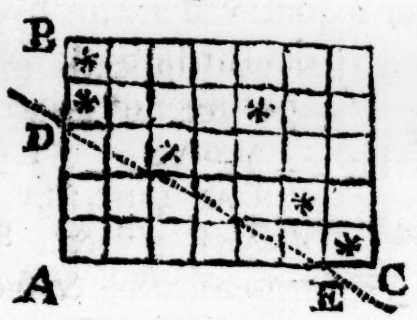
Quæ Cl. Leibnitius à me deſiderat explicanda, ex parte ſuprà deſcripſi. Quod verò attinet ad inventionem terminorum p, q, r, in extractione radicis affectæ: primum, p, ſic eruo. Deſcripto angulo recto BAC, latera ejus BA, CA divido in partes æquales; & inde normales erigo, diſtribuentes angulare ſpatium in æqualia parallelogramma vel quadrata; quæ concipio de- N° LXIII.

B Fig. 1.

x^4	x^4y	x^4yy	x^4y^3	x^4y	x^4y^5	x^4y^6
x^3	x^3y	x^3yy	x^3y^3	x^3y^4	x^3y^5	x^3y^6
x^2	x^2y	x^2yy	x^2y^3	x^2y^4	x^2y^5	x^2y^6
x	xy	xyy	xy^3	xy^4	xy^5	xy^6
o	y	yy	y^3	y^4	y^5	y^6

A C

Fig. 2.



nominata eſſe à dimenſionibus duarum indefinitarum ſpecierum, puta x & y, regulariter aſcendentium à termino A; prout vides in fig. 1. inſcriptas. Ubi y denotat radicem extrahendam; & x alteram indefinitam quantitatem, ex cujus poteſtatibus ſeries conficienda eſt. Deinde, cùm æquatio aliqua proponitur, parallelogramma, ſingulis ejus terminis correſpondentia, inſignio notâ aliquâ: & regulâ ad duo, vel fortè plura, ex inſignitis parallelogrammis applicatâ (quorum unum ſit humillimum in columna ſiniſtrâ juxtâ AB, & alia ad regulam dextrorſum ſita, cæteraque omnia non contingentia regulam ſupra eam jaceant) ſeligo terminos æquationis per parallelogramma, contingentia regulam, deſignatos; & inde quæro quantitatem quotienti addendam.

Sic ad extrahendam radicem y, ex $y^6 - 5xy^5 + \frac{x^3}{a}y^4 - 7a^2x^2y^2 + 6a^3x^3 + b^2x^4 = 0$; parallelogramma, hujus terminis reſpondentia, ſigno notâ aliquâ ✱; ut vides fig 2. Dein applico regulam, DE, ad inferiorem è locis ſignatis in ſiniſtrâ columnâ; eamque ab inferioribus ad ſuperiora dextrorſum gyrare facio, donec alium ſimiliter, vel fortè plura, è reliquis ſignatis locis cœperit attingere. Videoque loca ſic attacta eſſe x^3, $xxyy$ & y^6. E terminis itaque $y^6 - 7aaxxyy + 6a^3x^3$ tanquam nihilo æqualibus (& inſuper ſi placet reductis ad $v^6 - 7vv + 6 = 0$, ponendo $y = v\sqrt{ax}$) quæro valorem y, & invenio quadruplicem, $+\sqrt{ax}$, $-\sqrt{ax}$, $+\sqrt{2ax}$, & $-\sqrt{2ax}$: quorum quemlibet pro primo termino quotientis accipere licet, prout è radicibus quampiam extrahere decretum eſt.

NEWTONI AD OLDEN-

Sic * æquatio $y^3+axy+aay-x^3-2a^3=0$, quam reſolvebam in priori epiſtolâ, dat $-2a^3+aay+y^3=0$; & inde $y=a$ proximè. Cùm itaque a ſit primus terminus valoris y, pono p pro cæteris omnibus in infinitum, & ſubſtituo $a+p=y$. (Obvenient hîc aliquando difficultates nonnullæ; ſed ex iis, credo, D. Leibnitius ſe proprio marte extricabit.) Subſequentes verò termini, q, r, s, &c. eodem modo ex æquationibus ſecundis, tertiis, cæteriſque eruuntur, quo primus p è primâ; ſed curâ leviori; quia cæteri valores y ſolent prodire, dividendo terminum involventem infimam poteſtatem indefinitæ quantitatis, x, per coefficientem radicis p, q, r aut s.

N° LXIV. Intellexti credo, ex ſuperioribus, regreſſionem, ab areis Curvarum ad lineas rectas, fieri per hanc extractionem radicis affectæ. Sed duo alii ſunt modo, quibus idem perficio.

Eorum unus affinis eſt computationibus, quibus colligebam approximationes ſub finem alterius epiſtolæ, & intelligi poteſt per hoc exemplum. Proponatur æquatio ad aream hyperbolæ $z=x+\frac{1}{2}xx+\frac{1}{3}x^3+\frac{1}{4}x^4+\frac{1}{5}x^5$ &c. Et partibus ejus multiplicatis in ſe, emerget $z^2=x^2+x^3+\frac{11}{12}x^4+\frac{5}{6}x^5$ &c. $z^3=x^3+\frac{3}{2}x^4+\frac{7}{4}x^5$ &c. $z^4=x^4+2x^5$ &c. $z^5=x^5$ &c. Jam de z aufero $\frac{1}{2}z^2$, & reſtat $z-\frac{1}{2}z^2=x-\frac{1}{6}x^3-\frac{5}{24}x^4-\frac{13}{60}x^5$ &c. Huic addo $\frac{1}{6}z^3$, & fit $z-\frac{1}{2}z^2+\frac{1}{6}z^3=x+\frac{1}{24}x^4+\frac{3}{40}x^5$ &c. Aufero $\frac{1}{24}z^4$, & reſtat $z-\frac{1}{2}z^2+\frac{1}{6}z^3-\frac{1}{24}z^4=x-\frac{1}{120}x^5$ &c. Addo $\frac{1}{120}z^5$, & fit $z-\frac{1}{2}z^2+\frac{1}{6}z^3-\frac{1}{24}z^4+\frac{1}{120}z^5=x$ quamproximè; ſive $x=z-\frac{1}{2}z^2+\frac{1}{6}z^3-\frac{1}{24}z^4+\frac{1}{120}z^5$ &c.

Eodem modo ſeries de unâ indefinitâ quantitate in aliam transferri poſſunt. Quemadmodum ſi poſito r radio circuli, x ſinu recto arcûs z, & $x+\frac{x^3}{6rr}+\frac{3x^5}{40r^4}+$&c. longitudine arcûs iſtius; hanc ſeriem à ſinu recto ad tangentem vellem transferre: quæro longitudinem tangentis $\frac{rx}{\sqrt{rr-xx}}$, & reduco in infinitam ſeriem $x+\frac{x^3}{2rr}+\frac{3x^5}{8\times 4}$ &c. Vocetur hæc quantitas t. Colligo poteſtates ejus; $t^3=x^3+\frac{3x^5}{2rr}$ &c; $t^5=x^5+$&c. Aufero autem t de z; &, ponendo 1 pro r, reſtat $z-t=-\frac{x^3}{3}-\frac{3x^5}{10}$ &c. Addo $\frac{1}{3}t^3$, & fit $z-t+\frac{1}{3}t^3=\frac{1}{5}x^5+$&c. Aufero $\frac{1}{5}t^5$, & reſtat $z-t+\frac{1}{3}t^3-\frac{1}{5}t^5=0$ quamproximè. Quare eſt $z=t-\frac{1}{3}t^3+\frac{1}{5}t^5-$&c. Sed ſiquis in uſus trigonometricos me juſſiſſet exhibere expreſſionem arcûs per tangentem; eam, non hoc circuitu, ſed directâ methodo quæſiviſſem.

Per hoc genus computi colliguntur etiam ſeries, ex duabus, vel pluribus, indefinitis quantitatibus conſtantes; & radices affectarum æquationum magnâ ex parte extrahuntur. Sed ad hunc poſteriorem uſum adhibeo potius methodum in alterâ epiſtolâ deſcriptam, tanquam generaliorem, &, regulis

* Hanc Reſolutionem, vid. N° VII.

† Id eſt, *Una methodus conſiſtit in extractione fluentis quantitatis ex æquatione ſimul involvente fluxionem ejus: altera tantum in aſſumptione ſeriei, pro quantitate quâlibet incognitâ, ex quâ cætera commodè derivari poſſunt, & in collatione terminorum homologorum æquationis reſultantis, ad eruendos terminos aſſumptæ ſeriei.* Analyſin inverſam, per fluentes & earum momenta, in æquationibus tam infinitis quàm finitis, Newtonus in his epiſtolis ad regulas quatuor reduxit. Per primam extrahitur fluens ex binomiis,

gulis pro eliſione ſuperfluorum terminorum habitis, paulo magis expeditam. EBURGUM POSTERIOR.

Pro regreſſione verò ab areis ad lineas rectas, & ſimilibus, poſſunt hujuſmodi theoremata adhiberi.

Theorema 1. Sit $z = ay + byy + cy^3 + dy^4 + ey^5$ &c. Et vicim erit $y = \frac{z}{a} - \frac{b}{a^3}z^2 + \frac{2bb-ac}{a^5}z^3 + \frac{5abc-5b^3-aad}{a^7}z^4 + \frac{3aacc-21abbc+6aabd+14b^4-a^3e}{a^9}z^5$ &c.

Exempli gratiâ. Proponatur æquatio ad aream hyperbolæ, $z = y - \frac{yy}{2} + \frac{y^3}{3} - \frac{y^4}{4} + \frac{y^5}{5}$ &c. Et ſubſtitutis in regulâ 1 pro a, $-\frac{1}{2}$ pro b, $\frac{1}{3}$ pro c, $-\frac{1}{4}$ pro d, & $\frac{1}{5}$ pro e; viciſſim exurgit, $y = z + \frac{1}{2}zz + \frac{1}{6}z^3 + \frac{1}{24}z^4 +$ &c.

Theor. 2. Sit $z = ay + by^3 + cy^5 + dy^7 + ey^9 +$ &c. Et viciſſim erit $y = \frac{z}{a} - \frac{b}{a^4}z^3 + \frac{3bb-ac}{a^7}z^5 + \frac{8abc-aad-12b^3}{a^{10}}z^7 + \frac{55b^4-55abbc+10aabd+5aacc-a^3e}{a^{13}}z^9$ &c.

Exempli gratiâ. Proponatur æquatio ad arcum circuli, $z = y + \frac{y^3}{6rr} + \frac{3y^5}{40r^4} + \frac{5y^7}{112r^6}$ &c. Et ſubſtitutis in regulâ 1 pro a, $\frac{1}{6rr}$ pro b, $\frac{3}{40r^4}$ pro c, $\frac{5}{112r^6}$ pro d, &c; orietur $y = z - \frac{z^3}{6rr} + \frac{z^5}{120r^4} - \frac{z^7}{5040r^6} +$ &c.

Alterum modum regrediendi ab areis ad lineas rectas celare ſtatui.

Ubi dixi, omnia pene problemata ſolubilia exiſtere; volui de iis præſertim intelligi, circa quæ Mathematici ſe hactenus occupârunt, vel ſaltem inquibus ratiocinia mathematica locum aliquem obtinere poſſunt. Nam alia ſane, adeo perplexis conditionibus implicata, excogitare liceat, ut non ſatis comprehendere valeamus; & multo minus tantarum computationum onus ſuſtinere, quod iſta requirerent.

Attamen, ne nimium dixiſſe videar, inverſa de tangentibus problemata ſunt in poteſtate; aliaque illis difficiliora. Ad quæ ſolvenda uſus ſum duplici methodo; unâ concinniori, alterâ generaliori. Utramque viſum eſt impræſentia literis tranſpoſitis conſignare; ne, propter alios idem obtinentes, inſtitutum in aliquibus mutare cogerer. † *5accdæ10effh12i4l3m10n6oqqr7s11t10v3x: 11ab3cdd10eæg10ill4m7n6o3p3q6r5s11t7vx, 3acæ4egh6i4l4m5n8oq4r3s6t4v, aaddæeeeeeiiimmnnooprrrsssstuu.*

Inverſum hoc problema de tangentibus, quando tangens inter punctum contactûs & axem figuræ eſt datæ longitudinis, non indiget his methodis. Eſt tamen Curva illa mechanica, cujus determinatio pendet ab areâ hyperbolæ.

Ejuſdem generis eſt etiam problema, quando pars axis, inter tangentem & ordinatim applicatam, datur longitudine.

nomiis, adeoque ex æquationibus quibuſcunque non affectis, in ſerie infinitâ; & momentum fluentis ſimul prodit, quo evaneſcente, ſeries in æquationem finitam redit. Per ſecundam, extrahitur fluens ex æquationibus affectis fluxionem non involventibus. Per tertiam, extrahitur fluens ex æquationibus affectis fluxionem ſimul involventibus. Per quartam, eruitur fluens ex conditionibus problematis. Regulæ duæ primæ in principio epiſtolæ ſuperioris, duæ ultimæ in fine hujus ponuntur. Harum regularum Newtonum eſſe inventorem primum, nemo dubitat.

Sed

Sed hos caſus vix numeraverim inter ludos naturæ. Nam quando in triangulo rectangulo, quod ab illâ axis parte, & tangente, ac ordinatim applicatâ conſtituitur, relatio duorum quorumlibet laterum, per æquationem quamlibet, definitur; problema ſolvi poteſt abſque meâ methodo generali: ſed ubi pars axis, ad punctum aliquod poſitione datum terminata, ingreditur vinculum; tunc res aliter ſe habere ſolet.

Communicatio reſolutionis affectarum æquationum, per methodum Leibnitii, pergrata erit; juxta & explicatio quomodo ſe gerat, ubi indices poteſtatum ſunt fractiones; ut in hâc æquatione: $20+x^{\frac{3}{7}}-x^{\frac{2}{3}}-y^{\frac{7}{11}}=0$, aut ſurdæ * quantitates; ut in hâc, $\overline{x^{\sqrt{2}}+x^{\sqrt{7}}}\,^{\sqrt{\frac{2}{3}}}=y$: ubi $\sqrt{2}$ & $\sqrt{7}$ non deſignant coefficientes ipſius x, ſed indices poteſtatum, ſeu dignitatum ejus; & $\sqrt[3]{\frac{2}{3}}$ indicem dignitatis binomii $x^{\sqrt{2}}+x^{\sqrt{7}}$. Res, eredo, meâ methodo patet; aliter deſcripſiſſem.

Sed meta tandem prolixæ huic epiſtolæ ponenda eſt. Literæ ſane excellentiſſimi Leibnitii valdè dignæ erant, quibus fuſius hoc reſponſum darem. Et volui hâc vice copioſior eſſe, quia credidi amœniora tua negotia, ſeveriori hoc ſcribendi genere, non debere à me crebro interpellari.

N° LXV. *Excerpta ex epiſtolâ* D. Collins *ad* D. Newtonum, Londini, 5 Martii 1677 *datâ. Integra autem extat impreſſa in tomo tertio Operum* D. Walliſii *pag.* 646, *&c.*

Clariſſime Vir,

Aderat hîc D. Leibnitius per unam ſeptimanam, in menſe Octobris; in reditu ſuo ad ducem Hanoveræ; cujus literis revocatus erat, in ordine ad quandam promotionem.

Dixit Leibnitius, ſe poſſe & velle conſilia impertire, pro obtinendis ſeriebus, abſque ſpecioſâ extractione radicum æquationum affectarum; modò quis velit laborem illum obire.

Et conſequenter ad hoc, poſtquam ego D. Bakerum ipſi nominaveram, literis ejus ad D. Oldenburgium, datis Amſtelodami, $\frac{18}{28}$ Novemb. 1676, hæc ſcribit.

‘ D. Collinio hæc quæſo communica. Dixit ille mihi D. Bakerum, ‘ doctum admodum & induſtrium apud vos analyticum, utilibus conſiliis ‘ exequendis parem eſſe. Elegi ego unum præ reliquis utile & facile. ‘ Nimirum, methodus tangentium, à Sluſio publicata, nondum rei faſtigi- ‘ um tenet. Poteſt aliquid amplius præſtari in eo genere; quod maximi ‘ foret uſûs ad omnis generis problemata: etiam ad meam (ſine extractio- ‘ nibus) æquationum ad ſeries reductionem. Nimirum, poſſet brevis quæ- ‘ dam calculari circa tangentes tabula, eouſque continuanda, donec pro- ‘ greſſio tabulæ apparet; ut eam ſcilicet quiſque, quouſque libuerit, ſine ‘ calculo continuare poſſit.

* Surdos indices D. Leibnitius in epiſtolâ ſequente mutavit in fluentes; & inde natus eſt calculus exponentialis.

‘ Amſtelodami

‘ Amſtelodami cum Huddenio locutus ſum; cui negotia civilia tempus ‘ omne eripiunt. Eſt enim ex numero duodecem urbis Conſulum, qui ‘ ſubinde imperium obtinent: nuper Conſul regens erat; nunc Theſaurarii ‘ munus exercet. Præclara admodum in ejus ſchedis ſupereſſe, certum eſt. ‘ Methodus tangentium, à Sluſio publicata, dudum illi fuit nota. Amplior ‘ ejus methodus eſt, quàm quæ à Sluſio fuit publicata. Sed & quadratura ‘ hyperbolæ Mercatoris ipſi, jam anno 1662, innotuit. Hactenus Leib- ‘ nitius.’

P. S. Exemplar epiſtolæ tuæ, quatuor ſchedarum, nondum eſt ad D. Leibnitium miſſum: ſed, intra ſeptimanam, eſt quidam hinc profecturus Hanoveram, qui tum illud, tum libros quoſdam laturus eſt.

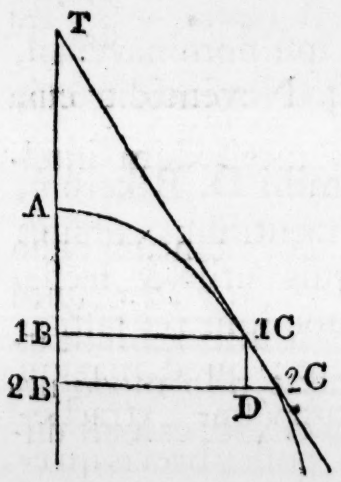
Epiſtola D. Leibnitii *ad D.* Oldenburgum, 21 Junii 1677, *data cum D.* Newtono *communicanda. Cujus extat & autographum, & exemplar manu D.* Collins *deſcriptum.* N° LXVI.

Ampliſſime Domine,

Accepi literas tuas diu expectatas, cum incluſis Newtonianis ſane pulcherrimis; quas plus ſemel legam cum curâ & meditatione; quibus certè non minus dignæ ſunt, quàm indigent. Nunc pauca, quæ feſtinante oculo obeunti incidere, è veſtigio annotabo.

Egregiè placet, quòd deſcripſit, quâ viâ in nonnulla ſua elegantia ſane theoremata inciderit. Et quæ de Walliſianis interpolationibus habet, vel ideo placent, quia hâc ratione obtinetur harum interpolationum demonſtratio; cùm res ea antea (quod ſciam) ſolâ inductione niteretur; tametſi pars eorum per tangentes ſit demonſtrata.

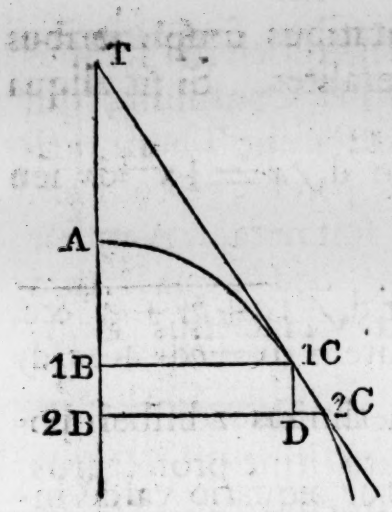
Clariſſimi Sluſii methodum tangentium nondum eſſe abſolutam, celeberrimo Newtono aſſentior. Et jam à multo tempore † rem tangentium longè generaliùs tractavi; ſcilicet per differentias ordinatarum. Nempe TIB intervallum tangentis ab ordinatâ in axe ſumptum, eſt ad IBIC ordinatam; ut ICD, differentia duarum abſciſſarum AIB, A2B, ad D2C, differentiam duarum ordinatarum IBIC, 2B2C. Nec refert, quem angulum faciunt ordinatæ ad axem. Unde patet, nihil aliud eſſe invenire tangentes, quàm invenire differentias ordinatarum, poſitis differentiis abſciſſarum (ſeu IB2B = ICD) ſi placet æqualibus. Hinc nominando ‡ in poſterum dy differentiam duarum proximarum y (nempe AIB & A2B); & dx, ſeu D2C, differentiam duarum proximarum x (prioris IBIC, poſterioris 2B2C); patet dy^2 eſſe $2ydy$; & dy^3 eſſe $3y^2dy$, &c. & ita porro. Nam ſint duæ proximæ



† Idem fecit D. Barrow in ejus Lect. 10, anno 1669 impreſſâ, idque calculo conſimili.

‡ Cœpit igitur D. Leibnitius, hoc ipſo tempore, methodum differentialem cum amicis ſcripto communicare; lectis priùs, quæ Newtonus de hâc methodo in duabus epiſtolis ſcripſerat; lectis fortean & aliis Newtonianis, ſub finem anni 1676, ubi domum per Londinum redibat; quo tempore prælectiones Barrovii ſecum tulit.

ſibi



ſibi (id eſt, differentiam habentes infinitè parvam) ſcilicet AIB $= y$; & A2B $= y + dy$. Quoniam ponimus dy^2 eſſe differentiam quadratorum ab his duabus rectis, æquatio erit $dy^2 = y^2 + 2ydy + dydy - y^2$. Seu omiſſis $y^2 - y^2$, quæ ſe deſtruunt; item omiſſo quadrato quantitatis infinitè parvæ, ob rationes ex methodo de Maximis & Minimis notas; erit $dy^2 = 2ydy$. * Idemque eſt de cæteris potentiis. Hinc etiam haberi poſſunt differentiæ quantitatum, ex diverſis indefinitis in ſe invicem ductis factarum: ut dyx erit $= ydx + xdy$; & $dy^2x = 2xydy + y^2dx$. Hinc ſi æquatio $a + by + cx + dyx + ey^2 + fx^2 + gy^2x + hyx^2$ &c. $= 0$; ſtatim habetur tangens Curvæ, ad quam eſt iſta æquatio. Nam ponendo AB $= y$, & A2B $= y + dy$ (ſcilicet, quia 1B2B ſeu 1CD $= dy$) itemque ponendo 1B1C $= x$, & 2B2C $= x + dx$ (ſcilicet quia 2CD $= dx$): et, quia eadem æquatio exprimit quoque relationem inter A2B & 2B2C, quæ eam exprimebat inter AIB & 1B1C; † tunc in æquatione illâ pro y & x ſubſtituendo $y + dy$, & $x + dx$, fiet

$$\left.\begin{array}{rrrrrrrr} a+by & +cx & +dyx & +ep^2 & +fx^2 & +gy^2x & +hyx^2 & \text{\&c.} \\ \hline bdy+cdx & +dydx & +2eydy & +2fxdx & +2gxydy & +2hxydx & \text{\&c.} \\ & +dxdy & & & +gy^2dx & +hx^2dy & \text{\&c.} \\ \hline & +ddxdy & +edydy & +fdxdx & +gxdydy & +hydxdx & & \\ & & & & +2gydydx & +2hxdxdy & \text{\&c.} \\ & & & & +gdxdydy & +hdydxdx & \text{\&c.} \end{array}\right\} = 0.$$

d eſt quantitas communi more.
d eſt nota differentiæ.

Ubi, abjectis illis quæ ſunt ſupra primam lineam, quippe nihilo æqualibus per æquationem præcedentem; & abjectis illis quæ ſunt infra ſecundam, quia in illis duæ infinitè parvæ in ſe invicem ducuntur; hinc reſtabit tantùm æquatio hæc, $bdy + cdx + dydx + dxdy = 0$; quicquid ſcilicet reperitur inter lineam primam & ſecundam. Et, mutatâ æquatione in rationem ſeu analogiam, fiet $-\frac{dy}{dx} = \frac{c + dy + 2fx + gy^2 + 2hxy \text{ \&c.}}{b + dx + 2ey + 2gxy + hx^2 \text{ \&c.}}$. Id eſt (quia $-\frac{dy}{dx}$ ſeu $\frac{-1B2B, \text{ ſeu } -1CD}{D2C} = -\frac{T1B}{1B2C}$) erit $\frac{c + dy \text{ \&c.}}{b + dx \text{ \&c.}} = -\frac{T1B}{1B1C}$. Quod coincidit cum regulâ Sluſianâ; oſtenditque eam ſtatim occurrere hanc methodum intelligenti.

Sed methodus ipſa (priore) noſtrâ longe eſt amplior. Non tantùm enim exhiberi poteſt, cùm plures ſunt literæ indeterminatæ quàm y & x (quod ſæpe fit maximo cum fructu); ſed & tunc utilis eſt, cùm interveniunt irrationales; quippe quæ eam nullo morantur modo: neque ullo modo neceſſe eſt irrationales tolli, quod in methodo Sluſii neceſſe eſt, & calculi difficultatem in immenſum auget.

* Id eſt, ſi ſecundus terminus binomii ſit differentia primi termini; ſecundus terminus potentiæ binomii erit differentia potentiæ. Hoc eſt fundamentum methodi differentialis à Leibnitio jam poſitum. Et hoc idem fundamentum methodi ſuæ Newtonus anno 1669 poſuerat in analyſi ſuprà impreſſâ, N° XII. Perſimilibus calculis, Newtonus momenta, & Leibnitius differentias collegerunt; & diſcrepant ſolùm in rerum nominibus.

Quod

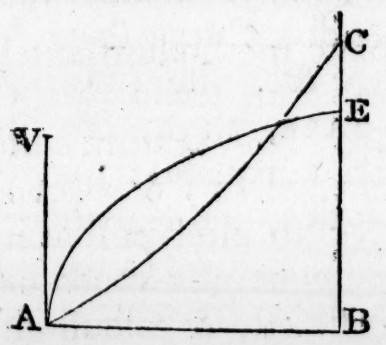
Quod ut appareat, tantùm utile erit in irrationalitatibus ſimplicioribus rem explanare. Et primum ſit in ſimpliciſſimis generaliter. Si ſit aliqua potentia aut radix x^z; erit $dx^z = zx^{z-1}dx$.

Si z ſit $\frac{1}{2}$, ſeu ſi x^z ſit $\sqrt{x}$, erit dx^z, ſeu hoc loco $d\sqrt{x} = \frac{1}{2}x^{-\frac{1}{2}}dx$ ſeu $\frac{dx}{2\sqrt{x}}$; ut notum, aut facilè demonſtrabile.

Sit jam binomium; ut $\sqrt[3]{a+by+cy^2}$ &c: quæritur $d\sqrt[3]{a+by+cy^2}$ &c. ſeu dx^z, poſito $\frac{1}{3} = z$, & $a+by+cy^2$ &c. $= x$. Eſt autem $dx = bdy + 2cydy$ &c. Ergo dx^z, ſeu $\frac{dx}{3x^{\frac{2}{3}}} = \frac{bdy+2cydy\ \&c.}{3\times a+by+cy^2\ \&c\ ^{\frac{2}{3}}}$. Eadem methodus adhiberi poteſt, etſi radices in radicibus implicentur. Hinc ſi detur æquatio valdè incricata, ut $a+bx\sqrt{y^2+b\sqrt[3]{1+y}}+hx^2y\sqrt{y^2+y\sqrt{1-y}} = 0$, ad aliquam Curvam cujus abſciſſa ſit y (AB) ordinata x (BC); tunc æquatio proveniens utilis ad inveniendam tangentem TC, ſtatim ſine calculo ſcribi poterit; & erit hæc $bdx\sqrt{y^2+b\sqrt[3]{1+y}} + \frac{bx}{2\sqrt{y^2+b\sqrt[3]{1+y}}}\times 2ydy + \frac{bdy}{3\times 1+y^{\frac{2}{3}}} + \sqrt{hx^2dy+2hxydx}\times\sqrt{y^2+y\sqrt{1-y}} + \frac{hyx^2}{2\sqrt{y^2+y\sqrt{1-y}}}\times 2ydy + dy\sqrt{1-y} - \frac{ydy}{2\sqrt{1-y}} = 0$. Seu, mutando quotientem hanc inventam in analogiam, erit $-dy$ ad dx, ſeu TIB ad IBIC, ut omnes provenientis æquationis termini, per dx multiplicati, ad omnes ejuſdem terminos per dy multiplicatos.

Ubi ſane mirum & maximè commodum evenit, quòd dy & dx ſemper extant extra vinculum irrationale. Methodo autem Sluſianâ omnes ordine irrationales tollendas eſſe, nemo non videt.

Arbitror, quæ celare voluit Newtonus de tangentibus ducendis, ab his non abludere. Quod addit, ex hoc eodem fundamento † quadraturas quoque reddi faciliores, me in ſententiâ hâc confirmat. Nimirum ſemper figuræ illæ ſunt quadrabiles, quæ ſunt ad æquationem differentialem. Æquationem differentialem voco talem, quâ valor ipſius dx exprimitur, quæque ex aliâ derivata eſt, quâ valor ipſius x exprimebatur. Exempli gratiâ: ſit AB $= y$. EB $= x$ ponatur $\frac{b+cy+dy^2+ey^3\ \&c.}{2\sqrt{1+by+\frac{1}{2}cy^2+\frac{1}{3}dy^3+\frac{1}{4}ey^4\ \&c.}}$. Quæritur quadratura figuræ ABEA; quanquam fortè ſæpe tale trilineum non ſit proditurum, quale hoc ſchemate depinximus. Deſcribatur alia Curva AC, talis ut BC [quæ] ſit $\sqrt{1+by+\frac{1}{2}cy^2+\frac{1}{3}dy^3+\frac{1}{4}ey^4}$ &c. [ipſius ordinatam] ſignificet; & rectangulum ſub rectâ AV, repræſentante unitatem conſtructionis, & ſub ordinatâ novâ BC, æquabitur figuræ ABEA. Ejuſmodi theoremata condi poſſunt infinita: imo pleraque ſub generaliſſimis



† Calculus etiam, in his exemplis allatus, à calculo Newtoniano in ſolis notarum formulis differt; ſed notis minùs aptis obſcurior redditur.

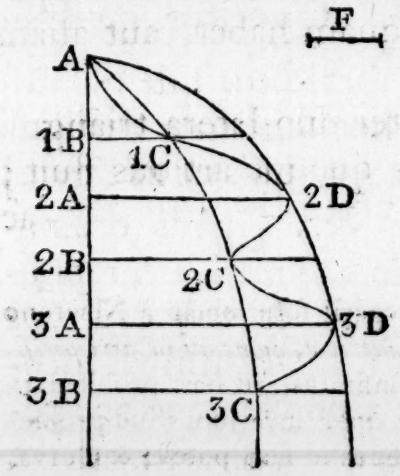
‡ Characteres methodi Newtoni Leibnitius hîc enumerat; & gaudet, ſe in methodum incidiſſe, cui characteres hi omnes competunt. Fatetur etiam Newtonum, intellexiſſe facilem quadraturam figurarum, quæ ſunt ad æquationem differentialem. Vel doceat, methodum aliam in rerum naturâ extare, cui characteres hi omnes competunt, vel deſinat negare, ſe in methodum Newtoni incidiſſe.

neraliſſimis quibuſdam complecti. Licet nihil refert, ſive ſeries hæ producantur, ſive ubilibet finiantur. Unde patet hanc unicam regulam pro infinitis figuris quadrandis inſervire, diverſæ plane naturæ ab iis, quæ hactenus quadrari ſolebant.

Pulcherrimæ ſunt illæ ſeries Newtonianæ, quæ ex infinitis in finitas degenerant; qualis illa eſt, quam exhibet pro extractione radicum binomii, aut ejus quadraturâ. Quòd ſi in ipſius generali illâ æquationis affectæ indefinitæ extractione, cùm ſit $x = ay + by^2 + cy^3$ &c. & y ſit $\frac{z}{a} - \frac{bz^2}{a^3}$ &c. vel $y = \frac{z}{a} - \frac{bz^3}{a^4}$ &c: idem præſtari poſſet; ut ſcilicet, inter extrahendum radices ex æquationibus aut binomiis, invenire liceret radices rationales finitas, quando eæ inſunt, vel etiam irrationales: tunc dicerem, methodum ſerierum infinitarum ad ſummam perfectionem eſſe perductam.

Opus eſſet tamen præterea, diſcerni poſſe varias æquationis ejuſmodi radices: item neceſſe eſſet, ope ſerierum, diſcerni æquationes poſſibiles ab impoſſibilibus. Quòd ſi hæc nobis obtinuerit vir in his ſtudiis maximus, atque effecerit ſcilicet, ut poſſimus ſeriem infinitam convertere in finitam, quando id fieri poteſt, aut ſaltem agnoſcere ex quânam finitâ ſit deducta: tunc in methodo ſerierum infinitarum, quæ diviſione & extractione inveniuntur, vix quicquam amplius optandum reſtabit. Hæc, ſi quiſquam mortalium, certè Newtonus præſtare poterit. Eâdem credo operâ efficietur, ut, ex multis ſeriebus infinitis, poſſimus deligere maximè naturales; quales haud dubiè illæ erunt, quæ ita erunt comparatæ, ut, cùm fieri poteſt, atque opus eſt, degenerent in finitas. Atque ita egregiè apparebit, methodum extractionum per ſeries infinitas minimè indirectam, ſed maximè naturalem eſſe.

Problema eſt perelegans, cujus meminit, Curvam deſcribere, quæ per data quæcunque tranſeat puncta. Huddenius mihi Amſtelodami dixit, poſſe ſe Curvam deſcribere analyticam, ſeu certâ æquatione uniformi conſtantem, quæ faciei hominis cujuſdam noti lineamenta deſignet.



Cæterùm quærendum eſt, an hoc Newtonus intelligat de punctis infinitis; ut ſi ſit axis A1B2A2B3A &c. in infinitum productus; & duæ Curvæ datæ infinitæ analyticæ, una A1C2C3C &c. altera A2D3D &c; ſi ponamus A1B, 1B2A, 2A2B, 2B3A, &c. inter ſe & datæ cuidam quantitati F æquales: quæritur, an dari poſſit Curva analytica, ſeu æquationis capax, quæ, in infinitum producta, tranſeat (alternis) per puncta 1C, 2D, 2C, 3D, 3C, &c. Fermatius alicubi ſcribit, ſe methodum habere, per quam Curva inveniri poſſit, cujus proprietas ſpecifica data non pertineat

* Vid pag. 519, lin. 36, & pag. 522, lin. 14 & ſeq.

† Dixerat Newtonus, analyſin, beneficio æquationum infinitarum, ad omnia pene problemata ſeſe extendere (pag. 530, lin. 24.) Reſpondit Leibnitius, multa eſſe problemata uſque adeo mira & implexa, ut neque ab æquationibus pendeant, neque à quadraturis; qualia ſunt problemata methodi tangentium inverſæ, &c. pag. 538, 539. Reſcripſit Newtonus, *inverſa de tangentibus problemata eſſe in poteſtate, aliaque illis difficiliora; ad quæ ſolvenda ſe uſum eſſe duplici methodo*, &c. pag. 557.

pertineat ad unum punctum, ut vulgo fit; cùm ordinatæ referuntur ad partes axis; ſed ad duo quælibet ſimul, vel etiam ad tria quælibet ſimul, &c.

* Quæ de variis ſeriebus ſuis & noſtris examinandis, atque inter ſe comparandis, dicit Clariſſimus Newtonus; in ea me immergere non audeo, antequam in gratiam cum analyſi rediero: nam harum rerum veſtigia in animo meo prope non obliterata ſunt. Agnoſco interim, pulcherrima & utiliſſima ab eo annotari. Elegantiſſima & minimè expectata eſt via, quâ ſeriem meam $\frac{1}{1}t - \frac{1}{3}t^3 + \frac{1}{5}t^5$ &c. deduxit ex ſuâ.

Quod ait, problemata methodi tangentium inverſæ, eſſe in poteſtate; hoc arbitror ab eo intelligi per ſeries ſcilicet infinitas. † Sed à me ita deſiderantur, ut Curvæ exhibeantur geometricè quatenus id fieri poteſt, ſuppoſitis (minimum) quadraturis. Exempli cauſâ. Cycloidem deprehendit Hugenius ſui ipſius evolutione deſcribi: difficile autem fuiſſet, credo, ſolvere hoc problema, invenire Curvam, quæ ſui ipſius evolutione deſcribitur. Neque refert, quòd curvæ deſcriptio quadraturam circuli ſupponit: et hoc problema etiam ex eorum eſt numero, quæ voco methodi tangentium inverſæ. Ita inter methodos tangentium inverſas generales eſt, invenire Curvam analyticam, cujus longitudines ſint areis datæ figuræ, curvâ analytica comprehenſæ, proportionales. Contrarium enim dudum poſſumus. Quod problema arbitror non eſſe inſolubile, & videtur non contemnendum: facilius enim eſt lineam, quàm ſpatium, organicè metiri. Et, reductâ ſpatiorum dimenſione ad dimenſionem linearum, ſolis filis in rectum extenſis mechanica fieri poterit conſtructio; & ſpatia poterunt in datâ ratione ſecari, inſtar linearum rectarum.

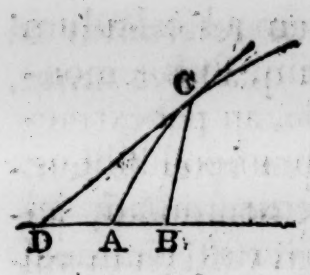
Cùm ait Newtonus, inveſtigationem Curvæ, quando tangens, vel inter- N° LXVIII.
vallum tangentis & ordinatæ in axe ſumptum, eſt recta conſtans, non indigere his methodis: innuit credo, ſe intelligere methodum tangentium inverſam generalem in poteſtate eſſe, per methodos ſerierum appropinquativas; in hoc vero caſu ſpeciali ‡ non opus eſſe ſeriebus. Ego vero methodum quærebam, quæ accuratè Curvam quæſitam exhibeat, ſaltem ex ſuppoſitis quadraturis; & cujus ope ejus æquationem, ſi quam habet, aut aliam primariam proprietatem poſſumus invenire.

Quod ait, problemata, in quibus datur relatio inter duo latera trianguli DBC, ſemper poſſe ſolvi: id verum eſt; at ex § meis quoque artibus fluit; ac

557. Leibnitius vero, ne quid à Newtono jam didiciſſe videretur, regerit ſolutionem à Newtono *intelligi per ſeries infinitas; ſed à ſe ita deſiderari, ut Curvæ exhibeantur geometricæ, quatenus id fieri poteſt.* In priore epiſtolâ negaverat analyſin Newtonianam, per æquationes infinitas, ad hæc problemata extendi. Jam negat ſe negâſſe; & verbis prioribus nubem obducit, quaſi inverſum illud problema ſuo ſenſu non ſolveretur, niſi Curvæ exhibeantur geometricè quatenus id fieri poteſt, & Curva, quæ ſui ipſius evolutione deſcribitur, inveniri poſſit per eandem ſolutionem.

‡ Hoc non dixit Newtonus, ſed perſpicuè dixit problema in hoc caſu non indigere methodis duabus generalibus, quas literis tranſpoſitis celaverat. Vide pag. 557.

§ Per artes ſuas intelligit methodum differentialem; ut patet ex calculis, quos ſubjungit. Ubi epiſtolam priorem ſcribebat, problema de Curvâ inveniendâ, in quâ intervallum tangentis & ordi-



ac ſæpe, ne quadraturis quidem accitis, ſimplici analyticâ æquatione præſtari poteſt. Ut, ſi BC poſitâ x, ſit DB $= bx + cx^2 + dx^3$; quæritur, qualiſnam ſit hæc Curva, quæ hanc tangentium habeat proprietatem: id eſt, quænam ſit æquatio relationem exprimens inter AB ſeu y, & BC ſeu x. Aio eam fore $y = bx + \frac{1}{2}cx^2 + \frac{1}{3}dx^3$. Si fuiſſet DB $= a + bx + cx^2$, opus fuiſſet quadraturâ hyperbolæ ad inveniendam Curvam quæſitam. Generaliter autem quomodocunque datur relatio inter duo ex lateribus trianguli (quod ego *Characteriſticum*, ob crebros uſus, vocare ſoleo) ſemper, ſuppoſitis quadraturis figurarum analyticarum, haberi poteſt Curva quæſita. Quod tamen neſcio, an præter Newtonum præſtiturus ſit quiſquam.

Meâ methodo, res unius lineolæ calculo peragitur, ac demonſtratur. Sed & rem infinitis caſibus præſtare poſſum, tametſi ipſa y, ſeu AB, ingrediatur in ipſius DB expreſſionem. Ut, ſi ſit DB $= bx + cx^2 + dx^3 + y$, fiet æquatio Curvæ $yx = bx + \frac{1}{2}cx + \frac{1}{3}dx^2$. [*Forte legendum*, DB $= b + cx + dx^2 - y$, *fiet æquatio Curvæ*, $yx = bx + \frac{1}{2}cx^2 + \frac{1}{3}dx^3$.] Itaque ſi habeatur valor ipſius DA, ex BC haberi poterit Curva.

Quod verò ait Cl. Newtonus *, non æquè rem procedere, ſi detur relatio ipſius DB ad partem axis, ſeu ad AB vel y, ad hoc reſpondeo; mihi æquè facile eſſe, invenire Curvæ naturam vel æquationem, ſi detur relatio ipſius DB ad AB, quàm ſi, ut ipſe requirit, detur relatio ad BC. Generalem verò methodum tangentium inverſam nondum, quod ſciam, habemus.

N° LXIX. Sunt & alia problematum genera, quæ hactenus in poteſtate non habeo; quorum ecce exempla. Sint duæ æquationes $x^y + y^x = xy$, & $x^x - y^y = x + y$. Duæ ſunt incognitæ x, y, duæque ad eas inveniendas æquationes; quæritur valor tam unius quàm alterius literæ. Talia problemata vel in numeris vel in lineis ſolvere, difficillimum arbitror; ſi tamen de appropinquationibus agatur, puto poſſe iis ſatisfieri. Si quam huic difficultati lucem afferre poteſt Newtonus, pro eâ quâ pollet ingenii vi, multum analyſim promovebit.

Analyſis quoque Diophantæa, ſeu ſolutio problematum in numeris rationalibus, nondum perfectionem nacta eſt.

Hæc annotavi feſtinans, atque inter legendum; ad reliqua majore otio opus eſt: interea celeberrimum Newtonum quæſo officioſiſſimè à me ſaluta, &, poſt actas maximas gratias, eum roga, ut communicet continuationem harum ſerierum; nempe poſita $z = ay + by^2 + cy^3 + dy^4$ &c. ait fore $y = \frac{z}{a} - \frac{bz^2}{a^3} + \frac{2b^2 - ac}{a^5}z^3$ &c. vel $y = \frac{z}{a} - \frac{bz^3}{a^4} + \frac{3b^2 - ac}{a^7}z^5$ &c. Et ſi qua alia in

natæ, in axe ſumptum, ſit recta conſtans, vocabat ludum naturæ; & ejuſmodi problemata, mira & implexa, ab æquationibus pendere noluit. Reſpondebat Newtonus, hoc problema non eſſe ludum naturæ; ſed ubi datur relatio quævis inter ordinatam, & tangentem, & intervallum utriuſquc, in axe ſumptum, ſemper poſſe ſolvi, idque abſque ſuâ methodo generali; nempe per fluxionum methodum ſimplicem, & quadraturam Curvarum. Jam reſcribit Leibnitius, *id verum eſſe, at ex ejus quoque artibus fluere* (id eſt ejuſmodi problemata ab æquationibus ſuis pendere) & triangulum DBC, *ab crebros uſus, characteriſticum vocat*, quaſi hæc ipſi dudum innotuiſſent. Hujuſmodi problemata ab æquationibus non pendere, anno ſuperiore ſcripſit: jam fluunt horum ſolutiones ex ejus artibus,

in promptu habet theoremata * nonnihil generalia; quoniam ad calculum contrahendum plurimum ſerviunt: quòd ſi eorum originem, ſive demonſtrationem, addet, tanto magis obligabit. Velim etiam noſſe, an per extractiones in ſeriebus diſcernere poſſit æquationes poſſibiles ab impoſſibilibus. Nam ſi generalis ejuſmodi extractio procederet, ſequeretur nullam æquationem fore impoſſibilem: item quomodo inveniat diverſas ejuſmodi æquationis radices, ita ut ex pluribus radicibus eam poſſit invenire, quam quærimus: item an tales habeat ſeries, quarum ope, extrahendo, æquationis inveniuntur valores finiti, quando tales inſunt æquatione: denique quid ſentiat de reſolutione æquationum, quales paulo ante poſui; ut $x^y + y^x = xy$, & $x^x + y^y = x + y$; ubi ſcilicet incognita ingreditur in exponentem.

Oblitus eram dicere, pulchram mihi videri Ciſſoidis extenſionem in rectam, quam Newtonus invenit, ex ſuppoſitâ quadraturâ hyperbolæ. Ego mihi videor, eodem modo etiam metiri poſſe Curvam hyperbolæ æquilateræ, ſed nondum omnis; neque Curvam ellipſeos, quantum memini.

Antequam finiam, adjiciam uſum pulcherrimum ſierum, qui imprimis Collinio noſtro non erit ingratus. Scis magnam eſſe difficultatem circa extrahendas radices ex binomiis cubicis, quando eas ingreditur quantitas imaginaria, orta ex radice quadraticâ negativæ quantitatis; ut $\sqrt[3]{:a+\sqrt{-bb}} = M + \sqrt[3]{:a-\sqrt{-bb}} = N$: ubi utraque quantitas, M & N, eſt ſingulatim impoſſibilis; ſumma autem, ut alibi oſtendi, † eſt quantitas poſſibilis & realis, æqualis cuidam quæſitæ z. Ut vero ea eximatur, & ut extrahatur radix, nempe ut inveniatur $\frac{1}{2}z + e\sqrt{-bb} = \sqrt[3]{:a+\sqrt{-bb}}$; & $\frac{1}{2}z - e\sqrt{-bb} = \sqrt[3]{:a-\sqrt{-bb}}$ (unde fit $\sqrt[3]{:a+\sqrt{-bb}} + \sqrt[3]{:a-\sqrt{-bb}} = z$) non poteſt adhiberi methodus Schotenii, geometriæ Carteſianæ ſubjecta; quia opus eſt ad eam, ut valor ipſius $\sqrt[3]{:a+\sqrt{-bb}}$ exhibeatur ſaltem approximando, quod notis methodis impoſſibile eſt. Quis enim valorem ipſius $\sqrt{-bb}$ prope verum dabit? neceſſe eſt enim invenire $b\sqrt{-1}$; quis autem exprimat $\sqrt{-1}$ appropinquando? Scripſi olim Collinio, me remedium inveniſſe; quod etiam ad omnes gradus ſuperiores valeat: id ecce hîc uno verbo. Ex binomio $\sqrt[3]{:a+\sqrt{-bb}}$ extraho radicem per ſeriem infinitam, ſive per theorema Newtonianum, ſive etiam more meo priore, inſtituendo calculum ſecundum naturam cujuſque gradûs, cùm ſcilicet nondum theorema generale abſtraxiſſem: quæ radix ponatur eſſe $l + m\sqrt{-bb} + n + p\sqrt{-bb}$ &c. Extrahatur jam & radix ex binomio altero, $\sqrt[3]{:a-\sqrt{-bb}}$: fiet illa $+l - m\sqrt{-bb} + n - p\sqrt{-bb}$ &c. ut facilè demonſtrari poteſt ex calculo: ergo ‡ addendo hæc duo extracta, deſtruentur imaginariæ quantitates, &

bus, ac ſæpe ne quadraturis quidem accitis; ſimplici analyticâ æquatione (differentiali ſcilicet) peraguntur.

* Dixerat Newtonus, quòd ubi *relatio duorum quorumlibet laterum trianguli definiretur per æquationem, problema ſolvi poteſt abſque generali ejus methodo, quam literis tranſpoſitis celaverat; ſed ubi pars axis, vel abſciſſæ, ingreditur vinculum, res aliter ſe habere ſolet*; id eſt, indiget ejus methodo generali. præterquam in particularibus quibuſdam. Leibnitius, ad particularia illa alludens, *ſibi æquè facile eſſe ait invenire Curvæ naturam, vel æquationem, in utroque caſu*; quibus verbis manifeſtum, eſt ſolutionem generalem ei nondum innotuiſſe.

† Summa eſt quantitas triplex poſſibilis, ideoque non niſi tripliciter exhiberi poteſt.

‡ Examinanda eſt hæc methodus.

fiet

fiet $z = 2l + 2n$ &c. quæ ſunt eæ ſeriei portiones, in quibus nulla reperitur imaginaria. Invento ergo valore ipſius z, quantum ſatis eſt propinquo, quemadmodum Schotenius poſtulat, reliqua methodo Schotenianâ, perinde ac in illis binomiorum extrahendorum generibus, tranſigentur.

Junii 21, 1677.

N° LXX. *Epiſtola D.* Leibnitii *ad D.* Oldenburgum, 12 Julii 1677 *datâ, cum D.* Newtono *communicanda. Hujus extat exemplar manu D.* Collins *deſcriptum, & impreſſa eſt à D.* Walliſio *pag.* 652.

Ampliſſime Domine,

Nuperas meas credo acceperis; nunc iſtas maturè ſummitto, ne facilitate D. Newtoni abutamur. Rogaveram enim in prioribus, ut quædam ſuæ epiſtolæ loca explicaret; nempe quomodo inveniſſet theoremata, quòd poſito $z = ay + by^2 + cy^3$ &c. ſit $y = \frac{z}{a} - \frac{bz^2}{a^3} + \frac{2b^2 - ac}{a^5} z^3$ &c. vel $y = \frac{z}{a} - \frac{b^3}{a^4} + \frac{3b^2 - ac}{a^7} z^5$ &c. Nunc verò, relectis ejus literis, video id facile non tantùm ex ejus extractionibus derivari, ſed & alterâ illâ methodo, ſub finem literarum ejus expoſitâ inveniri; quâ me quoque * aliquando uſum in veteribus meis ſchedis reperio; ſed cùm in exemplo, quod fortè in manus meas ſumſeram, nihil prodiiſſet elegans, ſolitâ impatentiâ eam porro adhibere neglexiſſe.

Difficultatem moveram, in præcedentibus literis, circa æquationes impoſſibiles; quarum radices poſſibiles videntur inveniri per ſeries infinitas; necdum verò illa ſublata eſt, & meretur res excuti diligentius. Illud tamen video, ſi in æquatione datâ, $z = ay + by^2 + cy^3$ &c. literæ z & y ſint indeterminatæ, tunc æquationem ſemper eſſe poſſibilem; ſed ſi z eſſet determinata, rurſuſque in ipſis a vel b &c. lateret æquatio, poſſet eſſe impoſſibilis, & tamen per ſeriem generalem aliqua prodire videretur radix poſſibilis; cujus difficultatis ſolutionem, re diligenter expenſâ, reperiri poſſe arbitror: ſed nunc in iſta accuratiùs inquirere non licet. Meretur autem explicari, tum quomodo ex ſeriebus agnoſci poſſit æquationes eſſe impoſſibiles (quanquam id aliâs ſatis facilè inveniatur) tum quomodo dignoſcantur diverſæ radices.

Præter ea quæ in ſuperiore epiſtolâ notavi, ſcilicet methodum tangentium inverſam & geometricam (ſaltem ſuppoſitis Curvarum analyticarum quadraturis)

* D. Leibnitius ſeries plures reciprocas ante biennium ab Oldenburgo acceperat: methodum ſerierum reciprocarum anno ſuperiore Newtonum rogaverat: hoc anno acceptam ægrè intellexerat: & intellectam ſe olim inveniſſe, ex chartis ſuis antiquis mox didicit: et quamvis ſeries pro hyperbolâ & circulo ante annos plures haberet, & hæc methodus ex arcu daret ſinum, ex logarithmo daret numerum, & ſerierum omnium exhiberet reciprocas; eandem tamen olim inventam neglexiſſe, ut inutilem. Sic methodum, quam diu deſideraverat, rogaverat, acceperat, & ægrè intellexerat, vel primus, vel ſaltem proprio marte ſcilicet invenit.

† Quod hîc deſideratur, Newtonus, in epiſtolâ ſuâ noviſſimâ, ſignificavit ſe aliquâ ex parte inveniſſe;

quadraturis) & alia id genus, † deeſt nobis circa quadraturas, ut ſcire certo poſſimus, an non quadratura figuræ alicujus propoſitæ reducatur ad quadraturam circuli aut hyperbolæ: nam pleræque figuræ, hactenus tractatæ, ope alterutrius quadrari potuerunt. Quòd ſi demonſtrari poteſt (ut arbitror) quaſdam figuras non eſſe quadrabiles nec per circulum nec hyperbolam; reſtat, ut alias quaſdam figuras primarias altiores conſtituamus, ad quarum quadraturam reducantur cæteræ omnes, quando id fieri poteſt. Hoc quamdiu non fit, hæremus; & ſæpe per ſeriem infinitam particularem quærimus, quod ad circuli aut hyperbolæ, aut aliam notioris figuræ quadraturam reduci poterat. Crediderat Gregorius, dimenſionem Curvarum hyperbolæ & ellipſeos non pendere à quadraturâ circuli aut hyperbolæ; ego verò reperi aliquam ſpeciem Curvæ hyperbolicæ, quam, ex datâ ipſius hyperbolæ quadraturâ, metiri poſſum: de cæteris nondum mihi liquet.

Hannoveræ 12 Julii 1677. Nº LXXI.

Brevi poſtea, autumno ſcilicet anni 1677, mors Oldenburgi huic literarum commercio finem impoſuit. Deinde anno 1682 Collins mortuus eſt, & Acta eruditorum Lipſiæ primùm edita ſunt; ejuſque anni menſe Februario prodiit D. Leibnitii Quadratura Arithmetica, circuli ſcilicet & hyperbolæ; quarum prior non differt à Gregorianâ toties dictâ, neque poſterior ab eâ Vicecomitis Brounkeri, ante quatuordecim annos, in Philoſophicis Tranſactionibus Nº XXXIV. pro menſe Aprilis 1668, publicatâ. Non multo pòſt, anno ſcilicet 1684, in iiſdem actis Lipſicis pro menſe Octobri, Calculi Differentialis elementa primùm edidit D. Leibnitius literis G. G. L. deſignatus. Anno autem 1683 ad finem vergente, D. Newtonus propoſitiones principales earum, quæ in Philoſophiæ Principiis Mathematicis habentur, Londinum miſit, eædemque cum Societate Regiâ mox communicatæ ſunt; annoque 1686 liber ille ad Societatem miſſus eſt ut imprimeretur, proximoque anno menſe Martio lucem vidit: & exemplar ejus D. Nicolao Fatio datum eſt ut ad Leibnitium mitteretur. Deinde anno 1688 epitome ejus in Actis Lipſicis impreſſa eſt: quâ lectâ D. Leibnitius epiſtolam de lineis Opticis, ſchediaſma de reſiſtentia Medii, & motu Projectilium gravium in Medio reſiſtente, & tentamen de Motuum Cæleſtium cauſis compoſuit, & in Actis Lipſicis, ineunte anno 1689, imprimi curavit; quaſi ‡ ipſe quoque præcipuas Newtoni de his rebus propoſitiones inveniſſet, idque diverſâ methodo, quâ vias novas geometricas aperuiſſet; & librum Newtoni tamen nondum vidiſſet.

niſſe; & quod invenerat, poſtea publicavit in libro de Quadraturâ Curvarum.

‡ Hâc licentiâ conceſſâ authores quilibet inventis ſuis facilè privari poſſunt. Viderat Leibnitius epitomen libri in Actis Lipſicis. Per commercium epiſtolicum, quod cum viris doctis paſſim habebat, cognoſcere potuit propoſitiones in libro illo contentas. Si librum non vidiſſet, videre tamen debuiſſet, antequam ſuas, de iiſdem rebus, in itinere ſcriptas compoſitiones publicaret. Dicunt aliqui, falſas eſſe Tentaminis Propoſitiones XI, XII & XV; & D. Leibnitium ab his, per calculum ſuum, deduxiſſe Propoſitiones XIX & XX ejuſdem tentaminis. Talis autem calculus ad propoſitiones priùs inventas aptari quidem potuit, non autem inventorem conſtituere.

Anno

N° LXXII. Anno autem 1695 Opera Mathematica celeberrimi Walliſii duobus tomis Oxonii prodiere: & in Actis Eruditorum anni inſequentis menſe Junio, habetur libri Epitome; in quâ ſequentia leguntur, pag. 257 & ſeq.

Newtonianis etiam ſeriebus jam in Anglicanâ editione expoſitis, adjicit quædam quæ David Gregorius Scotus Profeſſor Oxonienſis, & Archibaldus Pitcarnius Medicinæ Lugduni Batavorum Profeſſor, non abludentia attulerunt. Addit Cap. 95. Algebræ, pag. 389, apud exteros (ut verba ejus ſonant) etiam Leibnitium & Tſchurnhauſium nonnihil ejuſmodi præſtitiſſe, & apud Britannos Jacobum Gregorium, & Nicolaum Mercatorem; ſed quæ ſunt, ut plurimum, nonniſi caſus particulares intra ambitum generalem regularum Newtoni. Calculo quoque differentiali Leibnitii affinem eſſe methodum fluxionum Newtoni, in Principiis Naturæ Mathematicis primùm editam, tum utrâque eſſe antiquiorem Barrovii; & omnes Walliſianæ Arithmeticæ Infinitorum ſuperſtrui, quæ Cavallerii geometriam promovit, ut hic Archimedæam. Exhibet etiam methodum quandam Joſephi Raphſon pro infinitis ſeriebus, libello Londini *1690 edito, ſub titulo Analyſeos Æquationum Univerſalis, comprehenſam. Cæterùm ipſe Newtonus, non minus candore quàm præclaris in rem mathematicam meritis inſignis, * publicè & privatim agnovit, Leibnitium tum cùm, interveniente celeberrimo viro Henrico Oldenburgo Bremenſi, Societatis Regiæ Anglicanæ tunc Secretario, inter ipſos, ejuſdem jam tum Societatis Socios, commercium intercederet; id eſt jam fere ante annos viginti & amplius; calculum ſuum differentialem, ſerieſque infinitas, & pro iis quoque methodos generales habuiſſe; quod Walliſius, in præfatione Operum factæ inter eos communicationis mentionem faciens, præteriit; quoniam de eo fortaſſe non ſatis ipſi conſtabat. Cæterùm differentiarum conſideratio Leibnitiana, cujus mentionem facit Walliſius, ne quis ſcilicet, ut ipſe ait, cauſaretur de calculo differentiali nihil ab ipſo dictum fuiſſe, meditationes aperuit, quæ aliunde non æquè naſcebantur. Eſt enim differentia analyticum quiddam, & calculi capax; & quod rei caput eſt, ſummæ reciprocum; eáque demum ratione factum eſt, ut calculus analyticus non minus in Geometriâ altiore, quam Carteſius à ſuo calculo excluſerat, quàm in ordinariâ, ab ipſo tractatâ, procedat. Et quemadmodum Apollonius, & alii veteres, habebant quidem proprietates ordinatarum pro lineis conicis & aliis, ex quibus formatæ ſunt poſtea æquationes à Carteſio; ita ſimiliter lineæ, quas ipſe Carteſius, quippe calculo ſuo intractabiles, à Geometriâ excluſerat, Leibnitianâ primùm methodo æquationibus finitis ſunt expreſſæ, & ſub leges analyſeos redactæ; quâ ratione, omnes earum proprietates analytico jam calculo inveſtigari poſſunt, prorſus ut in ordinariis. Et cùm antea per viam figurarum, & imaginationibus, etiam præſtantiſſimi geometræ faciliora tantùm aſſequi in his potuerint, nunc, ope hujus calculi, non tantùm priora illa primo velut obtutu patent, quæ tunc*

* Methodum differentialem Moutoni & Leibnitius habuit anno 1673, & ſuam eſſe voluit: methodum aliam differentialem nondum habuit: ſeries poſtea habuit, ſed quas anno 1675 ab Oldenburgo accepit, ab aliis priùs accipere potuiſſet. Methodum generalem perveniendi ad ejuſmodi ſeries anno proximo ab Oldenburgo petiit, à Newtono accepit, antea non habuit. Methodum extrahendi radices in ſpeciebus à Newtono ſimul accepit, quâ methodus ejus per tranſmutationem

tunc merito admirationi erant, ſed & multo magis abſtruſa deteguntur, ad quæ imaginatio non pertingit, in quo conſiſtit potiſſimus calculi analytici uſus. Cæterùm ipſum celeberrimum Walliſium, quo eſt candore, non dubitamus etiam noſtratium meditationibus, ſi ſufficientem earum habuiſſet notitiam, locum ampliorem in ſuo opere daturum fuiſſe. Sed ipſe queritur, ultimâ Algebræ ſuæ paginâ, hæc noſtra Eruditorum Acta, in quibus bona earum pars continetur, minùs ſibi fuiſſe viſa: unde neque illa ſatis ſibi cognita ait, quæ de Geometriâ Incomparabilium, vel Analyſi Infinitorum, à Leibnitio data fuere, quæ libenter alioqui in ſuo quoque opere exhibiturus fuerit. Cæterùm hâc occaſione & de Nicolao Mercatore, quem Walliſius velut inter ſuos recenſere videtur, notare voluimus, Germanum fuiſſe, & ex Holſatiâ oriundum, etſi in Angliam habitatum conceſſerit; eumque primum fuiſſe, quantum conſtet, qui quadraturam publicè dederit per ſeriem infinitam, tametſi tunc quoque Newtonus in eadem, ipſo inſcio, incidiſſet, eaque multo longiùs produxiſſet.

Excerpta ex epiſtolâ D. J. Walliſii *ad D.* Leibnitium, Oxonii 1° Decemb. 1696 *datâ; quâ reſpondetur ad ea, quæ ex Actis Eruditorum modò deſcripſimus.* N° LXXIII.

Dum hæc ſcripturus eram; oſtendit mihi nonnemo, heſterno die, Acta Lipſica, pro menſe Junii præſentis anni 1696. Quorum eruditus editor dignatus eſt inibi amplam meorum Operum Mathematicorum, Oxonii editorum, mentionem facere. Quo nomine me ipſi obſtrictum ſentio, & gratias habeo.

Sed conqueri videtur, ſaltem ſubinſinuare, quòd, quum Newtoni methodos fuſiùs expoſuerim; de Leibnitianis parciùs dixerim. At nolim ego te, quem magni æſtimo, à me quoquo modo læſum iri. Sed gratulor potius, te, in tantâ nobilitate poſitum, ad res noſtras mathematicas deſcendere voluiſſe. Et tantum abeſt, ut velim ego tibi quocunque modo iniquus eſſe, ut ſiqua ferat occaſio, demerere malim.

Dum addit eruditus editor, illas me fortè præteriiſſe, quòd de illis mihi non ſatis conſtiterit; id omnino verum eſt.

Dicam utique quod res eſt, neque enim fateri pudet: tuarum ego rerum nihil, quod memini, vidi quicquam, præter hæc duo. Quorum alterum, illud eſt, quod inter Londinenſium Collectiones Philoſophicas habetur ſed abſque demonſtratione, ex Actis Lipſicis deſcriptum, de quadrato diametri ad aream Circuli ut 1 ad $\frac{1}{1}-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\frac{1}{9}-\frac{1}{11}$ &c. in infinitum; quod ego meis inſerui, ut † à te factum, ad Algebræ meæ Prop. XCV.

Alterum

tionem figurarum nondum generalis, in methodum quandam generalem evaſit, ſed inutilem: per extractiones ſolas res citius peragitur. Anno 1677 methodum novam differentialem habuit, ac tantam methodi hujus antiquitatem editores jactant, majores non aſſerunt. Methodum generalem, vel ſerierum vel differentialem, Leibnitium, vel primum vel priori marte, inveniſſe Newtonus nondum agnovit publicè.

† Ignoravit Walliſius Gregorium hanc ſeriem anno 1671 cum Collinio, Oldenburgum anno 1675

Alterum eſt illud de Teſtudine Quadrabili; cujus ego, ut de tuo, mentionem facio in Algebræ meæ poſtremo folio. Præter hæc duo ſi plura noverim, non reticuiſſem.

Tuam Geometriam Incomparabilium, vel Analyſin Infinitorum, quam ibidem à te memoratam dixi, ego nondum vidi; nec ejus quicquam vel de nomine ante inaudiveram, quàm prout ibidem ad calcem Algebræ dictum eſt.

Neque Calculi Differentialis vel nomen audiviſſe me memini, niſi poſtquam utrumque volumen abſolverant operæ, eratque præfationis præfigendæ poſtremum folium ſub prelo, ejuſque typos jam poſuerant typothetæ. Quippe tum me monuit amicus quidam harum rerum gnarus, qui peregrè fuerat, tum talem methodum in Belgio prædicari, tum illam cum Newtoni methodo fluxionum quaſi coincidere. Quod fecit, ut, tranſmotis typis jam poſitis, id monitum interſeruerim.

Sed antè monueram, Algebræ, Prop. xcv. pag. 389, quod ſolum potui, Leibnitium & Tſchurnhauſium talia meditatos; ſed quæ ego non videram, necdum vidi. Et ſicubi fortè videram literas G. G. L; neſciebam, quem illæ virum indicabant.

Extant, credo, plura in Actis Lipſicis; ſed quæ ego non vidi: ut nec tu, credo, vidiſti Brounkeri Quadraturam Hyperbolæ, quæ extat in Tranſactionibus Londinenſibus. Mihique condonari poteſt, hâc ætate, qui annum octogeſimum ſuperavi, ſi non omnia ſciſcitarer.

Noveram quidem jamdudum, & indicavi, de rebus hujuſmodi nonnulla te meditatum eſſe; tibique cum Newtono, mediante Oldenburgo, interceſſiſſe literas quaſdam tuas: ſed, quas ego non vidi, nec ſcio quales fuerint: eratque Oldenburgus diu mortuus, ut non potuerim ab illo ſciſcitari. Rogabam quidem per literas Newtonum noſtrum, ut ſi eas penes ſe haberet, earum mihi copiam faceret literarum; ſed retulit ille, ſe non habere. Et quidem periiſſe credo flammis inopinato correptas, cum pluribus Newtoni ſcriptis meliori luce dignis: & niſi per me ſtetiſſet, periiſſent etiam Newtoni literæ. Eoque animo rogabam, ut tuas illas cum Newtoni literis junctim ederem. Idque etiamnum, ſi ferat occaſio, facturus fortè ſum, modo mihi dignaberis earum copiam facere *.

Quòd Henricus Oldenburgus fuerit Bremenſis; & Nicolaus Mercator Holſatus, quod ſuggerit eruditus editor, omnino verum eſſe credo; ſaltem Anglos non fuiſſe ſatis novi; eoſque propterea Germaniæ veſtræ non invideo, adeoque non Noſtrates dixi, ſed "Apud Nos:" nec tamen ideo minùs eos aut amavi, aut æſtimavi. Nam mihi perinde eſt, quâ quis gente ſit; *Tros Tyriuſve foret, nullo diſcrimine*——modo ſit vir bonus & bene meritus. Sed apud nos diu vixerant; & quicquid hâc in re fecerint, apud nos factum eſt.

Quæ fuſiùs expoſui, ut ſentias quàm tibi non iniquus fuerim, aut parum candidus.

1675 cum Leibnitio communicâſſe; & præterea Leibnitium in Angliâ fuiſſe anno 1673. Collinius enim, ab anno 1670 ſeries, à Newtono & Gregorio acceptas, rogatus non rogatus liberrimè nec ſine jactantiâ commrnicavit, ut ex ſuperioribus patet. Et Pellius, cui hæ ſeries ignotæ non erant, cum Leibnitio de ſeriebus verba habuit.

Ex

Ex epistolâ D. Leibnitii *ad* Wallisium *scriptâ,* $\frac{19}{29}$ Martii, *ineuntis anni* 1697. N° LXXIV.

—— Quoniam videris nonnulla, in Actis dicta, ita accepisse, quasi animi parum erga Germanos æqui accuseris, & quasi vicissim tua recensendo extenuentur: putavi non ingratum tibi fore, si epistolam dominis editoribus Actorum scriberem, cujus hîc exemplum addo; quâ, si ipsis videretur, Actis iisdem insertâ, satisfieri tibi, scrupulis illis sublatis, possit. [Habetur in Actis Lipsicis pro mense Junio 1697.]

Ego qui te magni facio, & publicè professus sum, quantum meo judicio tibi debeat altior Geometriâ, æquissimum puto viris præclarè, non de suo tantùm seculo, sed & posteritate meritis debitas gratias rependi. Ut autem animi mei certior esse possis, ecce verbo tenus transcripta, quæ ipse de tuis meritis geometricis dixi, Actorum Lipsiensium mense Junio 1686, pag. 298.

" Paucis dicam, quid potissimum insignibus nostri seculi Mathematicis
" in hoc Geometriæ genere, meâ sententiâ, debeatur.

" Primum Galilæus & Cavallerius involutissimas Cononis & Archime-
" dis artes detegere cœperunt.

" Sed Geometria Indivisibilium Cavallerii scientiæ renascentis non nisi
" infantia fuit. Majora subsidia attulerunt triumviri celebres; Fermatius,
" inventâ methodo de Maximis & Minimis; Cartesius, ostensâ ratione li-
" neas geometriæ communis (transcendentes enim excluserit) exprimendi
" per æquationes: et P. Gregorius à S. Vincentio, multis præclaris in-
" ventis. Quibus egregiam Guldini regulam de Motu Centri Gravitatis
" addo.

" Sed & hi intra certos limites constitere; quos transgressi sunt Huge-
" nius & Wallisius, geometriâ inclyti. Satis enim probabile est, Huge-
" niana Heuratio, & Wallisiana Nelio & Wrennio, qui primi Curvis æ-
" quales rectas demonstravere, pulcherrimorum inventorum occasionem
" dedisse. Quod tamen meritissimæ laudi inventionum nihil detrahit.

" Secuti sunt hos Jacobus Gregorius Scotus, & Isaacus Barrovius An-
" glus: qui præclaris in hoc genere theorematibus scientiam mirificè lo-
" cupletârunt.

" Interea Nicolaus Mercator Holsatus, Mathematicus & ipse præstan-
" tissimus, † primus (quod sciam) quadraturam aliquam dedit per Seriem
" Infinitam.

" At idem inventum non suo tantùm marte assecutus est, sed & uni-
" versali quâdam ratione absolvit, profundissimi ingenii Geometra Isaacus
" Newtonus. Qui, si sua cogitata ederet, quæ illum adhuc premere in-
" telligo, haud dubiè nobis novos aditus ad magna scientiæ incrementa
" compendiaque aperiret."

* Eas tandem obtinuit D. Wallisius è schediasmatis Collinii; alteras Newtoni olim acceperat ab Oldenburgo.

† Mercator quadraturam D. Brounkeri pe .divisionem Wallisianam tantum demonstravit, ut suprà.

Quibus deinde nonnihil de iis addo, * quæ meâ operâ acceſſere; præſertim dum novo calculi genere effeci, ut etiam Algebram tranſcendentia analyſi ſubjiciantur; & Curvas, quas Carteſius à geometriâ malè excluſerat, ſuis quibuſdam † æquationibus explicare docui; unde omnes earum proprietates certo calculi filo deduci poſſunt: exemplo Cycloidis, cui æquationem ibidem aſſigno, $y = \sqrt{2x - xx} + \int \frac{dz}{\sqrt{2x - xx}}$. Ubi ∫ ſignificat ſummationem; & d, differentiationem; x, abſciſſam ex axe inde à vertice; & y, ordinatam normalem.

De te autem queri, nunquam mihi in mentem venit; quem facilè apparet noſtra, in Actis Lipſienſibus prodita, non ſatis vidiſſe.

Quæ inter Oldenburgum & me commutatæ ſunt literæ, quibus aliqua acceſſerint à D. Newtono excellentis ingenii viro, variis itineribus & negotiis, ab hoc ſtudiorum genere planè diverſis, vel periere, ut alia multa; vel jacent in mole chartarum aliquando excutienda digerendaque, ubi à neceſſariis occupationibus vacatio erit; quam mihi tam ſubito quàm vellem promittere non poſſum.

N° LXXV. *Ex epiſtolâ* Walliſii *ad D.* Leibnitium, Apr. 6, 1697.

Vir Nobiliſſime Celeberrimeque,

Literas tuas humaniſſimas, Martii $\frac{19}{29}$ Hannoveræ datas, accepi, & exoſculatus ſum, Martii 13 ſtilo noſtro 1697; hoc eſt, Apr. 10, ſtilo novo. Mihique gratulor, quòd nobiliſſimo viro ego meaque non diſplicuerint. Veniam interim exorare debeo, ſi locorum diſtantia fecerit, ut eruditiſſima tua ſcripta, & inventa, minus ego ſciverim aut intellexerim, quàm vellem; & quidem, quis ſit ille tuus Calculus Differentialis, non ſatis mihi compertum ſit; niſi quòd mihi nuper nunciatum eſt, eum cum Newtoni Doctrinâ Fluxionum quaſi coincidere.

Nec pudet me meam hâc in parte ignorantiam fateri; qui jam ab aliquot annis contentus fuerim, hâc ætate, lampadem tradere; aliiſque permittere, ut promoveant ea quæ, ſiqua, ego non infeliciter detexerim.

Quòd literas ſcripſeris, in mei gratiam, ad editores Actorum Lipſicorum, favori tuo debeo, & grates habeo.

Quis eorum ille ſit, qui mea ſcripta recenſuit in Actis Lipſicis pro menſe Junii 1696, ego quidem non ſcio; ſed ei gratias habeo. Neque enim eſt, cur ego ei ſuccenſere debeam, ſi non primo intuitu ſtatim perſpexerit omnia

* Leibnitius, recitando inventa nova mathematica, prætermittit methodum fluxionum; quaſi analyſis tota infinitefimalis ſolâ ſuâ operâ acceſſerat.

† Annon Newtonus hujuſmodi æquationes priùs invenit; qui docuit fluentem ex æquatione fluxionem involvente extrahere, & Curvas Mechanicas ad æquationes numero terminorum infinitas reduxit, pergendo ab hujuſmodi æquationibus finitis? Annon tota Fluxionum methodus inverſa, ubi de Curvis agitur, pendeat ab hujuſmodi æquationibus ad Curvas applicatis?

‡ Quaſi Leibnitius hoc non advertiſſet anno 1677, ubi primum incidit in methodum Newtoni. Vide litteras ejus ſuprà impreſſas, p. 561. Certè methodum Newtoni, ante annum 1671 inventam fuiſſe, Leibnitius ex litteris ejus intellexerat, ſed in Actis Lipſicis hoc nunquam agnovit. Vide ſuprà p. 481, 544, 545. Sic & ſe ab Oldenburgo ſeræs Newtonianas & Gregorianas, ineunte anno

omnia, quæ penitus rimanti occurriſſent, aut etiam ſint occurſura. Sufficit enim inſtituto ſuo, ut ſumma quæque carpat, & magis obvia; lectoribus permittendo, ſi penitiora deſiderent, apud authores indicatos quærere. Nolim autem exiſtimes, quòd in gentem veſtram minus æquo ſim animo; nam ſecus eſt, &c.

Ubi dicituſ, *Nicolaum Mercatorem primum eſſe qui quadraturam aliquam dedit per ſeriem infinitam*: vide annon mea talis ſit, Ar. Infin. Pr. 191. $\square = \frac{3\times3\times5\times5\times7\times7\times9\times9\ \&c.}{2\times4\times4\times6\times6\times8\times8\times10\ \&c.}$. *Et Brounkeri* $\square =$

$$1\frac{1}{\frac{2}{\frac{9}{\frac{2}{\frac{25}{\frac{2}{\frac{49}{\frac{2}{\frac{21}{2}}}}}}}}}\ \&c.$$

Sed & omnes mearum tabellarum ſeries, in Arithmeticâ infinitorum, ſunt Series Infinitæ; & earum plurimæ, quales quæ vobis dicuntur, novo nomine, ſeries Tranſcendentales.

Nolim utique ut Clariſſimo viro fraudi ſit nova compellatio. Nam quas ego vocaveram Continuas Approximationes, vocat Jacobus Gregorius Series Convergentes; & Newtonus, Series Infinitas; ſed res eadem eſt. Sic quod ego vocaveram Centrum Percuſſionis, vocat Hugenius, novo nomine, Centrum Oſcillationis; ſed eadem res eſt. Et Fermatii Hyperbolæ Infinitæ eædem ſunt cum meis Seriebus Reciprocis. Et Galilæi Cycloides, Merſenni Trochoides, mea Cyclois, & Cuſani Curva, quocunque nomine dicatur, ſunt res eadem. Sic rectificata Curva Nelii, & Curva Heuratii, & Curva demum Fermatii, eadem eſt cum meâ Paraboloide Semi-cubicali. Et Gallorum Socia Cycloidis eſt ea Curva, quæ mihi terminat figuram Sinuum rectorum. Et, ni fallor, ſic ſaltem mihi nunciatum eſt, Newtoni Doctrina Fluxionum res eadem eſt, vel quàm ſimillima, quæ vobis dicitur Calculus Differentialis: quod tamen neutri præjudicio eſſe debet.

Ex epiſtolâ D. Leibnitii *ad* Walliſium *ſcriptâ*, 28 Maii 1697. N° LXXVI.

Methodum Fluxionum profundiſſimi Newtoni, cognatam eſſe methodo meæ Differentiali, non tantùm animadverti ‡. poſtquam opus ejus & tuum prodiit; ſed etiam profeſſus ſum in Actis Eruditorum, & aliâs quoque monui. Id enim candori meo convenire judicavi, non minus quàm ipſius merito. Itaque communi nomine deſignare ſoleo § Analyticos Infiniteſimalis; quæ latiùs quàm methodus Tetragoniſtica patet.

anno 1675, accepiſſe, ſtatim oblitus eſt; p. 517—520, 522. Et methodum ſerierum ſe ab Oldenburgo poſtulâſſe, & à Newtono accepiſſe, ſtatim oblitus eſt; p. 522, 535, 536, 567. Et problemata tangentium inverſa ab æquationibus & quadraturis pendere, ſe primum negâſſe, & ſubinde à Newtono didiciſſe, ſtatim oblitus eſt; p. 544, 557, 558.

§ Methodum fluxionum & methodum differentialem eſſe unam & eandem methodum, Leibnitius hîc agnoſcit; ideoque ſe communi nomine analyſeos infiniteſimalis deſignare ſolere, licet in nonnullis differre poſſint: ut analyſis ſpecioſa Vietæ & ea Carteſii in nonnullis differunt. Quæritur, quis ſit analyſeos hujus infiniteſimalis inventor primus; & ecquid alter alterius inventis addiderit.

Interim,

Interim, quemadmodum & Vietæ & Carteſiana methodus Analyſeos Specioſæ nomine venit; diſcrimina tamen nonnulla ſuperſunt: ita fortaſſe & Newtoniana & Mea differunt in nonnullis.

Mihi conſideratio differentiarum & ſummarum, in ſeriebus numerorum, * primam lucem affuderat, cùm animadverterem differentias tangentibus, & ſummas quadraturis reſpondere. Vidi † mox differentias differentiarum in Geometriâ *Oſculis* exprimi. Et notavi mirabilem analogiam relationis inter differentias & ſummas, cum relatione inter potentias & radices. Itaque judicavi, præter affectiones quantitatis hactenus receptas y, y^2, y^3, $y^{\frac{1}{2}}$, $y^{\frac{1}{3}}$, &c. vel generaliter y^e, ſive $[p]\,y$, vel potentiæ ipſius y ſecundum exponentem e; poſſe adhiberi novas differentiarum vel fluxionum affectiones, dy, d^2y (ſeu ddy) d^3y (ſeu $dddy$) imo utiliter etiam occurrit $d^{\frac{1}{2}}y$, & ſimiliter generaliterque d^ey.

Hâc jam affectione admiſſâ, ‡ vidi commodè per æquationes exprimi poſſe quantitates, quas à ſuâ Analyſi & Geometriâ excluſerat Carteſius; & Curvas, quas ille non rectè vocat Mechanicas, hâc ratione calculo non minùs ſubjici, quàm ab ipſo in Geometriam receptas. Et, quemadmodum Veteres jam æquationes Curvarum Locales obſervaverant, ſed Carteſius tamen utilem operam nobis navavit, dum eas calculo ſuo expreſſit: ita putavi, me non inutiliter facturum, ſi oſtenderem methodum Curvas, ab ipſo excluſas, ſimiliter per æquationes exprimendi; quarum ope, omnia de iis certo calculi filo haberi poſſint.

Et licet fatear, quemadmodum rem ipſam, in æquationibus Curvarum localibus facilioribus, calculo Carteſii expreſſam, jam tenebant Veteres; ita rem ipſam, meis æquationibus differentialibus facilioribus expreſſam, non potuiſſe tibi, aliiſque egregiis viris, eſſe ignotam: non ideo minùs tamen puto, & Carteſium & me aliquod utile præſtitiſſe. Nam antequam talia ad conſtantes quoſdam characteres calculi analytici reducuntur, tantúmque omnia vi mentis & imaginationis ſunt peragenda, non licet in magis compoſita abditaque penetrare; quæ tamen, calculo ſemel conſtituto, luſus quidem jocuſque videantur.

Unde jam mirum non eſt, § problemata quædam, poſt receptum calculum meum, ſoluta haberi, quæ antea vix ſperabantur: ea præſertim quæ ad tranſitum pertinent à Geometriâ ad Naturam. Quoniam ſcilicet vulgaris geometria minùs ſufficit, quoties Infiniti involvitur conſideratio; quam pleriſque Naturæ operationibus ineſſe, conſentaneum eſt, quo meliùs referat autorem ſuum.

* Fatetur hîc Leibnitius, methodum tangentium per differentias primam lucem ipſi affudiſſe; id eſt, methodum, quam Fermatius, Gregorius, Barrovius coluere, Newtonus N° X. ad quantitatum augmenta momentanea generaliter applicuit. Hancce tangentium methodum Leibnitius, lectis Newtonianis, meditatur, p. 523, 544, 558, 559: generalem reddit, p. 503: & Newtonianæ ſimilem eſſe ſtatim videt, p. 561, 564.

† Fermatius & Schootenus hoc antè viderunt, determinando punctum flexûs contrarii in Conchoide.

‡ Leibnitius hoc non vidit ante annum 1677. Scripſit enim anno 1676, inverſa tangentium problemata, & alia multa, ab æquationibus non pendere. Reſcripſit Newtonus, hujuſmodi problemata in poteſtate eſſe, nempe per æquationes ſuas. Et tum demum Leibnitius, à Newtono admonitus, hæc vidit. Vide p. 538, lin. penult.

Hugenius

Hugenius certe, || qui hæc ſtudia haud dubiè profundiſſimè inſpexerat, multiſque modis auxerat, initio parvi faciebat calculum meum, nondum perſpectâ ejus utilitate. Putabat enim, dudum nota ſic tantùm novè exprimi: prorſus quemadmodum Robervallius, & alii initio, Carteſii Curvarum calculos parvi faciebant. Sed mutavit poſtea Hugenius ſententiam ſuam; cùm videret quàm commodâ eſſet hæc exprimendi ratio, & quàm facile per eam res involutiſſimæ evolverentur. Itaque maximi eam à ſe fieri, aliquot ante obitum annis, non tantùm in privatis ad me alioſque literis, ſed publicè quoque eſt profeſſus.

Cæterùm Tranſcendentium appellationem, nequid à me præter rationem in phraſi geometricâ novari putes, ſic accipio, ut tranſcendentes quantitates opponam ordinariis & algebraicis: et Algebraicas quidem, vel ordinarias, voco quantitates, quarum relatio ad datas exprimi poteſt algebraicè; id eſt, per æquationes certi gradûs; primi, ſecundi, & tertii, &c; quales quantitates Carteſius ſolas in ſuam Geometriam recipiebat: ſed Tranſcendentes voco, quæ omnem gradum algebraicum tranſcendunt. Has autem exprimimus, vel per valores infinitos, & in ſpecie per ſeries (neque enim ipſas ſeries Tranſcendentales voco, ſed quantitates ipſis exprimendas) vel per æquationes finitas; eaſque vel differentiales; ut cùm ordinata cycloidis methodo meâ exprimitur per æquationem ** $y = \int \frac{x dx}{\sqrt{ay - yq}}$; vel exponentiales; ut cum incognita quædam x exprimitur per hanc æquationem $x^x + x = 1$. Et quidem tranſcendentium exponentialem pro perfectiſſimâ habeo; quippe quâ obtentâ, nihil ultrà quærendum reſtare arbitror; quod ſecus eſt in cæteris.

Primus autem, ni fallor, etiam Exponentiales æquationes introduxi, cùm ignota ingreditur exponentem. Et jam anno primo †† Actorum Lipſienſium, ſpecimen dedi in exemplo quantitatis ordinariæ Tranſcendentaliter expreſſæ, ut res fieret intelligibilior; nempe, ſi quæratur $x^x + x = 30$, patet $x = 3$ ſatisfacere; cum ſit $3^3 + 3 = 27 + 3 = 30$.

Ex epiſtolâ Walliſii *ad* Leibnitium, Julii 30, 1697. N° LXXVII.

Optaverim ita, ut tibi vacet tuum Calculum Differentialem, & Newtono ſuam Fluxionum Methodum, juſto ordine exponere; ut quid ſit utrique

§ Mirum eſt, hæc à D. Leibnitio dici; qui ex literis & Principiis Newtoni intellexerat, methodum ſolvendi hujuſmodi problemata Newtono, ante annum 1671, innotuiſſe; & ipſum primum, per hanc methodum, problemata tractâſſe, quæ ad tranſitum pertinent à geometriâ ad naturam.

|| Hugenius literas, quæ inter Newtonum & Leibnitium mediante Oldenburgo interceſſerant, nunquam vidit.

** Legendum $y = \int \frac{\dot{x} dx}{\sqrt{ax - xx}}$. Idem ſic deſignari poteſt $y = \frac{\dot{x}\dot{x}}{\sqrt{ax - xx}}$; vel ſic, $\dot{y} = \frac{\dot{x}\dot{x}}{\sqrt{ax - xx}}$. Et nota, quòd ubi differentiæ referuntur ad ſummas, rectiùs dicerentur partes. Sunt enim non differentiæ ſummarum, ſed partes; & nullam relationem habent ad ſummas, niſi quatenus ſunt earum partes.

†† Imo anno 1677. Vide pag. 560, 564.

commune,

commune, & quid interſit diſcriminis, & utramque diſtinctius intelligamus *.

N°LXXVIII. *In diſſertatione D.* Nicolai Fatii Duillierii, *R. S. S. de inveſtigatione geometricâ lineæ breviſſimi deſcenſûs, &c.* Londini, *anno* 1699 *editâ, pag.* 18, *hæc habentur.*

" Newtonum primum, ac pluribus annis vetuſtiſſimum, hujus calculi " inventorem, ipſâ rerum evidentiâ coactus, agnoſco: à quo utrum quic- " quam mutuatus ſit Leibnitius, ſecundus ejus inventor, malo eorum, " quàm meum, ſit judicium, quibus viſæ fuerint Newtoni literæ, aliique " ejuſdem manuſcripti codices."

Et reſpondit D. Leibnitius *in Actis Lipſienſibus menſe* Maii 1700.

" Certè cùm elementa calculi mea edidi anno 1684, † ne conſtabat " quidem mihi aliud de inventis ejus in hoc genere, quàm quod ipſe olim " ſignificaverat in literis; poſſe ſe tangentes invenire, non ſublatis irratio- " nalibus: quod Hugenius quoque ſe poſſe mihi ſignificavit poſtea, etſi " cæterorum iſtius calculi adhuc expers: ſed majora multo conſecutum " Newtonum, viſo demum libro Principiorum ejus, ſatis intellexi. Cal- " culum tamen differentiali tam ſimilem ab eo exerceri, non antè didici- " mus, quàm cùm non ita pridem magni geometræ Johannis Walliſii Ope- " rum volumina primum & ſecundum prodiere, Hugeniuſque, curioſitati " meæ favens, locum inde deſcriptum, ad Newtonum pertinentem, mihi " maturè tranſmiſit."

Et poſt aliqua, de methodi hujus parte ſublimiore verba faciens, addit:

" Quam [methodum] ante dominum Newtonum & me nullus quod ſciam " geometra habuit; uti ante hunc maximi nominis geometram, NEMO, " ſpecimine publicè dato, ſe habere probavit: ante dominos Bernoullios & " me nullus communicavit."

D. Fatio *autem replicationem ſuam ad editores* Lipſienſes, *ut publicaretur, mittente, hi, quaſi lites averſati, eandem Actis ſuis inſerere recuſârunt. Vide* Act. Lipſ. Martii 1701, *pag.* 134.

* Ut Leibnitius differentiam methodorum exponat, iterum rogat Walliſius; ſed fruſtra.

† Conſtabat certè D. Leibnitio, jam ab anno 1677, Curvarum quadraturas faciliores reddi; & problemata tangentium inverſa D. Newtoni methodis ſolvi; idque nonnunquam per quadraturas ſolas, nonnunquam per methodos generaliores. Confer literas ejus pag. 561, & ſeq. cum pag. 544, 545, 557, 558, ut & cum pag. 510, lin. 25, 26, & pag. 523, lin. 4, 7, 17.

‡ Compilatores Actorum, in ſcribendis librorum Breviariis, à cenſuris temerariis abſtinere debent. Ex hâc cenſurâ patet animus ſcriptoris in D. Newtonum.

§ Literis D. T. Tſchurnhauſius deſignatur.

|| Hæc Iſagoge, & ſcholium propoſitionis ultimæ, ſcripta ſunt ubi liber prodiit; reliqua ex MS antiquo, manibus amicorum trito, impreſſa ſunt.

Tandem ubi prodiere Newtoni Libri de Numero Curvarum ſecundi generis, de- N° LXXIX.
que quadraturâ figurarum, editores Actorum Lipſienſium, ſtylo Leibnitiano, ſynopſin libri prioris his verbis concluſerunt. Vide Act. Lipſ. Januarii 1705.

Cæterùm autor non attigit focos, vel umbilicos, Curvarum ſecundi generis; & multo minùs generum altiorum. Cùm ‡ ergo ea res abſtruſioris ſit indaginis, & maximi tamen in hoc genere uſûs, tum ad deſcriptiones, tum ad alias proprietates Curvarum, & doctrina hæc focorum ab illuſtriſſimo D. § D. T. profundiùs ſit verſata; ſupplementum ejus pro his Curvis ab ipſius ingenio expectamus.

Dein libri alterius ſynopſin ſequentem, ſi ſynopſis dici mereatur, eodem ſtylo ſubjunxerunt.

*Ingenioſiſſimus deinde autor, antequam ad quadraturas Curvarum, vel potius figurarum curvilinearum, || veniat, præmittit brevem Iſagogen. Quæ ** ut meliùs intelligatur, ſciendum eſt, cùm magnitudo aliqua continuè creſcit, veluti linea, exempli gratiâ, creſcit fluxu puncti quod eam deſcribit, †† incrementa illa momentanea appellari differentias; nempe inter magnitudinem quæ antea erat, & quæ per mutationem momentaneam eſt producta; atque hinc natum eſſe calculum differentialem, eique reciprocum ſummatorium; cujus elementa ab inventore D. Godefrido Guilielmo Leibnitio in his Actis ſunt tradita; variique uſus tum ab ipſo, tum à D. D. Fratribus Bernoulliis, tum & D. Marchione Hoſpitalio (cujus nuper extincti immaturam mortem omnes magnopere dolere debent, qui profundioris doctrinæ profectum amant) ſunt oſtenſi. Pro differentiis igitur Leibnitianis D. Newtonus ‡‡ adhibet, ſemperque adhibuit, fluxiones; quæ ſunt quàm proximè ut fluentium argumenta, æqualibus temporis particulis quàm minimis genita; iiſque, tum in ſuis Principiis Naturæ Mathematicis, tum in aliis poſtea editis, eleganter eſt uſus; quemadmodum & Honoratus Fabrius in ſuâ Synopſi Geometricâ, motuum progreſſus Cavallerianæ methodo ſubſtituit.*

Subinde editores vice ſymbolorum Newtoni deſcribunt ſymbola Leibnitii, & poſtea librum Newtoni ſic breviter attingunt.

Cùm regreſſus à differentiis ad quantitates, vel à quantitatibus ad ſummas, vel denique à fluxionibus ad fluentes non ſemper algebraicè fieri poſſit; ideo quærendum eſt, tum quibus caſibus quadratura algebraicè ſuccedat, tum quomodo, algebraico ſucceſſu deficiente, aliquid ſubſidiarium adhiberi queat. In utroque enim à D. Newtono eſt utiliſſimè laboratum, tum aliâs, tum in hoc tractatu de quadraturis; ubi

** Ut Iſagoge meliùs intelligatur, Leibnitius deſcribit calculum ſuum differentialem, & omittit calculum Newtonianum, quem ſolum deſcribere debuiſſet. Hoc fecit, non ut calculus Newtonianus, in Iſagoge traditus meliùs intelligatur, ſed ut rejiciatur.

†† Incrementa illa momentanea Newtonus momenta, Leibnitius poſtea differentias vocavit. Et inde natum eſt nomen calculi differentialis.

‡‡ Senſus verborum eſt, quòd Newtonus fluxiones differentiis Leibnitianis ſubſtituit, quemadmodum Honoratus Fabrius motuum progreſſus Cavallerianæ methodo ſubſtituerat; id eſt, quod Leibnitius author primus fuit hujus methodi, & Newtonus eandem à Leibnitio habuit, ſubſtituendo fluxiones pro differentiis.

*ſeries adhibet infinitas; quæ, eo caſu quo abrumpuntur, ſeu finiuntur, quæſitum algebraicè exhibent. De * quo etiam dictum eſt nuper in recenſione tractatus D. Cheynæi, Medici Scoti Londini degentis. Conferri etiam poteſt tractatus D. Craigii Scoti de Quadraturis, & ejuſdem theorema ad quadraturas pertinens, nuper in his Actis exhibitum; quæ faciunt etiam, ut ipſis theorematis Newtonianis recenſendis ſuperſedeamus, quia paucis exponi non poſſunt: quemadmodum nec ejuſdem theoremata quædam reductionis ad quadraturas faciliores.*

His permotus D. Joannes Keill, in epiſtolâ in Philoſophicis Tranſactionibus A. C. 1708, menſibus Septemb. & Octob. impreſſâ, ſcripſit in contrarium, quòd Fluxionum Arithmeticam, ſine omni dubio, primus invenit Dominus Newtonus, ut cuilibet ejus epiſtolas, à Walliſio editas, legenti facilè conſtabit. Eadem tamen arithmetica poſtea, mutatis nomine & notationis modo, à Domino Leibnitio in Actis Eruditorum edita eſt.

N° LXXX. *Epiſtola D.* Leibnitii *ad D.* Hans Sloane *Regiæ Societatis Secretarium,* 4 Martii *S. N.* 1711 *data.*

Gratias ago, quòd noviſſimum volumen præclari operis Tranſactionum Philoſophicarum ad me miſiſti; quamvis nunc demum mihi, Berolinum excurrenti, redditum ſit. Itaque excuſabis, quòd pro munere ſuperioris anni nunc demum gratiæ, dudum debitæ, redduntur.

Vellem inſpectio operis me non cogeret, nunc ſecundâ vice, ad vos querelam deferre. Olim Nicolaus Fatius Duillierius me pupugerat, in publico ſcripto, tanquam alienum inventum mihi attribuiſſem. Ego eum in Actis Eruditorum Lipſienſibus meliora docui; & vos ipſi, ut ex literis à Secretario Societatis veſtræ inclytæ (id eſt, quantum memini, à teipſo) ſcriptis didici, hoc improbâſtis. Improbavit Newtonus ipſe vir excellentiſſimus, quantum intellexi, præpoſterum quorundam hâc in re erga veſtram gentem & ſe ſtudium. Et tandem D. Keillius in hoc ipſo volumine, menſe *Sept. Octob.* 1708, pag. 185, renovare ineptiſſimam accuſationem viſus eſt; cùm ſcripſit, *Fluxionum Arithmeticam à Newtono inventam, mutato nomine & notationis modo, à me editam fuiſſe.* Quæ qui legit, & credit, non poteſt non ſuſpicari, alterius inventum à me, larvatum ſubdititiis nominibus characteribuſque, fuiſſe protruſum. Id quidem quàm falſum ſit, nemo melius ipſo D. Newtono novit. Certè ego nec nomen calculi fluxionum fando audivi; nec characteres, quos adhibuit D. Newtonus, his oculis vidi, antequam in Walliſianis operibus prodiere. Rem etiam me habuiſſe, multis antè annis quàm edidi, ipſæ literæ apud Walliſium editæ demonſtrant. Quomodo ergo aliena mutata edidi, quæ ignorabam.

Etſi autem D. Keillium, à quo magis præcipiti judicio quàm malo animo peccatum puto, pro calumniatore non habeam; non poſſum tamen non ipſam accuſationem in me injuriam pro calumniâ habere. Et quia verendum eſt,

* Senſus eſt, quòd quæ Newtonus habet in hoc tractatu de quadraturis, & ſpeciatim de quadraturis illis ubi ſeries abrumpuntur, vel finiuntur, à Cheynæo & Craigio priùs dicta ſunt, & in his Actis nuper exhibita; quæ, quia multa ſunt, faciunt, ut à Newtonianis recenſendis editores Actorum

eſt, ne ſæpe vel ab improbis, vel ab imprudentibus repetatur; cogor remedium ab inclytâ veſtrâ Societate Regiâ petere. Nempe æquum eſſe vos ipſi, credo, judicabitis, ut D. Keillius teſtetur publicè, non fuiſſe ſibi animum imputandi mihi, quod verba inſinuare videntur, quaſi ab alio hoc quicquid eſt inventi didicerim, & mihi attribuerim. Ita ille & mihi læſo ſatisfaciet, & calumniandi animum à ſe alienum eſſe oſtendet; & aliis, aliâs ſimilia aliquando jactaturis, frænum injicietur. Quod ſupereſt vale & fave.

Dabam Berolini 4 *Martii* 1711.

Epiſtola D Johannis Keill, *A. M. ex Ædе Chriſti* Oxon. *R. S. Socii, & jam Aſtronomiæ Profeſſoris* Saviliani, *ad D.* Hans Sloane, *M. D. Regiæ Societatis Secretarium, cum D.* Leibnitio *communicanda.* N° LXXXI.

Cùm D. Leibnitii epiſtolam mecum Vir Cl. communicare dignatus ſis; ea etiam, quæ mihi viſum fuerit reſcribere, ne graveris accipere. Sentio virum egregium acerrimè de me queri, quaſi ei injuriam fecerim, & rerum à ſe inventarum gloriam aliò tranſtulerim. Fateor querelam hanc ideo mihi moleſtam eſſe; quòd nolim, ea ſit de me hominum opinio, quaſi ego calumniandi ſtudio, cuiquam in rebus mathematicis verſanti, nedum viro in iiſdem verſatiſſimo, obtrectarem; certè nihil ab ingenio meo magis alienum eſt, quàm alterius laboribus quicquam detrahere.

Agnoſco me dixiſſe fluxionum Arithmeticam à D. Newtono inventam fuiſſe, quæ mutato nomine & notationis modo, à Leibnitio edita fuit. Sed nollem hæc verba ita accipi; quaſi aut nomen, quod methodo ſuæ impoſuit Newtonus, aut notationis formam, quam adhibuit, D. Leibnitio innotuiſſe contenderem: ſed hoc ſolum innuebam, D. Newtonum fuiſſe primum inventorem Arithmeticæ fluxionum, ſeu calculi differentialis; eum autem in duabus ad Oldenburgum ſcriptis epiſtolis, & ab illo ad Leibnitium tranſmiſſis, indicia dediſſe, perſpicaciſſimi ingenii viro ſatis obvia, unde Leibnitius principia iſtius calculi hauſit, vel ſaltem haurire potuit: at çùm loquendi & notandi formulas, quibus uſus eſt Newtonus, ratiocinando aſſequi nequiret vir illuſtris, ſuas impoſuit.

Hæc ut ſcriberem, impulerunt Actorum Lipſienſium Editores; qui in eâ quam exhibent, operis Newtoniani de fluxionibus ſeu quadraturis enarratione, diſertè affirmant D. Leibnitium fuiſſe iſtius methodi inventorem; & Newtonum aiunt pro differentiis Leibnitianis fluxiones adhibere, ſemperque adhibuiſſe. Id quidem in iiſdem ſcriptoribus obſervatu dignum; quod loquendi & notandi formam, à Newtono adhibitam, in Leibnitianam paſſim in eâdem enarratione transferunt; de differentiis ſcilicet, & ſummis, & calculo ſummatorio loquuntur, de quibus eſt nullus apud Newtonum ſermo: quaſi inventa Newtoni Leibnitianis poſteriora fuerint, & à calculo Leibnitii, in Actis Lipſienſibus anno 1684 deſcripto, ortum derivârint. Cùm reverâ Newtonus, ut ex ſequentibus patebit, fluxionum methodum invenerit, octodecim ſaltem annos antequam Leibnitius quicquam de calculo dif-

torum ſuperſedeant. Et eodem ſenſu D. Leibnitius ad Secretarium Societatis Regiæ nuper ſcripſit, ſuum cuique hic redditum eſſe, quaſi ſecundam Newtoni ad Oldenburgum epiſtolam, ad ſe miſſam, & ſuprà impreſſam nunquam legiſſet. Vide pag. 544—547.

ferentiali edidiſſet, tractatumque de eâ re conſcripſerit; cujus cùm ſpecimina quædam Leibnitio oſtenſa ſint, rationi non incongruum eſt, ea aditum illi ad calculum differentialem aperuiſſe.

Unde ſi quid de Leibnitio liberiùs dixiſſe videar, id eo animo feci, non ut ei quicquam eriperem; ſed ut quod Newtoni eſſe arbitrabar, auctori ſuo vindicarem.

Maxima equidem eſſe Leibnitii in rempublicam literariam merita, lubens agnoſco; nec eum in reconditiore matheſi ſcientiſſimum eſſe, diffitebitur, qui ejus in Actis Lipſienſibus ſcripta perlegerit. Cùm autem tantas tamque indubitatas opes de proprio poſſideat; certè non video, cur ſpoliis ab aliis detractis onerandus ſit. Quare cùm intellexerim, populares ſuos ita illi favere, ut eum laudibus non ſuis accumulent; haud præpoſterum in gentem noſtram ſtudium eſſe duxi, ſi Newtono, quod ſuum eſt, tueri & conſervare anniterer: nam ſi Lipſienſibus fas fuerit, aliena Leibnitio affingere, Britannis ſaltem ea, quæ à Newtono erepta ſunt, ſine crimine calumniæ repoſcere licebit. Itaque cùm ad Regiam Societatem appellet vir illuſtris, meque publicè teſtari velit, calumniandi animum à me alienum eſſe; ut calumniandi crimen à me amoveam, mihi oſtendendum incumbit, D. Newtonum verum & primum fuiſſe Arithmeticæ fluxionum, ſeu calculi differentialis, inventorem; deinde ipſum adeo clara & obvia methodi ſuæ indicia Leibnitio dediſſe, ut inde ipſi facile fuerit, in eandem methodum incidere.

Sciendum verò primum eſt, celeberrimos tunc temporis geometras, Dominos Franciſcum Sluſium, Iſaacum Barrovium, & Jacobum Gregorium, methodum habuiſſe, quâ Curvarum tangentes ducebant, quæ à fluxionum methodo non multum abludebat; & iiſdem principiis innixa fuit. Nam ſi pro literâ *o*, quæ in Jacobi Gregorii Parte Matheſeos Univerſali quantitatem infinitè parvam repræſentat; aut pro literis *a* vel *e*, quas ad eandem deſignandam adhibet Barrovius; ponamus $\dot{x}$ vel $\dot{y}$ Newtoni, vel dx ſeu dy Leibnitii; in formulas fluxionum, vel calculi differentialis, incidemus: & regreſſus, quo à datâ tangentium proprietate ad naturam Curvæ perveniebant, quem methodum tangentium inverſam nominabant, eadem plane res erat, ac methodus quâ à fluxionibus ad fluentes revertitur: interim ſuam methodum non ultra fluxiones primas extendebant; neque eandem ad quantitates ſurdis aut fractionibus involutas accommodare potuerunt. At prius quàm quicquam de hoc argumento à ſummis hiſce viris publico datum eſt, D. Newtonus methodum excogitavit, priori quidem non diſſimilem, ſed multo latius patentem; quæ non ſubſtitit ad æquationes eas, in quibus una vel utraque quantitas indefinita radicalibus eſt involuta; ſed, abſque ullo æquationum apparatu, tangentem confeſtim ducere monſtrabat; quæſtiones de maximis & minimis eodem artificio tractabat; & ſpeculationes de quadraturis facilius explicuit. Hæc conſtant ex epiſtolâ Newtoni, ad D. Collinium datâ, Decembris die 10, anno 1672, & inter Collinii chartas repertâ.

Hæc epiſtola habetur impreſſa N° XXVI.

Ex

Ex hâc epiſtolâ clarè conſtat, D. Newtonum methodum fluxionum habuiſſe ante annum 1670, eodem nempe quo Barrovii Lectiones editæ ſunt.

Anno 1669 miſit Newtonus ad D. Collinium tractatum de Analyſi per Æquationes Infinitas; quem etiam, inter ſchedas Collinii repertum, D. Jones nuper edidit. Sub hujus fine habetur demonſtratio regulæ pro Quadraturis Curvarum, nata ex proportione augmentorum naſcentium abſciſſæ & ordinatæ, cùm abſciſſa ſit z, & ordinata $x^{\frac{p}{n}}$; quæ quidem demonſtratio commune fundamentum eſt tam doctrinæ fluxionum, quàm calculi differentialis. Ex eo autem tractatu non pauca, amicis ſuis communicanda, deprompſit Collinius: unde certum eſt, D. Newtono, ante illud tempus, fluxionum Arithmeticam innotuiſſe. Præterea conſtat ex poſteriore Newtoni ad Oldenburgum epiſtolâ, eum, ſuadentibus amicis, circa annum 1671 tractatum de hiſce rebus conſcripſiſſe; quem, unà cum theoriâ lucis & colorum, in publicum dare ſtatuerat: ſcribitque Oldenburgo, ſeries infinitas non magnam ibi obtinuiſſe partem; ſeque alia haud pauca congeſſiſſe, inter quæ erat methodus ducendi tangentes, quam ſolertiſſimus Sluſius, ante annos duos treſve, cum Oldenburgo communicaverat; ſed quæ generalior facta, non ad æquationes, quæ ſurdis aut fractionibus involutæ ſunt, hærebat; &, eodem fundamento uſum, ad theoremata generalia, quadraturas Curvarum ſpectantia, perveniſſe ſe ait Newtonus. Horum unum exempli loco in ipſâ epiſtolâ ponit; ſeriem exhibens, cujus termini dant quadraturam Curvæ, cùm abſciſſa eſt z & ordinatim applicata $dz^{\theta} \times \overline{e + fz}^{\lambda}$. Quæ ſeries abrumpitur, & terminis finitis Curvæ quadraturam comprehendit, quandocunque illa finitâ æquatione exprimi poteſt. Hoc dicit eſſe primum theorematum generaliorum; unde ſequitur, eum alia, ad caſus difficiliores & magis intricatos accommodata, habuiſſe: eſt autem theorema illud Propoſitio v. in tractatu de quadraturis. Eodem etiam ſpectat ejuſdem Prop. VI. ſed ad caſus magis implicatos ſe extendit. Propoſitiones tertia & quarta ſunt lemmata Theor. hiſce demonſtrandis præmiſſa. Secunda autem in quadraturis Propoſitio extat in tractatu de Analyſi per Æquationes Infinitas, & prima propoſitio eſt ea ipſa, quam in dictâ epiſtolâ fundamentum operationum vocat, & tranſpoſitis literis celari tunc voluit. Scribit etiam Newtonus, ſe dudum theoremata quædam, quæ comparationi Curvarum cum ſectionibus conicis inſerviant, in catalogum retuliſſe; & ordinatas Curvarum, quæ ad eam normam comparari poſſunt, in eâdem epiſtolâ deſcribit; quæ profecto eædem planè ſunt cum iis, quas tabula ſecunda ad ſcholium Propoſitionis x. in tractatu de quadraturis, exhibet; unde ſatis liquet tabulam illam & Propoſitiones VII, VIII, IX & X, quæ ſunt in tractatu de quadraturis (à quibus tabula pendet) Newtonum dudum inveniſſe ante annum 1676, quo ſcripta eſt epiſtola illa poſterior. Cùm verò, in primâ ad Oldenburgum epiſtolâ, dicit ſe ab ejuſmodi ſtudiis per quinquennium abſtinuiſſe; hinc ſatis clarè colligi poteſt, propoſitiones in tractatu de quadraturis à D. Newtono inventas fuiſſe, quinquennio ſaltem antequam epiſtolæ illæ ad Oldenburgum ſcriptæ

N° LXXXII.

tæ eſſent; totamque illam de fluxionibus doctrinam, ante illud tempus ulterius à Newtono provectam eſſe, quàm ad hunc uſque diem à quoquam alio factum eſt ſub nomine calculi differentialis. Certè neminem novi, qui, in hâc provinciâ peragrandâ, æquis paſſibus cum Newtono progreſſus ſit: & pauci ſunt, iique inſignes geometræ, qui proſpicere queant, quouſque ille in eâdem provinciâ proceſſerit. Præterea, in poſteriore illâ ad Oldenburgum epiſtolâ, modum deſcribit, quo in ſeriem inciderit, cujus termini fluxiones, ſeu differentias, quantitatum in infinitum exhibent; quæ poſtquam inventa eſſet, dicit, peſtem ingruentem ipſum coegiſſe hæc ſtudia deſerere, & alia cogitare. At peſtis illa contigit annis 1665 & 1666; unde patet, etiam ante illud tempus fluxionum calculum D. Newtono innotuiſſe; hoc eſt duodecim ſaltem annos antequam calculum ſuum Oldenburgo communicavit Leibnitius; & novemdecim annos antequam Vir Illuſtris eandem in Actis Lipſienſibus edidit: & certè ante viſas haſce duas Newtoni epiſtolas, Leibnitium calculum ſuum differentialem habuiſſe, nulla apparent veſtigia. His omnibus ritè perpenſis, certiſſimè cuivis conſtabit, D Newtonum pro vero inventore Arithmeticæ fluxionum habendum eſſe.

N°LXXXIII. Reſtat jam ut inquiramus, quænam fuere indicia Leibnitio à Newtono derivata, unde ei facile foret calculum differentialem elicere. Et primo, ut dixi, nullibi oſtendit Leibnitius ſibi notum fuiſſe calculum differentialem, ante viſas has duas Newtoni epiſtolas; imo ante illud tempus longiore uſus eſt circuitu, cùm res faciliùs multo & ſuccinctiùs ex calculo fluerent differentiali. Hujus rei teſtis ſit epiſtola, ad Oldenburgum data $\frac{1}{2}\frac{8}{8}$ Novembris 1676, quæ in Operum Walliſianorum tomo tertio etiam extat, in quâ modum tradit exprimendi rationem ſubtangentis ad ordinatam, in terminis quos non ingreditur ordinata; ubi ſi loco y & dy ipſarum valores vinculo incluſos poſuiſſet, ſtatim ſcopum attigiſſet.

In primâ epiſtolâ, quæ per Oldenburgum ad Leibnitium tranſmiſſa eſt, docuit Newtonus methodum, quâ quantitates in ſeries infinitas reducendæ ſint, i. e. quâ quantitatum fluentium incrementa exhiberi poſſunt. In ipſo enim initio ſeriem oſtendit, cujus termini hæc incrementa repræſentant. Sed illa D. Leibnitium prorſus latebat, ante viſam Newtoni epiſtolam, quâ exponitur.

Sit o incrementum momentaneum quantitatis fluentis x, & $\frac{m}{n}$ index dignitatis ejuſdem, & ſi pro x ſcribatur $x+o$, $x+2o$, $x+3o$, $x+4o$, &c. & quantitates $\overline{x+o}^{\frac{m}{n}}$, $\overline{x+2o}^{\frac{m}{n}}$, $\overline{x+3}^{\frac{m}{n}}$, $\overline{x+4o}^{\frac{m}{n}}$, &c. in ſeries infinitas expandantur; habebimus totidem ſeries, quarum prima hæc eſt, quæ ſequitur: $x^{\frac{m}{n}} + \frac{m}{n}ox^{\frac{m-n}{n}} + \frac{m^2-mn}{2n^2}oox^{\frac{m-2n}{n}} + \frac{m^3-3m^2n+2mn^2}{6n^3}ooox^{\frac{m-3n}{n}}$ &c. In omnibus ſeriebus primus terminus erit ipſa quantitas fluens $x^{\frac{m}{n}}$; & ſi prior quælibet ſeries à poſteriore auferatur, habebimus harum ſerierum differentias primas; in quibus omnibus primus terminus eſt ſeriei primæ terminus primus,

primus, quem ingreditur quantitas o; ſcilicet $\frac{m}{n}ox^{\frac{m-n}{n}}$: &, evaneſcente o, fit ille terminus differentiis hiſce primis æqualis; vel quod idem eſt, erit quantitas $\frac{m}{n}ox^{\frac{m-n}{n}}$ fluentis incrementum primum.

Præterea ſi differentia quælibet prior à poſteriore auferatur, deveniemus ad differentias ſecundas; quarum omnium terminus primus per 2 diviſus, idem eſt cum termino ſecundo ſeriei primæ, quem ingreditur quantitas o; &, evaneſcente o, fiunt differentiæ illæ per binarium diviſæ ſingulæ æquales termino illi primo ſeriei, qui eſt $\frac{m^2-mn}{2n^2}oox^{\frac{m-3n}{n}}$. Et eodem modo inveniemus ſuprà deſcriptæ ſeriei terminum $\frac{m^3-3m^2n+2mn^2}{6n^3}ooox^{\frac{m-3n}{n}}$, æqualem eſſe ſingulis differentiis tertiis, per ſex diviſis. Et quilibet terminus ejuſdem ſeriei ad differentias reſpectivas ſemper habebit datam rationem; ſcilicet terminus primus, quem ingreditur o, æqualis eſt differentiis primis; ſecundus eſt differentiarum ſecundarum pars media; tertius, pars ſexta differentiarum tertiarum, &c. Haſce ſeries, quarum termini differentias omnes in infinitum repræſentant, invenit Newtonus, uti dixi, ante annum 1665; ſed illæ ante viſam Newtoni epiſtolam, in quâ exponitur, D. Leibnitium * latebant: nam ante illud tempus agnoſcit Leibnitius, ſemper ipſi neceſſe fuiſſe tranſmutare quantitatem irrationalem in fractionem rationalem, & deinde, dividendo Mercatoris methodo, fractionem in ſeriem reducere. Exinde etiam patet ſeriem hanc, differentias continentem, non habuiſſe D. Leibnitium, quòd poſtquam ipſi per Oldenburgum oſtenſa eſt, * rogat, ut D. Newtonus ipſius originem ſibi pandat.

Sit jam quantitas quælibet, ex conſtanti & indeterminatis utcunque compoſita, & vinculo incluſa; ſcilicet $\overline{a+bx^c}\,\big|^{\frac{m}{n}}$; cujus differentia habenda eſt. Conſtat, per regulam priùs traditam, quantitatis $a+bx^c$ differentiam eſſe $cbx^{c-1}o$, poſito quòd o ſit incrementum momentaneum fluentis x. Quare ſi pro $a+bx^c$ ſcribatur z, & pro $cbx^{c-1}o$ ſcribatur ω, erit $\overline{a+bx^c+cbx^{c-1}o}\,\big|^{\frac{m}{n}} = \overline{z+\omega}\,\big|^{\frac{m}{n}}$; quæ ſi per regulam Newtoni in ſeriem expandatur, fit $z^{\frac{m}{n}} + \frac{m}{n}\omega z^{\frac{m-n}{n}}$ + &c. cujus ſeriei terminus $\frac{m}{n}\omega z^{\frac{m-n}{n}}$ eſt differentia prima quantitatis $z^{\frac{m}{n}}$, ſeu $\overline{a+bx^c}\,\big|^{\frac{m}{n}}$. Unde ſi loco z & ω reſtituantur ipſorum valores, $a+bx^c$ & $cbx^{c-1}o$, habebimus differentiam quantitatis $\overline{a+bx^c}\,\big|^{\frac{m}{n}} = \frac{m}{n}cbx^{c-1}o \times \overline{a+bx^c}\,\big|^{\frac{m-n}{n}}$: vel ſi more Leibnitiano pro o ponatur dx, erit quantitatis $\overline{a+bx}\,\big|^{\frac{m}{n}}$ differentia $\frac{m}{n}cbx^{c-1}dx \times \overline{a+bx^c}\,\big|^{\frac{m-n}{n}}$; ubi videmus quantitatem differentialem,

* Vide epiſtolam Leibnitii ad Oldenburgum 27 Auguſti 1676, pag. 537.

ferentialem, $\frac{m}{n} cbx^{c-1}dx$, extra vinculum ſemper manere. Atque hinc facile fuit D. Leibnitio, ope regulæ Newtonianæ, differentias quantitatum omnium exhibere, utcunque quantitates fluentes ſurdis aut fractionibus ſint implicatæ: id quod ante epiſtolicum illud, per Oldenburgum, cum Newtono commercium ipſi minimè notum fuit.

Quamvis hæc per ſe ſatis manifeſta ſunt calculi differentialis indicia; in ſecundâ tamen epiſtolâ, quæ per Oldenburgum ad Leibnitium miſſa eſt, alias adhuc clariores deſcribit Newtonus methodi ſuæ notas. Dicit enim, ſe habuiſſe methodum ducendi tangentes, quam ſolertiſſimus Sluſius ante annos duos treſve Oldenburgo impertitus eſt, ita ut habito ſuo fundamento nemo poſſet tangentes aliter ducere, niſi de induſtriâ à recto tramite erraret. Quinetiam ibi quoque oſtendit "Methodum hanc non hærere ad "æquationes, quibus una vel utraque quantitas indefinita radicalibus involuta eſt; ſed abſque ullâ æquationum reductione, quæ opus plerumque "redderet immenſum, tangentem confeſtim duci; & eodem modo in quæſtionibus de Maximis & Minimis, aliiſque quibuſdam, rem ſic ſe habere. "Fundamentum harum operationum dicit eſſe ſatis obvium, quod tamen "tranſpoſitis literis in illâ epiſtolâ celare voluit. Hoc etiam adjicit; hoc "fundamento ſpeculationes de quadraturis Curvarum ſimpliciores ſe reddiſſe; & ad theoremata quædam generalia ſe perveniſſe ſcribit."

Cùm verò methodus Sluſiana tunc temporis Leibnitium minimè latere potuit, utpote in Actis Philoſophicis Londini publicata: cúmque Newtonus dicit, eandem & ſibi innotuiſſe, ex fundamento, quo habito non hærebat ad æquationes radicalibus utcunque involutas; in quâ quidem tota rei difficultas poſita eſt: cúmque in priore epiſtolâ ſeriem deſcripſit, cujus ope differentiæ haberi poſſunt, ubi fluentes ſurdis aut fractionibus utcunque implicatæ ſunt: cùm denique idem fundamentum ad quadraturas Curvarum ſe applicuiſſe dicit: minimè dubitandum eſt, hæc omnia facem Leibnitio prætuliſſe, quo faciliùs methodum Newtoni perſpiceret.

N°LXXXIV. Quod ſi hæc non ſuffeciſſe videantur indicia; etiam ulterius proceſſit Newtonus, & exempla methodi ſuæ dedit, & regulam oſtendit, quâ ex datis quarundam Curvarum ordinatis, earundem areæ exhibentur in terminis finitis, cùm hoc fieri poteſt; hoc eſt, in ſtylo Leibnitiano, ipſi exempla tradidit, quibus à differentiis ad ſummas pervenitur. Et à ſimplicioribus orſus, * proponit primo parabolam, cujus abſciſſa eſt z, & ordinatim applicata $\sqrt{az} = a^{\frac{1}{2}}z^{\frac{1}{2}}$; & Curvæ area erit $\frac{2}{3}a^{\frac{1}{2}}z^{\frac{3}{2}}$; hoc eſt, quando differentia areæ eſt $dz \times \sqrt{az}$, ſeu $a^{\frac{1}{2}}z^{\frac{1}{2}} \times dz$, oſtendit fore aream $\frac{2}{3}a^{\frac{1}{2}}z^{\frac{3}{2}}$; unde viciſſim concluditur, ſi quantitas differentianda ſit $a^{\frac{1}{2}}z^{\frac{1}{2}}$, fore ejus differentiam $\frac{1}{2}a^{\frac{1}{2}}z^{\frac{1}{2}}dz$ ſeu $\frac{1}{2}dz\sqrt{az}$. Exemplum ejus ſecundum eſt Curva, cujus abſciſſa eſt z, & ordinatim applicata $\frac{a^4z}{c^2-z^2}$: ubi oſtendit Newtonus Curvæ aream fore $\frac{a^4}{2c^2-2z^2}$: hoc eſt, ſi differentia areæ ſit $\frac{a^4zdz}{\overline{c^2-z^2}^2}$, oſtendit aream

* Vide pag. 545.

fore $\frac{a^4}{2c - 2z}$. Unde viciſſim ſi quantitas differentianda ſit $\frac{a^4}{2c^2 - 2z^2}$, concludi poteſt differentiam fore $\frac{a^4z \times dz}{\overline{c^2 - z^2}^2}$. Vel ſi ejuſdem Curvæ ordinata ſic enuncietur, $\frac{a^4}{z^2 \times \overline{c^2 z^{-1} - 1}^2}$; erit area $= \frac{a^4 z^2}{2c^4 - 2c^2 z^2}$. Quare & viciſſim, ſi quantitas differentianda ſit $\frac{a^4 z^2}{2c^4 - 2c^2 z^2}$, erit differentia $\frac{a^4 dz}{\overline{z^2 \times c^2 z^{-1} - 1}^2}$.

Hinc ad exempla quædam difficiliora progreditur Newtonus; in iiſque oſtendit, quomodo ab ordinatis, hoc eſt à differentiis, ad ſummas perveniendum ſit: ex quibus patebit, Curvam omnem quadrabilem fore, cujus ordinata, in differentiam abſciſſæ ducta, ſit quantitatis alicujus differentia; & hinc innumera Curvarum genera aſſignari poſſunt, etiam geometricè quadrabilia.

His indiciis atque his adjutum exemplis, ingenium vulgare methodum Newtonianum penitus diſcerneret; ita ut ne ſuſpicari fas ſit, eam acerrimum Leibnitii acumen poſſe latuiſſe; quem quidem uſum fuiſſe his ipſis clavibus, ad hæc ſua quæ feruntur inventa, aditum, etiam ex ipſius ore ſatis eluceſcit. Nam in epiſtolâ ad Oldenburgum datâ, poſt explicatum calculum differentialem, exemplum addit, quod coincidere agnoſcit cum regulâ Sluſianâ; & poſtea addit. * "Sed methodus ipſa priore noſtrâ "longè eſt amplior: non tantùm enim exhiberi poteſt, cùm plures ſint li- "teræ indeterminatæ quàm x & y; quod ſæpe fit maximo cum fructu: ſed "& tunc utilis eſt, cùm interveniunt irrationales; quippe quæ eam nullo "morantur modo, neque ullo modo neceſſe eſt irrationales tolli; quod in "regulâ Sluſii neceſſe eſt, & calculi difficultatem in immenſum auget." Hæc omnia à Newtono priùs, in ſecundâ ejus epiſtolâ, dicta ſunt. Inde exempla proponit, quorum quidem quod primum eſt, neſcio quo fato, idem prorſus eſt ac id, quod, in eâ epiſtolâ quam Leibnitio tranſmiſerat Oldenburgus, etiam primum protulerit Newtonus.

Mox addit vir illuſtriſſimus, * "Arbitror, quæ celare voluit Newtonus "de tangentibus ducendis, ab his non abludere. Quod addit, ex hoc fun- "damento quadraturas quoque reddi faciliores, me in hâc ſententiâ con- "firmat. Nimirum ſemper figuræ illæ ſunt quadrabiles, quæ ſunt ad æ- "quationem differentialem. Æquationem differentialem voco talem, quâ "valor ipſius dx exprimitur, quæque ex aliâ derivata eſt, quâ valor ipſius "x exprimebatur." Et paulo pòſt, ſuam de hâc re ſententiam pleniùs aperit; dicitque hanc unicam regulam pro infinitis figuris quadrandis inſervire, diverſæ planè naturæ ab iis quæ hactenus quadrari ſolebant. Quis eſt jam, qui hæc perpendet, & non videbit, indicia & exempla Newtoni ſatis à Leibnitio perſpecta fuiſſe; ſaltem quoad differentias primas? Nam quoad differentias ſecundas, Leibnitium methodum Newtonianam tardiùs intellexiſſe videtur, quod brevi forſan clarius monſtrabo.

Interim facilè illuſtri viro aſſentior, & credo eum nec nomen calculi fluxionum fando audiviſſe; nec characteres, quos adhibuit Newtonus, oculis

* Vide pag. 560, 561.

vidiſſe, ante quàm in Walliſianis operibus prodiere. Obſervo enim, ipſum Newtonum ſæpius mutâſſe nomen & notationem calculi. In tractatu de Analyſi Æquationum per Series Infinitas, incrementum abſciſſæ per literam *o* deſignat: et in Principiis Philoſophiæ fluentem quantitatem genitam vocat, ejuſque incrementum momentum appellat: illam literis majoribus, A vel B, hoc minuſculis, *a* & *b*, deſignat.

Id etiam ultrà agnoſco; inter cætera quæ de re mathematicâ præclarè meritus eſt Leibnitius, hoc itidem illi deberi; quòd primus fuerit, qui calculum hunc typis edidit, & in publicum produxit: itaque eo ſaltem nomine magnam apud matheſeos amantes inibit gratiam, quòd inventum ita nobile, & in multiplices uſus deducendum, diutius eos noluerit latere.

Habes, Vir Cl. quæ de hoc argumento ſcribenda duxi; unde facilè credo percipies, hoc qualecunque fuerit meum in gentem noſtram ſtudium, ita parum præpoſterum fuiſſe, ut nihil omnino niſi quod Newtoni erat, Leibnitio detraxerim; nec dubito, quin æqui rerum æſtimatores uno ore fateantur me, uti nullo calumniandi animo, ita nec præcipiti judicio, ea dixiſſe, quæ tibi tot argumentis luce meridianâ clariùs comprobavi.

Lecta eſt hæc epiſtola coram Regiâ Societate, in conventu die 24 Maii 1711 *habito; & ut exemplar ejus D.* Leibnitio *mitteretur, D.* Sloane *Secretario ſuo mandatum eſt.*

N° LXXXV. *Epiſtola D.* Leibnitii *ad D.* Hans Sloane *M. D. & R. S. Sec.*

Quæ D. Johannes Keillius nuper ad te ſcripſit, candorem meum apertiùs quàm antè oppugnant: quem ut ego hâc ætate, poſt tot documenta vitæ, apologiâ defendam; & cum homine docto, ſed novo, & parum perito rerum anteactarum cognitore, nec * mandatum habente ab eo cujus intereſt, tanquam pro tribunali litigem; nemo prudens æquuſque probabit.

Quæ ille de meo rem cognoſcendi modo ſuſpicatur, haud ſatis exercitatus artis inveniendi arbiter, ipſius quidem docendi cauſâ non eſt cur refellam: ſed nôrunt † amici, quàm longè alio, & ad alia proficuo, itinere proceſſerim. Fruſtra ad exemplum Actorum Lipſienſium provocat, ut ſua dicta excuſet; in illis enim circa hanc rem quicquam cuiquam detractum non reperio, ſed potius paſſim ‡ ſuum cuique tributum. Ego quoque & amici

* Quaſi methodum Moutoni, & ſeries Brounkeri, Walliſii & Gregorii, aliorumque inventa non liceat propriis authoribus, niſi authoritate ab his acceptâ, aſſerere.

† Si amici illi ſunt Germani, invenit is hanc methodum poſt reditum ſuum in Germaniam.

‡ Scripſerit Keillius in hæc verba. *Hæc ut ſcriberem, impulerunt Actorum Lipſienſium Editores; qui, in eâ quam exhibent operis Newtoniani de fluxionibus ſeu quadraturis enarratione, diſertè affirmant, D Leibnitium fuiſſe iſtius methodi inventorem, & Newtonum aiunt pro differentiis Leibnitianis fluxiones adhibere ſemperque adhibuiſſe.* Leibnitius editores hîc palam defendit contra Keillium, quaſi ſuum cuique reddidiſſent.

§ In epiſtolâ Aug. 27, 1676, properavit ſe coinventorem methodi ſerierum proponere. In epiſtolâ Junii 21, 1677, properavit methodum ut ſuam deſcribere, de quâ Newtonus tractatum ante annos quinque ſcripſerat. In ſchedis tribus anno 1689 impreſſis, properavit propoſitiones principales Principiorum Philoſophiæ ad calculum ſuum revocatas in lucem edere, ut in inventoris jura veniret.

‖ Probandum eſt.

aliquoties

aliquoties oſtendimus, libenter à nobis credi, illuſtrem Fluxionum autorem per ſe ad ſimilia noſtris fundamenta perveniſſe. Neque eo minùs ego in inventoris jura venio; quæ etiam Hugenius, judex intelligentiſſimus incorruptiſſimuſque, publicè agnovit: in quibus tamen mihi vendicandis § non properavi; ſed inventum || pluſquam nonum in annum preſſi, ut nemo me præcucurriſſe queri poſſit.

Itaque veſtræ æquitati committo, annon coercendæ ſint vanæ & injuſtæ vociferationes; quas ** ipſi Newtono, viro inſigni, & geſtorum optimè conſcio, improbari arbitror: ejuſque ſententiæ ſuæ libenter daturum indicia mihi perſuadeo.

VALE. Dabam Hannoveræ 29 Decemb. 1711.

Cùm D. Leibnitius à D. Keill, ut homine novo, ad Societatem Regiam pro- N°LXXXVI.
vocaret; Societas juſſit monumenta antiquiora conſuli; & ſociis aliquot, qui his examinandis aptiores viderentur, in mandatis dedit, ut in hanc rem inquirerent; & quæ in ſcriptis antiquis invenirent, ad ſe referrent, unà cum eorum ſententiâ. Et arbitrorum conſeſſus collectionem ex epiſtolis & aliis MSS. ſuprà impreſſam ad Societatem retulerunt, unà cum eorum ſententiâ ſequente.

We have conſulted the letters and letter-books in the cuſtody of the Royal Society, and thoſe found among the papers of Mr. John Collins, dated between the years 1669 *and* 1677 *incluſive; and ſhewed them to ſuch as knew and avouched the hands of Mr. Barrow, Mr. Collins, Mr. Oldenburg, and Mr. Leibnitz; and compared thoſe of Mr. Gregory with one another, and with copies of ſome of them taken in the hand of Mr. Collins; and have extracted from them what relates to the matter referred to us; all which extracts herewith delivered to you, we believe to be genuine and authentick. And by theſe letters and papers we find,*

1. *That Mr. Leibnitz was in London in the beginning of the year* 1673; *and went thence in or about March to Paris; where he kept a correſpondence with Mr. Collins, by means of Mr. Oldenburg, till about September* 1676, *and then returned by London and Amſterdam to Hanover: and that Mr. Collins*

** Newtonus & Leibnitius nec ſunt idonei judices nec teſtes. Ex monumentis antiquis judicium ferendum eſt.

LITERAS & literarum apographa, tam quæ in archivis Regiæ Societatis, quàm quæ inter chartas D. Joannis Collinii aſſervantur, & inter annos 1669 & 1677 datæ ſunt, inſpeximus; & ex his, quæ D. Barrovii, D. Collinii, D. Oldenburgi & D. Leibnitii nomen ferebant, ex fide aliquorum qui eorum autographa probè noverant, ipſorum eſſe certo didicimus. Literas autem, quæ Gregorium præ ſe ferebant auctorem, ipſius eſſe cognovimus fide Collinii; qui nonnullas earum, Gregorio aſſignatas, manu ſuâ exſcripſerat. Ex his omnibus excerpſimus, quæcunque ad rem nobis commiſſam pertinere videbantur; atque illa excerpta, quæ unà cum ipſis literis jam vobis traduntur, fideliter & accuratè facta eſſe comperimus. Ex his autem literis chartiſque nobis conſtat;

I. D. Leibnitium, anno ineunte 1673, Londini fuiſſe; unde menſe Martio, vel circiter, Pariſios adiit; ubi literarum commercium habuit cum D. Collinio, intercedente Oldenburgo, uſque in menſem Septembrem 1676: deinde per Londinum & Amſtelodamum Hannoveram reverſum eſſe: D. autem Collinium matheſeos peritis ea, quæ à D. Newtono & Gregorio acceperat, lubentiſſimè communicâſſe.

was very free in communicating, to able mathematicians, what he had received from Mr. Newton and Mr. Gregory.

II. *That when Mr. Leibnitz was the first time in London, he contended for the invention of another Differential Method properly so called; and notwithstanding that he was shewn by Dr. Pell, that it was Mouton's method, persisted in maintaining it to be his own invention, by reason that he had found it by himself, without knowing what Mouton had done before, and had much improved it. And we find no mention of his having any other Differential Method than Mouton's, before his letter of the 21st of June 1677, which was a year after a copy of Mr. Newton's letter, of the 10th of December 1672, had been sent to Paris to be communicated to him; and above four years after Mr. Collins began to communicate that letter to his correspondents; in which letter, the method of Fluxions was sufficiently described to any intelligent person.*

III. *That by Mr. Newton's letter of the 13th of June 1676 it appears, that he had the method of Fluxions above five years before the writing of that letter. And by his* Analysis per Æquationes numero Terminorum Infinitas, *communicated by Dr. Barrow to Mr. Collins in July 1669, we find that he had invented the method before that time.*

IV. *That the Differential Method is one and the same with the Method of Fluxions, excepting the name and mode of Notation; Mr. Leibnitz calling those quantities Differences, which Mr. Newton calls Moments or Fluxions; and marking them with the letter* d, *a mark not used by Mr. Newton. And therefore we take the proper question to be, not who invented this or that method, but who was the first inventor of the method. And we believe, that those who have reputed Mr. Leibnitz the first inventor, knew little or nothing of his correspondence with Mr. Collins and Mr. Oldenburg long before; nor of Mr. Newton's having that method above fifteen years before Mr. Leibnitz began to publish it in the* Acta Eruditorum *of Leipsick.*

For which reason, we reckon Mr. Newton the first inventor; and are of opinion, that Mr. Keill, in asserting the same, has been no ways injurious to Mr. Leibnitz. And we submit to the judgment of the Society, whether the extract and papers now presented to you, together with what is extant to the same purpose in Dr. Wallis's third volume, may not deserve to be made publick.

His

II. D. Leibnitium, cùm primâ vice Londinum adiit, methodi cujusdam differentialis, propriè sic dictæ, se inventorem perhibuisse: et etiamsi D. Pellius ipsi monstraverat, eandem antea à D. Moutono usurpatam fuisse, haud tamen sibi inventoris jura asserere destitisse; cum quia proprio, ut aiebat, marte sua illa inveniisset, nondum visis iis quæ Moutonus priùs ediderat, tum quia plurima adjeciisset. Neque usquam mentionem reperimus factam alterius methodi ejus differentialis, præter istam Moutoni, ante literas ejus 21 Junii 1677 datas; hoc est, anno integro postquam D. Newtoni epistola, 10 Decembris 1672 scripta, Parisios, ipsi communicanda, transmissa fuit; & quadriennio postquam D. Collinius eandem epistolam cum amicis communicare cœpit. In hâc autem epistolâ methodus fluxionum, idoneo harum rerum cognitori, evidenter satis describitur.

III. Ex literis D. Newtoni, 13 Junii 1676 datis, manifestum est, fluxionum methodum ipsi innotuisse, quinquennio prius quàm epistolam illam describeret. Et ex Analysi ejus *per Æquationes numero Terminorum Infinitas*, quam D. Barrovius cum D. Collinio mense Julio anni 1669 communicavit, constat illum etiam ante illud tempus eandem excogitâsse.

IV. Methodus Differentialis una eademque est cum Methodo Fluxionum, si nomen & notationis modum excipias. D. Leibnitius enim eas quantitates differentias appellat, quas D. Newtonus

His autem die Aprilis 24, 1712, acceptis, Societas Regia collectionem epiſtolarum & MSS. & ſententiam Conſeſſûs imprimi juſſit; ut & quicquid amplius, ad hanc hiſtoriam elucidandam idoneum, in *Actis Eruditorum* occurreret.

tonus Momenta vel Fluxiones; eaſque notâ literæ [d] deſignat, quam non adhibet D. Newtonus. Rem proinde de quâ agimus hanc autumamus eſſe; non uter hanc, uter illam methodum invenerit; ſed uter methodum ipſam, quæ unica eſt, prior invenerit. Simul illos, qui D. Leibnitium pro inventore primo habuere, de eo, quod inter illum & D. Collinium olim interceſſerat, commercio, parum aut nihil reſciviſſe opinamur; neque intellexiſſe, D. Newtonum eâdem methodo uſum eſſe, quindecim prius annos quàm D Leibnitius eam in *Actis Eruditorum* Lipſiæ evulgare cœpit.

Quibus perpenſis, D. Newtonum primum eſſe hujus methodi inventorem arbitramur; atque ideo D. Keillium, eandem illi aſſerendo, nullo modo D. Leibnitium calumniâ aut injuriâ affeciſſe. Judicio autem Societatis permittimus, utrumne excerpta literarum, reliquæque chartæ his ſubnexæ, unà cum iis quæ extant in tertio volumine Operum D. Walliſii huc ſpectantibus, ſimul imprimi, & in publicum prodire mereantur.

APPENDIX.

APPENDIX.

HIC subjungere visum est Judicium Mathematici 7 Julii 1713 datum; & chartâ volante, sine nomine autoris, per orbem sparsum.

‘ VIDETUR Newtonus occasionem nactus, serierum opus multum promovisse per extractiones radicum, quas primus in usum adhibuit; & quidem in iis excolendis, ut verisimile est, ab initio omne suum studium posuit; nec credo tunc temporis vel somniavit adhuc de calculo suo fluxionum & fluentium, vel de reductione ejus ad generales operationes analyticas, ad instar algorithmi vel regularum arithmeticarum aut algebraicarum. Ejusque meæ conjecturæ [primum] validissimum indicium est, quod de literis x vel y punctatis, uno, duobus, tribus, &c. punctis superpositis, quas pro dx, ddx, d^3x; dy, ddy, &c. nunc adhibet, in omnibus istis epistolis (Commercii Epistolici, unde argumenta ducere volunt] nec volam, nec vestigium invenias. Imo ne quidem in Principiis Naturæ Mathematicis Newtoni, ubi calculo suo fluxionum utendi tam frequentem habuisset occasionem, ejus vel verbulo fit mentio, aut notam hujusmodi unicam cernere licet; sed omnia fere per lineas figurarum, sine certâ analysi, ibi peraguntur, more non ipsi tantùm, sed & Hugenio, imo jam antea [in nonnullis] dudum Torricellio, Roburvallio, Cavallerio, aliis, usitato. Primâ vice hæ literæ punctatæ comparuerunt in tertio Volumine operum Wallisii, multis annis postquam calculus differentialis jam ubique locorum invaluisset. Alterum indicium, quo conjicere licet, calculum fluxionum non fuisse natum ante calculum differentialem, hoc est; quòd veram rationem fluxiones fluxionum capiendi, hoc est differentiandi differentialia, Newtonus nondum cognitam habuerit; quod patet ex ipsis Principiis Phil. Math. ubi non tantùm incrementum constans ipsius x, quod nunc notaret per x punctatum uno puncto, designat per o [more vulgari, qui calculi differentialis commoda destruit] sed etiam regulam circa gradus ulteriores falsam dedit [quemadmodum ab eminente quodam Mathematico dudum notatum est] ..... Saltem apparet Newtono rectam methodum differentiandi differentialia non innotuisse, longo tempore postquam aliis fuisset familiaris.”

Hactenus Judicium.

ANNOTATIO.

ANNOTATIO.

HÆC omnia refutantur ſupra, pag. 454—458, 467—483, 489, 503, 509, 510, 512, 535, 536, 544, 581, 582, 585, 586.

Methodum fluxionum utique non conſiſtit in formâ ſymbolorum. Et Keillius hoc notaverat anno 1711 (pag. 477, 585, 586.) Pro fluxionibus ipſarum x, y, z, Newtonus quandoque ponit eaſdem literas punctis notatas, $\dot{x}$, $\dot{y}$, $\dot{z}$; quandoque eaſdem formâ majuſculâ X, Y, Z; quandoque literas alias, ut p, q, r; quandoque lineas exponentes, ut DE, FG, HI (pag. 478.) Et hoc Newtonus in hunc uſque diem facit, ut videre licet in libro de quadraturis; ubi fluxiones in propoſitione primâ denotantur per literas punctatas; in ultimâ, per ordinatas Curvarum; in introductione, per alia ſymbola; dum Newtonus ibi methodum fluxionum explicat illuſtratque per exempla (pag. 478.) Pro fluxionibus D. Leibnitius nulla habet ſymbola. Is momentorum, ſive differentiarum ſymbola, dx, dy, dx, primo cœpit adhibere anno 1677; Newtonus momenta denotabat per rectangula ſub fluxionibus & momento o, cùm analyſin ſuam ſcriberet; anno ſcilicet 1669, aut antea. Leibnitius ſymbolis $\int x$, $\int y$, $\int z$ pro ſummis ordinatarum uſus eſt jam inde ab anno 1686: Newtonus, in analyſi ſuâ, eandem rem denotavit, inſcribendo ordinatam in quadrato vel rectangulo ad hunc modum; $\left|\frac{aa}{64x}\right|$. Omnia Newtoni ſymbola ſunt in ſuo genere prima; & ſymbola Leibnitii nondum obtinuerunt in Angliâ (pag. 477, 478.)

In Principiis naturæ Mathematicis Newtonus analytico ſuo fluxionum calculo utendi non habuit frequentem occaſionem. Nam liber ille inventus eſt quidem per analyſin; at ſcriptus eſt per ſyntheſin, more Veterum, ut oportuit (pag. 479.) At analyſis tamen ita elucet per ſyntheſin illam, ut Leibnitius ipſe olim agnoverit, Newtonum non ſolùm methodo ſuâ tangentes duxiſſe, ſed majora multo conſecutum, viſo demum Libro Principiorum, ſe ſatis intellexiſſe (pag. 489.) Et in epiſtolâ ſuâ 28 Maii 1697 ad Walliſium ſcripta: "Methodum, inquit, profundiſſimi Newtoni "cognatam eſſe methodo meæ differentiali non tantùm animadverti, poſtquam opus ejus [Prin- "cipiorum ſcilicet] & tuum prodiit, ſed etiam profeſſus ſum in Actis Eruditorum, & aliâs quo- "que monui. Id enim candori meo convenire judicavi, non minus quàm ipſius merito. Itaque "communi nomine deſignare ſoleo Analyſeos Infiniteſimalis" (pag. 483.) Et alibi de ſublimi quâdam parte methodi, quâ Newtonus Solidum minimæ reſiſtentiæ invenerat, hæc habet verba. "Quam methodum, ante D. Newtonum & me, nullus quod ſciam geometra habuit; uti ante "hunc, maximi nominis geometram, nemo ſe habere PROBAVIT" (pag. 745.) Et in epiſtolâ ad Newtonum Hannoveræ datâ 7 Mart. 1693, ita ſcripſit: "Mirificè ampliaveras geometriam "tuis ſeriebus; ſed edito Principiorum opere offendiſti, patere tibi, quæ analyſi receptæ non "ſubſunt. Conatus ſum ego quoque, notis commodis adhibitis, quæ differentias & ſummas ex- "hibent, geometriam illam, quam tranſcendentem appello, analyſi quodammodo ſubjicere; nec "res male ſucceſſit" (pag. 472.) Atque iterum in reſponſo ad D. Fatium, quod habetur in Actis Eruditorum Maii 1700, pag. 203, lin. 21, id faſſus eſt Leibnitius (pag. 472.) Sed & ſectiones duas primas libri ſecundi Principiorum verbis aliis (abſque ſymbolis differentialibus) compoſuit, & ſubjunxit; "ſe jam fundamenta geometrica jeciſſe, & vias quaſdam novas, ſatis antea "impeditas, aperuiſſe; omnia autem reſpondere ſuæ analyſi infinitorum, hoc eſt calculo ſumma- "rum ac differentiarum" (pag. 481—483.) Et hoc fuit ſpecimen omnium primum methodi differentialis, quod D. Leibnitius, circa problemata ſublimiora orbi literario exhibuit.

Literæ punctatæ primâ vice comparuerunt, non (ut hîc dicitur) in tertio volumine Operum Walliſii, quod prodiit anno 1699; ſed in ſecundo volumine Operum ejus, quod prodiit anno 1693 (pag. 457, 481.) annis utique duobus antequam fama calculi differentialis ad aures Walliſii pervenerit, & annis tribus antequam Marchio Hoſpitalius Analyſin ſuam infinitè parvorum ediderit, quâ calculus differentialis ubique locorum invaleſcere cæpit (pag. 469, 473, 474, 480, 481.)

Newtonus nunquam mutavit literam o in literam $\dot{x}$ punctatam uno puncto, ſed literâ illâ o uſus eſt in introductione ad Librum de Quadraturis, & adhuc utitur in eodem ſenſu ac ſub initio; id-

que maximo cum fructu. Est enim *o* symbolum unicum, quo Newtonus utitur pro quantitate infinitè parvâ: at symbolum *x* quantitatem finitam designat (pag. 459, 456, 477—479.)

Methodus fluxiones omnes capiendi, seu differentiandi differentialia, habetur in propositione primâ libri de quadraturis: & est verissima, & optima. Eandem, cum exemplis in differentiis primis & secundis, Wallisius edidit in tomo secundo operum suorum anno 1693, ut suprà; annis scilicet tribus antequam regula Leibnitii differentiandi differentialia lucem viderit (pag. 457, 480, 481.) Eandem regulam Newtonus, anno 1686, demonstravit syntheticè in Lem. II. Lib. 2. Princip. (pag. 472) & posuit in epistolâ suâ ad Oldenburgum 24 Octob. 1676, tanquam fundamentum hujus methodi, de quâ tum ante annos quinque scripserat (pag. 455.) Et specimen ejusdem, quoad tangentes ducendas, posuit in epistolâ suâ ad Collinium 10 Decemb. 1672 (pag 510.) Et in eâdem epistolâ addidit problemata de Curvarum curvaturâ, seu geometricarum seu mechanicarum, per eandem methodum solvi (pag. 510.) Ex quo manifestum est, se jam tum suam methodum ad secunda ac tertia momenta extendisse. Cùm enim areæ Curvarum considerantur tanquam fluentes, ut in hâc analysi fieri solet, ordinatæ exprimunt fluxiones primas, tangentes autem datæ sunt per fluxiones secundas, & Curvaturæ per tertias (pag. 456.) Et anno 1669, in analysi suâ per series, Newtonus dixit: "Momentum est superficies, cùm de Solidis; & linea, cùm de superficiebus; & punctum, cùm "de lineis agitur:" quod perinde est, ac si dixisset: cùm Solida considerantur tanquam fluentia, eorum momenta sunt superficies; & horum momentorum momenta, vel secunda Solidorum momenta, sunt lineæ; & horum momenta, sive tertia Solidorum momenta, sunt puncta; adeoque quâ ratione momenta prima derivantur à fluentibus, secunda derivantur à primis, tertia à secundis, & sic deinceps in infinitum. Et quomodo momenta prima derivantur à fluentibus, ostenditur in analysi per series; inveniendo ordinatas curvilinearum ex areis (pag. 454—458, 503.)

In eâdem analysi Newtonus posuit secundam propositionem libri de quadraturis (pag. 581, lin. 30) dixitque "Curvarum areas & longitudines, id modò fiat, beneficio ejusdem methodi analyseos exactè "& geometricè determinari" (p. 503, lin. 2.) Et methodus hæcce Newtono innotuit annis aliquot antea, testibus Barrovio & Collinio (pag. 509, lin. 20, 21, 22, 23, 25) id est anno 1666, aut antea. Hæc methodus aliquatenus explicatur in epistolâ Newtoni ad Oldenburgium 24 Octob. 1676 datâ; ibique ex propositione primâ libri de quadraturis, illic ænigmaticè descriptâ, consequi dicitur (pag. 535, 536) & in propositione quintâ & sextâ libri illius plenius explicatur; & hæ propositiones ex propositionibus quatuor primis libri ejusdem consequuntur: ideoque methodus fluxionum, quatenus in propositionibus quinque vel sex primis libri de quadraturis exponitur, Newtono innotuit anno 1666 aut antea, testibus Barrovio & Collinio; ut & teste etiam Wallisio (pag. 474.) Sed & Marchio Hospitalius pro Newtono testis est; qui utique dixit, librum Principiorum Philosophiæ fere totum esse ex hoc calculo (pag. 472) & Leibnitium in methodum differentialem incidisse, efficiendo, ut methodus tangentium Barrovii non hæreret ad radicales (pag. 469—471) Newtonus enim, per epistolas 10 Decemb. 1672, & 24 Octob. 1676, Leibnitium admonuit, se hoc antea affecutum esse (pag. 510, 544.)

Ex iis etiam, quæ in epistolâ ad Oldenburgum 24 Octob. 1676 datâ, de tabulis figurarum curvilinearum, in scholio propositionis decimæ libri de quadraturis positarum, dicuntur; liquet methodum fluxionum & momentorum, quatenus in decem primis libri illius propositionibus habetur, diu ante annum 1676 Newtono innotuisse (pag. 581.) Id quod etiam colligi potest ex Corol 2. Prop. x. libri de quadraturis; quod utique in epistolâ Newtoni ad Collinium Nov. 8, 1676 datâ, & anno 1711 à Jonesio editâ, descriptum habetur.

FINIS COMMERCII EPISTOLICI.

ADDI-

ADDITAMENTA

COMMERCII EPISTOLICI

EX HISTORIA FLUXIONUM RAPHSONI.

PRÆFATUR RAPHSONUS.

Epiſtolæ ſequentes à D. Leibnitio *cum amicis ſuis in Gallia & alibi communicatæ, ad controverſiam præcedentem ſpectant.*

Prima ac tertia eſt ipſius Leibnitii; *ſecunda* Newtoni; *omnes ad amicum communem.*

Prima, circa menſem Novembrem vel Decembrem anni 1715 *ſcripta, non eſt epiſtola tota, ſed Additamentum.*

" VOILA', Monſieur, la lettre dont vous pourrez faire uſage, ſi vous
" le jugez à propos. Je viens maintenant à ce qui nous regarde.
" Je ſuis ravi que vous eſtes en Angleterre; il y a dequoy profiter; & il
" faut avouer qu'il y a là de trés habiles gens; mais ils voudroient paſſer
" pour étre preſque ſeuls inventeurs; & c'eſt in quoy apparemment ils ne
" reuſſiront pas. Il ne paroiſt point que M. Newton ait eu avant moy la
" characteriſtique & l'algorithme infiniteſimal, ſuivant ce que M. Ber-
" noulli a tres bien jugé: quoyqu'il luy auroit eté fort aiſé d'y parvenir, s'il
" s'en fut aviſé. Comme il auroit eté fort aiſé à Apollonius de parvenir à
" l'Analyſe de Des Cartes ſur les courbes, s'il s'en etoit aviſé. Ceux qui
" ont ecrit contre moy n'ayant pas fait difficulté d'attaquer ma candeur,
" par des interpretations forcées & mal fondées; ils n'auront point le
" plaiſir de me voir répondre à de petites raiſons de gens, qui en uſent ſi
" mal, & qui d'ailleurs s'ecartent du fait. Il s'agit du calcul des differen-
" ces, & ils ſe jettent ſur les ſeries, où M. Newton m'a precedé ſans dif-
" ficulté; mais je trouvay enfin une methode generale pour les ſeries, &
" aprés cela je n'avoit plus beſoin de recourir à ſes extractions. Ils au-
" roient mieux fait de donner les lettres entieres, comme M. Wallis a fait
" avec mon conſentement, & il n'a pas eu la moindre diſpute avec moy,
" comme ces gens la voudroient perſuader au public. Mes adverſaires
" n'ont publié du *Commercium Epiſtolicum* de M. Collins, que ce qu'ils
" ont crû capable de recevoir leur mauvaiſes interpretations. Je fis con-
" noiſſance avec M. Collins dans mon ſecond voyage d'Angleterre; car au
" premier (qui dura tres peu, parceque j'etois venu avec un miniſtre pub-
" lic) je n'avois pas encore la moindre connoiſſance de la geometrie avan-
" cée, & n'avois rien vû, ni entendu, du commerce de M. Collins avec Meſſ.
" Gregory & Newton; comme mes lettres echangées avec M. Oldenbourg
" en ce temps là, & quelque temps apres, feront aſſez voir. Ce n'eſt
" qu'en France que j'y ſuis entré, & M. Hugens m'en donna l'entrée.
" Mais a mon ſecond voiage, M. Collins me fit voir une partie de ſon
" commerce, & j'y remarquay que M. Newton avoua auſſi ſon ignorance
" ſur pluſieurs choſes; & dit entre autres, qu'il n'avoit rien trouvé ſur la

" dimenſion des curvilignes celebres que la dimenſion de la ciſſoide. Mais
" on a ſupprimé tout cela. Je ſuis faché qu'un auſſi habile homme que
" M. Newton s'eſt attiré la cenſure des perſonnes intelligentes, en defe-
" rant trop aux ſuggeſtions de quelque flatteurs, qui l'ont voulu brouiller
" avec moy.

" Sa philoſophie me paroit un peu étrange, & je ne crois pas qu'elle
" puiſſe s'etablir. Si tout corps eſt grave, il faut neceſſairement (quoyque
" diſent ſes deffenſeurs, & quelque emportement qu'ils temoignent) que la
" gravité ſoit une qualité occulte ſcholaſtique, ou l'effect d'un miracle.
" J'ay fait voir autrefois à M. Bayle, que tout ce qui n'eſt pas explicable par
" la nature des creatures eſt miraculeux. Il ne ſuffit pas de dire, Dieu à
" fait une telle loy de nature, donc la choſe eſt naturelle. Il faut que la
" loy ſoit executable par les natures des creatures. Si Dieu donnoit cette
" loy, par exemple à un corps libre, de tourner à l'entour d'un certain
" centre, il faudroit ou qu'il y joignit d'autres corps, qui par leur impul-
" ſion l'obligeaſſent reſter toujours dans ſon orbite circulaire, ou qu'il mit
" une ange à ſes trouſſes; on enfin il faudroit, qu'il y concourut extraor-
" dinairement. Car naturellement il s'ecartera par la tangente. Dieu
" agit continuellement ſur les creatures par la conſervation de leur natu-
" res, & cette conſervation eſt une production continuelle de ce qui eſt
" perfection en elles. Il eſt *intelligentia ſupramundana* parce qu'il n'eſt
" pas l'ame du monde, & n'a pas beſoin de *ſenſorium*.

" Je ne trouve pas le vuide demonſtré par les raiſons de M. Newton, ou
" de ſes ſectateurs; non plus que la pretendue gravité univerſelle, ou que
" les atomes. On ne peut donner dans le vuide & dans les atomes, que
" par des vues trop bornées. M. Clarke diſpute contre le ſentiment des
" Carteſiens, qui croyent que Dieu ne ſauroit déſtruire une partie de la ma-
" tiere, pour faire un vuide; mais je m'etonne qu'il ne voye point, que ſi
" l'eſpace eſt une ſubſtance differente de Dieu, la même difficulté s'y trou-
" ve. Or de dire que Dieu eſt l'eſpace, c'eſt luy donner des parties.
" L'eſpace eſt l'ordre des coexiſtences; & le temps eſt l'ordre des exiſtences
" ſucceſſives. Ce ſont des choſes veritables, mais ideales; comme les
" nombres.

" La matiere même n'eſt pas une ſubſtance, mais ſeulement *ſubſtantia-*
" *tum*, un phenomene bien fondé, & qui ne trompe point, quand on y pro-
" cede en raiſonnant ſuivant les loix ideales de l'arithmetique, de la geome-
" trie, & de la dynamique, &c. Tout ce que j'advance en cela paroiſt dé-
" montré. A propos de la dynamique, ou de la doctrine des forces, je
" m'etonne que M. Newton & ſes ſectateurs croyent que Dieu a ſi mal
" fait ſa machine, que s'il n'y mettoit la main extraordinairement, la
" montre ceſſeroit bien tôt d'aller. C'eſt d'avoir des idées bien étroites de
" la ſageſſe & de la puiſſance de Dieu. J'appelle extraordinaire toute ope-
" ration de Dieu, qui demande autre choſe que la conſervation des natures
" des creatures. Ainſi quoy que je croye la metaphyſique de ces meſſieurs
" là, *a narrow one*, & leur mathematique aſſez *arrivable*; je ne laiſſe pas
" d'eſtimer extremement les meditations phyſico-mathematiques de M.
" Newton;

" Newton; & vous obligeriez infiniment le public, monſieur, ſi vous " portiez cet habile homme â nous donner juſqu'a ſes conjectures en phy- " ſique. J'approuve fort ſa methode de tirer des phenomenes, ce qu'on en " peut tirer ſans rien ſuppoſer, quand même ce ne ſeroit quelquefois que " tirer des conſequences conjecturales. Cependant quand les *data* ne ſuf- " fiſent point, il eſt permis (comme on fait quelquefois en déchifrant) " d'imaginer des hypotheſes; & ſi elles ſont heureuſes, on s'y tient provi- " ſionellement, en attendant que des nouvelles experiences nous appor- " tent *nova data*, & ce que Bacon apelle *Experimenta Crucis*, pour choiſir " entre les hypotheſes. Comme j'apprends que certain Anglois ont mal " repreſenté ma philoſophie dans leur Tranſactions, je ne doute point qu'- " avec ce que je vous mande icy, je ne puiſſe etre juſtifié. Je ſuis fort " pour la philoſophie experimentale, mais M. Newton s'en ecarte fort, " quand il pretend que toute la matiere eſt peſante, ou que chaque partie " de la matiere attire chaque autre partie; ce que les experiences ne prou- " vent nulment, comme M. Hugens à deja fort bien jugé, la matiere " gravifique ne ſauroit avoir elle même cette peſanteur, dont elle eſt la " cauſe, & M. Newton n'apporte aucune experience, ni raiſon ſuffiſante, pour " le vuide & les atomes, ou pour l'attraction mutuelle generale. Et parce " qu'on ne ſait pas encore parfaitement, & en detail, comment ſe produit " la gravité, ou la force elaſtique, ou la magnetique, &c; on n'a pas rai- " ſon pour cela d'en faire des qualitez occultes ſcholaſtiques, ou des mira- " cles; mais on à encore moins raiſon de donner des bornes à la ſageſſe & " à la puiſſance de Dieu, & de luy attribuer un *ſenſorium* & choſes ſembla- " bles. Au reſte, je m'etonne que les ſectateurs de M. Newton ne don- " nent rien, qui marque que leur maiſtre leur à communiqué une bonne " methode j'ay eté plus heureux en diſciples."

Leiceſter-Fields,

SIR, *London*; *Feb.* 26, $17\frac{15}{16}$.

YOU know that the *Commercium Epiſtolicum* contains the antient letters and papers preſerved in the Archives and letter-books of the Royal Society, and library of Mr. Collins, relating to the diſpute between Mr. Leibnitz and doctor Keill; and that they were collected and publiſhed by a numerous committee of gentlemen of ſeveral nations, appointed by the Royal Society for that purpoſe. Mr. Leibnitz has hitherto avoided returning an anſwer to the ſame; for the book is matter of fact, and uncapable of an anſwer. To avoid anſwering it, he pretended the firſt year that he had not ſeen this book, nor had leiſure to examine it, but had deſired an eminent mathematician to examine it. And the anſwer of the mathematician (or pretended mathematician) dated June 7, 1713, was inſerted into a defamatory letter dated July 29 following, and publiſhed in Germany without the name of the author or printer, or city where it was printed. And the whole has been ſince tranſlated into French, and inſerted into another

other abuſive letter (of the ſame author, as I ſuppoſe) and anſwered by Dr. Keill in July 1714; and no anſwer is yet given to the doctor.

Hitherto Mr. Leibnitz avoided returning an anſwer to the *Commercium Epiſtolicum*, by pretending that he had not ſeen it. And now he avoids it, by telling you, that the Engliſh ſhall not have the pleaſure to ſee him return an anſwer to their ſlender reaſonings (as he calls them) and by endeavouring to engage me in diſputes about Philoſophy, and about ſolving of Problems; both which are nothing to the queſtion.

As for Philoſophy, he colludes in the ſignifications of words, calling thoſe things miracles, which create no wonder; and thoſe things occult qualities, whoſe cauſes are occult, though the qualities themſelves be manifeſt; and thoſe things the ſouls of men, which do not animate their bodies. His *Harmonia Præſtabilita* is miraculous, and contradicts the daily experience of all mankind; every man finding in himſelf a power of ſeeing with his eyes, and moving his body by his will. He prefers hypotheſes to arguments of induction drawn from experiments; accuſes me of opinions which are not mine; and inſtead of propoſing queſtions to be examined by experiments before they are admitted into Philoſophy, he propoſes hypotheſes to be admitted and believed, before they are examined: but all this is nothing to the *Commercium Epiſtolicum*.

He complains of the committee of the Royal Society, as if they had acted partially in omitting what made againſt me; but he fails in proving the accuſation. For he inſtances in a paragraph concerning my ignorance, pretending that they omitted it, and yet you will find it in the *Commercium Epiſtolicum*, pag. 547, lin. 2, 3, and I am not aſhamed of it. He ſaith, that he ſaw this paragraph in the hands of Mr. Collins when he was in London the ſecond time; that is, in October 1676. It is in my letter of the 24th of Octob. 1676, and therefore he then ſaw that letter. And in that, and ſome other letters writ before that time, I deſcribed my method of fluxions. And in the ſame letter I deſcribed alſo two general methods of ſeries, one of which is now claimed from me by Mr. Leibnitz.

I believe you will think it reaſonable, that Mr. Leibnitz be conſtant to himſelf; and ſtill acknowledge what he acknowledged above 15 years ago; and ſtill forbear to contradict what he forbore to contradict in thoſe days.

In his letter of the 20th of May 1675, he acknowledged the receipt of a letter from Mr. Oldenburg, dated the 15th of April 1675, with ſeveral converging ſeries contained therein. And I expect from him, that he ſtill acknowledge the receipt thereof. Many gentlemen of Italy, France, and Germany (yourſelf being one of them) have ſeen the original letters, and the entries thereof in the old letter-books of the Royal Society; and the Series of Gregory is in the letter of the 15th of April 1675, and in Gregory's original letter, dated the 15th of Feb. 1671.

In a letter dated the 12th of May 1676 (ſeen by the ſame gentlemen) he acknowledged that he then wanted the method for finding a Series for the arc whoſe ſine was given; and, by conſequence, that he wanted it when he

he wrote his letter of the 24th of October 1674; and I expect that he ſtill acknowledge it.

In the *Acta Eruditorum* for May 1700, in anſwer to Mr. Fatio, who had ſaid, that I was the oldeſt inventor by many years, Mr. Leibnitz acknowledged that nobody, ſo far as he knew, had the method of fluxions or differences before me and him; and that nobody before me had proved, by a ſpecimen made publick, that he had it. Here he allowed, that I had the method before it was publiſhed, or communicated by him to any body in Germany; that the *Principia Philoſophiæ* were a proof that I had it, and the firſt ſpecimen made publick of applying it to the different problems: and I expect, that he ſtill continue to make the ſame acknowledgment. At that time he did not deny what Mr. Fatio affirmed, and nothing but want of candour can make him unconſtant to himſelf.

In a letter to me dated the 7th of March 1693, and now in the cuſtody of the Royal Society, he wrote, *Mirificè amplivaveras geometriam tuis ſeriebus; ſed edito Principiorum opere, oſtendiſti patere tibi, quæ analyſi receptæ non ſubſunt. Conatus ſum ego quoque, notis commodis adhibitis, quæ differentias & ſummas exhibent, geometriam illam quam tranſcendentem appello, analyſi quodammodo ſubjicere, nec res malè proceſſit.* And what he then acknowledged, he ought ſtill to acknowledge.

In his letter of the 21ſt of June 1677, writ in anſwer to mine of the 24th of October 1676, wherein I deſcribed my method partly in plain words, and partly in cyphers; he ſaid, that he agreed with me, that the method of tangents of Sluſius was not yet made perfect; and then ſet down a Differential Method of Tangents, publiſhed by Dr. Barrow in the year 1670, and diſguiſed it by a new notation, pretending that it was his own, and ſhewed how it might be improved, ſo as to perform thoſe things which I had aſcribed to my method; and concluded from thence, that mine differed not much from his, eſpecially ſince it facilitated quadratures. And in the *Acta Eruditorum* for Octob. 1684, in publiſhing the Elements of his Method, he added, that it extended to the difficulter problems, which without this method, or another like it, could not be managed ſo eaſily. He underſtood therefore in thoſe days, that in the year 1676, when I wrote my ſaid letter, I had a method which did the ſame things with the method which he calls Differential, and he ought ſtill to acknowledge it; eſpecially now the ſentences in cyphers are decyphered, and other things in that letter, relating to the method, are fully explained, and the Compendium mentioned therein made publick.

In his letter of the 27th of Auguſt 1676, he repreſented, that he did not believe that my methods were ſo general as I deſcribed them in my letter of the 13th of June preceding; and affirmed, that there were many problems ſo difficult, that they did not depend upon equations or quadratures, ſuch as (amongſt many others) were the inverſe problems of tangents. And by theſe words he acknowledged, that he had not yet found the reduction of problems to Differential Equations. And what he then

acknowledged, he acknowledged again in the *Acta Eruditorum* for April 1691, pag. 178, and ought in candour to acknowledge ſtill.

Doctor Wallis, in the preface to the two firſt volumes of his works, publiſhed in April 1695, wrote, that I, in my two letters written in the year 1676, had explained to Mr. Leibnitz the method (called by me, the Method of Fluxions, and by him, the Differential Method) invented by me ten years before, or above (that is, in the year 1666, or before): and in the letters which followed between them, Mr. Leibnitz had notice of this paragraph, and did not then contradict it, nor found any fault with it; and I expect that he ſtill forbear to contradict it.

See Wallis's Vol. III. pag. 654, lin. 12.

But as he has lately attacked me with an accuſation which amounts to plagiary; if he goes on to accuſe me, it lies upon him by the laws of all nations to prove his accuſations, on pain of being accounted guilty of calumny. He hath hitherto written letters to his correſpondents full of affirmations, complaints and reflections, without proving any thing. But he is the aggreſſor, and it lies upon him to prove the charge.

I forbear to deſcend further into particulars; you have them in the *Commercium Epiſtolicum*, and the abſtract thereof, to both which I refer you. I am, Sir, yours, &c.

'MONSIEUR,

'C'EST ſans doute pour l'amour de la verité, que vous vous etes chargé 'd'une eſpace de cartel de la part de M. Newton. Je n'ay point voulu 'entrer en lice avec des enfans perdus, qu'il avoit detachés contre moy; 'ſoit qu'on entende celuy, qui à fait l'accuſateur ſur le fondement du *Com*'*mercium Epiſtolicum*; ſoit qu'on regarde la preface pleine d'aigreur, 'qu'un autre à miſe devant la nouvelle edition de ſes principes. Mais 'puiqu'il veut bien paroitre luy même, je ſeray bien aiſe de luy donner 'ſatisfaction.

'Je fus ſurpris au commencement de cette diſpute d'apprendre qu'on 'm'accuſoit d'etre l'aggreſſeur; car je ne me ſouvenois pas d'avoir parlé de 'M. N. que d'une maniere fort obligeante. Mais je vis depuis, qu'on abu'ſoit pour cela d'un paſſage des Actes de Leipzig du Janvier 1705, ou il y 'à ces mots: *Pro differentiis L...ſianis, D. N....nus adhibet, ſemperque ad*'*hibuit fluxiones*; ou l'auteur des remarques ſur le Commercium Epiſtoli'cum dit, pag. 577, *Senſus verborum eſt, quòd N....nus fluxiones differentiis* '*L.....tianis ſubſtituit.* Mais c'eſt une interpretation maligne d'un homme 'qui cherchoit noiſe: il ſemble que l'auteur de paroles inſeré dans les Ac'tes de Leipzig à voulu y obvier tout expres, par ces mots, *adhibet ſem*'*perque adhibuit*; pour inſinuer, que ce n'eſt pas apres la veue de mes dif'ferences, mais deja auparavant, qu'il s'eſt ſervi de fluxions. Et je defie, 'qui que ce ſoit, de donner un autre but raiſonnable à ces paroles, *ſemper*'*que adhibuit*. Au lieu qu'on ſe ſert du mot *ſubſtituit*, en parlant de ce 'que le Pere Fabri avoit fait apres Cavallieri. D'ou il faut conclure on

'que

‘ que M. N. s'eſt laiſſé tromper, par un homme qui a empoiſonné ces paro-
‘ les des Actes, qu'on ſuppoſoit n'avoir pas eté publiées ſans ma connoiſ-
‘ ſance, & s'eſt imaginé qu'on l'accuſoit d'etré plagiaire; ou bien qu'il a
‘ eté bien aiſe de trouver un pretexte, de s'attribuer ou faire attribuer privé-
‘ ment l'invention du nouveau calcul (depuis qu'il en remarquoit le ſuc-
‘ cés, & le bruit qu'il faiſoit dans le monde) contre ſes connoiſſances con-
‘ traires avouées dans ſon livre des Principes, pag. 253, de la premiere
‘ edition. Si l'on avoit fait connoitre qu'on trouvoit quelque difficulté, ou
‘ ſujet de plainte, dans les paroles des Actes de Leipzig; je ſuis aſſuré, que
‘ ces meſſieurs, qui ont part à ces Actes, auroient donné un plein contente-
‘ ment; mais il ſemble qu'on cherchoit un pretexte de rupture.

‘ Je n'ay pas eu connoiſſance du *numerous committee of gentlemen of ſeve-*
‘ *ral nations relating to the diſpute*; car on ne m'en a donné aucune part,
‘ & je ne ſay pas encore preſentement les noms de tous ces commiſſaires, &
‘ particulierement de ceux qui ne ſont pas des iſles Britanniques: je ne crois
‘ pas qu'ils approuvent tout ce qui a eté mis dans l'ouvrage publié con-
‘ tre moi.

‘ Il eſt aiſé à croire que j'ay eté quelque temps à Vienne, avant que
‘ d'avoir vû le *Commercium Epiſtolicum* deja publié, quoique j'en euſſe des
‘ nouvelles. Ainſi un ami ſachant cela, auſſi zelé pour moi que les ſe-
‘ conds de M. N. le peuvent être pour luy, a publié une papier, que M.
‘ N. appelle diffamatoire *(defamatory letter.)* Mais cette piece n'etant pas
‘ plus forte, que ce qu'on a publié contre moi, M. N. n'a pas droit de s'en
‘ plaindre. Si l'on n'a pas marqué l'auteur ni le lieu de l'impreſſion du
‘ papier; on connoit aſſez le nom & le lieu de l'auteur de la lettre y in-
‘ ſerée d'un excellent mathematicien, que j'avois prié de dire ſon ſentiment
‘ ſur le *Commercium*, & cela ſuffit. M. N. (dont les partiſans ont marqué
‘ qu'il ne leur etoit pas inconnu) l'appelle un mathematicien, ou pretendu
‘ mathematicien; & apres avoir fait inutilement des efforts pour le gagner,
‘ il le mépriſe contre l'opinion publique, qui le met entre ceux du premier
‘ rang, & contre l'evidence des choſes verifiées par ſes découvertes

‘ Lors que j'eus enfin le *Commercium Epiſtolicum*, je vîs qu'on s'y écar-
‘ toit entierement du but; & que les lettres, qu'on publioit, ne contenoient
‘ pas un mot, qui peut faire revoquer en doute mon invention du calcul des
‘ differences, dont il s'agiſſoit. Au lieu de cela je remarquay qu'on ſe jet-
‘ toit ſur les ſeries; ou l'on accorde l'avantage à M. N. & que les rematques
‘ contenoient de gloſes mal tournées, pour tacher de me decrier par des
‘ ſoupçons ſans fondement quelquefois ridicules, & quelquefois forgés,
‘ contre la conſcience de quelques uns de ceux qui en êtoient les auteurs, ou
‘ approbateurs.

‘ Pour repondre donc de point en point a l'ouvrage publié contre moi, il
‘ falloit un autre ouvrage auſſi grand pour le moins que celuy là; il falloit
‘ entrer dans un grand detail de quantitaté de minuties paſſées il y a 30 ou
‘ 40 ans dont je ne me ſouvenois gueres; il me falloit chercher mes vieilles
‘ lettres, dont pluſieurs ſe ſont perdues, outre que le plus ſouvent, je n'ay
‘ pas gardé les minutes des miennes; & les autres ſont enſevelies dans un

‘ grand tas de papiers, qui je ne pouvois debrouiller qu'avec du temps &
‘ de la patience. Mais je n'en avois gueres le loiſir, étant chargé preſente-
‘ ment d'occupations d'une toute autre nature.

‘ De plus je remarquay, que dans la publication du *Commercium Epiſtoli-*
‘ *cum* on a ſupprimé des endroits qui pouvoient être au deſadvantage de
‘ M. N; au lieu qu'on n'y a rien omis, de ce qu'on croyoit pouvoir tourner
‘ contre moi par des gloſes forcées. Comme je n'ay pas daigné lire le
‘ *Commercium Epiſtolicum* avec beaucoup d'attention, je me ſuis trompé
‘ dans l'exemple que j'ay cité, n'ayant pas pris garde, ou ayant oublié
‘ qu'il s'y trouvoit; mais j'en citéray un autre. M. N. avouoit dans un
‘ des ſes lettres à M. Collins, qu'il ne pouvoit point venir à bout des ſec-
‘ tions ſecondes (ou ſegments ſeconds) de ſpheroides, ou corps ſemblables:
‘ mais on n'a point inſeré ce paſſage, ou cette lettre, dans le *Commercium*
‘ *Epiſtolicum*; il auroit été plus ſincere par rapport à la diſpute, & plus
‘ utile au public, de donner le commerce litteraire de M. Collins tout en-
‘ tier, là ou il contenoit quelque choſe qui meritoit d'être lû; & particu-
‘ lierement de ne pas tronquer les lettres; car il y en a peu parmi mes pa-
‘ piers, ou dont il me reſte des minutes.

‘ Ainſi tout conſideré, voyant tant de marques de malignité & de chi-
‘ cane, je crûs indigne de moi d'entrer en diſcuſſion avec des gens, qui en
‘ uſoient ſi mal. Je voyois qu'en les refutant on auroit de la peine à éviter
‘ des reproches, & des expreſſions fortes, telles que meritoit leur procedé;
‘ & je n'avois point envie de donner ce ſpectacle au public, ayant deſſein
‘ de mieux employer mon temps, qui me doit être precieux, & mepriſant
‘ aſſez le jugement de ceux, qui ſur un tel ouvrage voudroient prononcer
‘ contre moi, d'autant que la Societé Royale même ne la point voulu faire;
‘ comme je l'ay appris par un extrait de ſes regiſtres.

‘ Je ne crois point d'avoir dit (comme M. N. me l'impute) que les An-
‘ glois n'auroient point le plaiſir de me voir repondre à des raiſonnemens
‘ ſi minces; car je ne crois point que tous les Anglois faſſent leur cauſe de
‘ celle de M. N. il y en a de trop habiles & de trop honnêtes pour épouſer
‘ les paſſions de quelquesuns de ſes adherens.

‘ Apres cela, il m'accuſe d'avoir voulu faire diverſion, en combattant ſa
‘ philoſophie, & en voulant l'engager dans des problemes; mais quant à
‘ la philoſophie, j'ay donné publiquement quelque choſe de mes principes
‘ ſans attaquer les ſiens; ſi ce n'eſt que par occaſion j'en ay parlé dans des
‘ lettres particulieres, depuis qu'on m'en a donné ſujet; & pour ce qui eſt
‘ des problemes, je n'ay garde d'en propoſer à M. N. car je ne voudrois pas
‘ m'y engager, quand on m'en propoſeroit à moi: nous pouvons nous en
‘ diſpencer à l'age ou nous ſommes; mais nous avons des amis, qui y peu-
‘ vent ſuppleer à notre defaut.

‘ Je ne veux point entrer icy dans le detail de ce que M. N. dit un peu
‘ aigrement contre ma philoſophie; car pour la ſienne, ce n'en eſt point le
‘ lieu. J'apelle Miracle tous evenement qui ne peut être arrivé que par la
‘ puiſſance de Createur, ſa raiſon n'étant pas dans la nature des creatures;
‘ & quand on veut neanmoins l'attribuer aux qualités ou forces des creatures,

‘ alors

‘ alors j'appelle cette qualité *une qualité occulte à la ſcholaſtique*; c'eſt à dire,
‘ qu'il eſt impoſſibile de rendre manifeſte : telle que ſeroit une peſanteur
‘ primitive ; car les qualités occultes qui ne ſont point chimeriques, ſont
‘ celles dont nous ignorons la cauſe, mais que nous n'excluons point ; &
‘ j'appelle l'ame de l'homme cette ſubſtance ſimple, qui s'appercoit de ſe
‘ qui ſe paſſe dans le corps humain, & dont les appetits ou volontés ſont
‘ ſuivis par les efforts du corps. Je ne prefére pas les hypotheſes aux ar-
‘ guments tirez de l'induction des experiences, mais quelquefois on fait
‘ paſſer pour inductions generales, ce qui ne conſiſte qu'en obſervations par-
‘ ticulieres, & quelquefois on veut faire paſſer pour une hypotheſe ce qui
‘ eſt demonſtratif. L'idée que M. N. donne icy de mon harmonie préeta-
‘ blie n'eſt pas celle qu'en ont quantité d'habiles gens hors d'Angleterre,
‘ & quelques uns en Angleterre ; & je ne crois pas que vous même Monſ.
‘ en ayez eu une ſemblable, ou l'ayéz maintenant, à moins que d'être bien
‘ changé.

‘ Je n'ay jamais nié, qu'à mon ſecond voyage en Angleterre j'aye vû
‘ quelques lettres de M. N. chez Monſieur Collins ; mais je n'en ay jamais
‘ vû, ou M. N. ait expliqué ſa methode des fluxions, & je n'en trouve point
‘ dans le *Commercium Epiſtolicum*.

‘ Je n'ay pas vû non plus qu'il ait expliqué la methode des ſeries que je
‘ m'attribue ; je crois qu'il veut parler de celle ou je prends une ſeries ar-
‘ bitraire ; je l'ay fait avant mon ſecond retour en Angleterre. Je ne nie
‘ pourtant pas, que M. N. n'eut pû l'avoir auſſi, & ce n'eſt pas même une
‘ invention fort difficile.

‘ M. N. veut que j'avoue, & que j'accorde ce que j'ay avoué ou accordé
‘ il y a 15 ans. Ou autrement on devroit en attendre de luy autant ; car il
‘ y a maintenant deux fois quinze ans, que dans la premiere edition de ſes
‘ Principes, pag. 253, 254, il m'accorde l'invention du calcul des diffe-
‘ rences, independement de la ſienne ; & depuis il s'eſt avisé, je ne ſcay
‘ comment, de faire ſoutenir le contraire.

‘ Il eſt bon de ſavoir, qu'à mon premier voyage d'Angleterre en 1673, je
‘ n'avois pas la moindre connoiſſance des ſeries infinies, telles que M. Mer-
‘ cator venoit de donner, ny d'autres matieres de la geometrie avancée par
‘ les dernieres methodes : je n'etois pas même aſſez versé dans l'analyſe de
‘ Des Cartes ; je ne traitois les mathematiques que comme un Parergon ;
‘ & je ne ſavois guere que la geometrie practique vulgaire, quoy que j'euſſe
‘ vû par hazard la geometrie des indiviſibiles de Cavalleri, & un livre de
‘ Pere Leotaud, ou il donnoit les quadratures des lunules & figures ſem-
‘ blables, ce qui m'avois donné quelque curioſité ; mais je me divertiſſois
‘ pluſtôt aux proprietez des nombres, à quoy le petit traité que j'avois pub-
‘ lié, preſque petit garcon, de l'art des combinaiſons en 1666 m'avoit donné
‘ occaſion ; & ayant obſervé des lors l'uſage des differences pour les ſom-
‘ mes, je l'appliquay à des ſuites de nombres. On voit bien par mes pre-
‘ mieres lettres échangées avec M. Oldenbourg, que je n'etois guere allé
‘ plus avant, auſſi n'avois je point alors la connoiſſance de M. Collins, quoy
‘ qu'on ait feint malicieuſement le contraire.

‘ Ce fût peu à peu que M. Hugens me fit entrer en ces matieres, quand ‘ je le pratiquois à Paris, & cela joint au traité de M. Mercator (que j'avois ‘ rapporté avec moi d'Angleterre parce que M. Pell m'en avoit parlé) me ‘ fit trouver environ, vers la fin de l'an 1673, ma quadrature arithmetique ‘ du cercle, qui fût fort approuvé par M. Hugens, & dont je parlay à M. ‘ Oldenbourg dans une lettre de l'an 1674 : alors ny M. Hugens ny moi, ‘ nous ne ſavions rien des ſeries de M. N. ny de M. Gregory. Ainſi je ‘ crus être le premier, qui eut donné la valeur du cercle par une ſuite de ‘ nombres rationaux, & M. Hugens le crut auſſi ; j'en écrivîs ſur ce ton-là ‘ à M. Oldenbourg, qui me repondit qu'on avoit deja de telles ſeries en An- ‘ gleterre ; & l'on voit par ma lettre du 15 Juillet de 1674, & par la re- ‘ ponſe de M. Oldenbourg du 8 Decembre de la même année, que je n'en ‘ devois avoir aucune connoiſſance alors ; autrement M. Oldenbourg n'au- ‘ roit pas manqué de me le faire ſentir, ſi luy ou M. Collins m'en euſſent ‘ communiqué quelque choſe ; mais je ne ſavois pas alors les extractions ‘ des racines des equations par des ſeries, ni les regreſſions ou l'extrac- ‘ tion d'une equation infinie ; j'etois encore un peu neuf en ces matieres ; ‘ mais je trouvai pourtant bientôt ma methode generale par des ſeries ar- ‘ bitraires ; & j'entrai enfin dans mon calcul des differences, ou les ob- ‘ ſervations que j'avois faites, encore fort jeune, ſur les differences des ſuites ‘ des nombres, contribuerent à m'ouvrir les yeux ; car ce n'eſt pas par les ‘ fluxions des lignes, mais par les differences des nombres qve j'y ſuis venu, ‘ en conſiderant enfin que ces differences appliquées aux grandeurs qui cro- ‘ iſſent continuellement, s'evanouiſſent en comparaiſon des grandeurs dif- ‘ ferentes, au lieu qu'elles ſubſiſtent dans les ſuites des nombres. Et je ‘ crois que cette voye eſt la plus analytique ; le calcul geometrique des dif- ‘ ferences qui eſt le même que celuy des fluxions, n'etant qu'un cas ſpe- ‘ cial, devient plus commode par les evanouiſſements.

‘ M. N. allegue par apres les paſſages, ou j'accorde, qu'il y a un calcul ‘ approchant de mon calcul des differences ; mais il pourra bien ſe ſouvenir ‘ qu'il m'en a accordé autant, et s'il luy eſt permis de ſe retracter, pour- ‘ quoy ne me ſera t'il pas permis d'en faire autant, ſur tout apres les ve- ‘ riſimilitudes que M. Bernouilli a remarquées ? J'ay une ſi grande opinion ‘ de la candeur de M. N. que je l'ay crû ſur ſa parole ; mais le voyant con- ‘ niver à des accuſations dont la fauſſeté luy eſt connue, il etoit naturel ‘ que je commençaſſe de douter.

‘ Je ne puis avouer ny deſavouer aujourdhuy d'avoir écrit, ou receu des ‘ lettres écrites, il y a plus de 40 ans telles qu'on les a publiées ; je ſuis ob- ‘ ligé de m'en rapporter à ce qui ſe trouve dans les papiers qu'on cite, mais ‘ je ne remarque rien contre moi dans celles que M. N. allegue du 15 ‘ Avril & 20 May 1675, & du 24 Octobre 1676, ſinon dans les fauſſetez ‘ du Gloſateur. Je crois que c'etoit purement par diſtraction, dans un ſejour ‘ comme celuy de Paris, ou je m'occupois à bien d'autres choſes encore ‘ qu'aux mathematiques, & par l'éloignement que j'avois des calculs, dont ‘ je craignois la longueur, que j'ay demandé quelquefois à M. Oldenbourg ‘ la demonſtration ou la methode d'arriver à certaines choſes, ou j'aurois

‘ bien

‘ bien pû arriver moi même. Par exemple, je crois d'avoir deja eu au
‘ douze de May 1676, ma methode d'une ſeries arbitraire, qui m'auroit
‘ pû mener a des ſeries, dont j'y demande la raiſon. Car ayant conſulté
‘ mon vieux traité de la Quadrature Arithmetique, achevé quelque temps
‘ avant ma ſortie de France, je m'y ſers de la ſeries arbitraire ; cependant les
‘ ſeries marquées dans cette lettre ſont une choſe dont je conſens dêtre re-
‘ devable à d'autres, & je crois de ne les avoir pas même connues en 1674.

‘ N'entendant pas bien ce que M. N. allegue des Actes de Leipzig de
‘ May 1700, j'y ay regardé, & je trouve qu'il n'en a pas bien pris le ſens.
‘ Il n'y eſt point parlé de l'invention du nouveau calcul des differences,
‘ mais d'un artifice particulier des *Maximis* & *Minimis*, qui en eſt indepen-
‘ dant, & dont je m'etois avisé bien du temps avant que M. Bernouilli eut
‘ proposé ſon probleme de la plus courte deſcente, mais dont je jugeois
‘ que M. N. ſe devoit être avisé auſſi, lors qu'il avoit donné la figure de
‘ ſon vaiſſeau dans ſes principes. Ainſi j'ay voulu dire, qu'il a fait con-
‘ noître publiquement avant moi, qu'il poſſedoit cet artifice ; ce que je ne
‘ pouvois pas dire du calcul des differences & des fluxions, puiſque j'en
‘ avois fait voir l'utilité publiquement avant la publication de ce livre. Cet
‘ artifice particulier de *Maximis & Minimis* n'eſt point neceſſaire, quand ils
‘ s'agit ſimplement d'une grandeur (car alors la methode de M. Fermat per-
‘ fectionée par les nouveaux calculs ſuffit) mais quand il s'agit de toute une
‘ figure qui doit faire le mieux un effect demandé, il faut autre choſe.

‘ M. N. hazarde icy une accuſation mais, qui va tomber ſur luy même.
‘ Il pretend que ce que j'ay écrit pour luy à M. Oldenbourg en 1677, eſt
‘ un deguiſement de la methode de M Barrow. Mais comme M. N. avoue
‘ dans la pag. 253 & 254, de la premier edition de ſes Principes, *Me ipſi*
‘ *tunc methodum communicâſſe à methodo ipſius vix abludentem præterquam in*
‘ *verborum & notarum formulis*, il s'enſuivra, que ſa methode auſſi n'eſt qu'un
‘ déguiſement de celle de M. Barrow.

‘ Je croy que luy & moi nous ſerons aiſement quites de cette accuſa-
‘ tion : car une infinité de gens liront de livre de M. Barrow, ſans y trou-
‘ ver nôtre calcul ; il eſt vray que feu M. Tſchirnhaus, qui s'apperçeut un
‘ peu tard de l'avantage de ce calcul, pretendoit qu'on pouvoit arriver à
‘ tout cela par les methodes de M. Barrow. Comme l'Abbé Catelan Fran-
‘ cois pretendit que même l'Analyſe de Des Cartes ſuffiſoit pour toute ces
‘ choſes ; mais il etoit plus aisé de le dire, que de le montrer.

‘ Cependant ſi quelqu'un a profité de M. Barrow, ce ſera plûs tôt M.
‘ N. qui a étudié ſous luy, que moi qui (autant que je puis m'en ſouvenir,)
‘ n'ay veu les livres de M. Barrow qu'à mon ſecond voyage d'Angleterre,
‘ & ne les ay jamais lûs avec attention ; parce qu'en voyant le livre, je
‘ m'appercus que par la conſideration du triangle caracteriſtique (dont les
‘ cotez ſon les elements de l'abſciſſe, de l'ordonnée & de la courbe) ſem-
‘ blable a quelque triangle aſſignable, j'etois venu, comme en me jouant,
‘ aux quadratures, ſurfaces, & ſolides, dont M. Barrow avoit remplis un cha-
‘ pitre des plus conſiderables de ſes leçons ; outre que je ne ſuis venu à mon
‘ calcul des differences dans la geometrie, qu'apres en avoir vû l'uſage (mais

‘ moins

‘ mons conſiderable) dans les nombres, comme mes premieres lettres ‘ dans le *Commercium Epiſtolicum* le peuvent inſineur. Il ſe peut que M. ‘ Barrow en ait plus ſcu, qu'il n'a pas dit dans ſon livre, & qu'il a donné ‘ des lumieres à M. N. que nous ne ſavons pas : & ſi j'etois ſemblable à ‘ certains temeraires, je pourrois aſſeurer ſur de ſimples ſoupçons, ſans autre ‘ fondement, que le calcul des fluxions de M. N. qu'el qu'il puiſſe être, ‘ luy a eté enſeigné par M. Barrow.

‘ On peut bien juger que lors que j'ay parlé en 1676, des problemes qui ‘ ne dependoient, ni des equations, ni des quadratures, j'ay voulu parler ‘ des equations telles qu'on connoiſſoit alors dans le monde ; c'eſt à dire, ‘ des equations de l'analyſe ordinaire. Et on le peut juger, de ce que ‘ j'ajoute les quadratures comme quelque choſe de plus que ces equations. ‘ Mais les equations differentielles vont au dela même des quadratures ; & ‘ l'on voit bien que j'entendois même parler des problemes, qui vont a ces ‘ ſortes d'equations inconnues alors au public ; cette objection ſe trouvoit ‘ deja dans les remarques an *Commercium*, mais je n'avois point crû que M. ‘ N. etoit capable de l'employer.

‘ Je juge par un endroit de ma lettre du 27 d'Aouſt 1676 *(vid. pag.* ‘ 539, *hujus Tomi)* que je devois deja avoir alors l'ouverture du cal- ‘ cul des differences ; car j'ay dit d'avoir reſolu d'abord par une certaine ‘ analyſe *(certâ Analyſi ſolvi)* le probleme de M. de Beaune proposé à M. ‘ Des Cartes ; ſi cette analyſe n'etoit que cela, on le peut reſoudre ſans ‘ cela ; & je crois que Monſieur Hugens & Monſieur Barrow l'auroient ‘ donné au beſoin, comme beaucoup d'autres choſes : mais ſelon ma ma- ‘ niere de noter, ce n'eſt qu'un jeu ; je trouve une petite faute dans cette ‘ page, il y a *ludus naturæ* au lieu de *luſus naturæ*, mais cette faute etoit ‘ ancienne, & ſe devoit deja trouver dans la copie de ma lettre du 24 d'Oc- ‘ tobre *(1676, pag.* 539, *hujus Tomi.) Hos caſus vix numeraverim in-* ‘ *ter luſus naturæ.* Je n'avois point entendu ce qu'il vouloit dire, mais à ‘ preſent je vois l'origine de la mépriſe.

‘ Je ne ſaurois dire aujourdhuy, ſi j'ay remarqué le paſſage de M. Wallis, ‘ ou il dit que M. N. ſervoit deja la methode des fluxions en 1666. Mais ‘ quand je l'aurois remarqué, je l'aurois laiſſé paſſer apparemment, etant ‘ fort porté alors à croire M. N. ſur ſa parole. Mais ſon dernier procedé ‘ m'a forcé d'être plus circomſpect à cet egard.

‘ M. N. dit, que je l'ay accusé d'être plagiaire : mais ou eſt ce que je l'ay ‘ fait ? Ce ſont ſes adherens, qui ont paru intenter cette accuſation contre ‘ moi, & il y a connivé. Je ne ſay pas s'il adopte entierement ce qu'ils ‘ ont publié ; mais je conviens avec luy, que la malice de celuy qui intente ‘ une telle accuſation ſans la prouver, le rend coupable de calomnie.

‘ Il finit ſa lettre en m'accuſant d'être l'aggreſſeur, & j'ay commencé ‘ celle cy en prouvant le contraire. Il ſera fort aiſé de vuider ce point pre- ‘ liminaire. Il y a eu du meſentendu, mais ce n'eſt pas ma faute ; au ‘ reſte je ſuis avec zele. Monſieur,

‘ Vôtre tres humble & tres obeiſſant ſerviteur,

Hannover,
ce 9 d'Avril 1716.

‘ LEIBNITZ.’

Cùm D. Leibnitius adduci non poſſet, ut vel Commercio Epiſtolico reſponderet, vel probaret quæ pro lubitu affirmabat, cúmque præcedentes epiſtolas in Galliam prius mitteret, quàm earum tertia in Angliam veniret, & prætenderet ſe hoc facere, ut teſtes haberet, & alias etiam adhiberet contumelias: Newtonus minimè reſcripſit, ſed obſervationes ſequentes in epiſtolam illam tertiam ſcriptas, cum amicis ſolummodo communicavit.

Obſervations upon the preceding EPISTLE.

MR. LEIBNITZ, by his letter of the 29th of December 1711, juſtified the paſſage in the *Acta Eruditorum* for January 1705, pag. 34 and 35, and thereby made it his own, and now endeavours in vain to excuſe it, pretending that the words *adhibet ſemperque adhibuit* are maliciouſly interpreted by the word *ſubſtituit*. But in the interpretation which he would put upon the place, he omits the words *igitur* and *quemadmodum*; the firſt of which makes the words, *ſemperque adhibuit*, a conſequence of what went before, and the latter makes them equivalent to *ſubſtituit*; neither of which can be true, in the ſenſe which Mr. Leibnitz endeavours now to put upon the words. He has therefore accuſed me. In both his letters to Dr. Sloane (that dated the 4th of March, and that dated the 29th of December 1711) he preſſed the Royal Society to condemn Dr. Keill; and before I meddled in this matter, challenged me to declare my opinion. His words in his ſecond letter are: *Itaque veſtræ æquitati committo, annon coercendæ ſint vanæ & injuſtæ* [Keill] *vociferationes, quas ipſi Newtono viro inſigni, & geſtorum optimè conſcio, improbari arbitror; ejuſque ſententiæ ſuæ libenter daturum indicia mihi perſuadeo.* The words are civil, but the ſenſe is, That I muſt either condemn Dr. Keill, or enter into a quarrel with Mr. Leibnitz; as has happened: and therefore he is the aggreſſor. For it is very well known here, that I conſtantly endeavoured to avoid theſe diſputes, till they were preſſed upon the Royal Society and me.

In his letter of the 4th of March ſt. n. 1711, he preſſed the Royal Society to condemn Dr. Keill, without hearing both parties; and when the Doctor put in his anſwer, Mr. Leibnitz refuſed to give his reaſons againſt the Doctor, and called it injuſtice to expect it from him, and yet perſiſted in preſſing them againſt him, and thereby put them upon a neceſſity of appointing a committee to ſearch out old papers, and give their opinion upon them. If they did it without him, it was his own fault: he was for over-ruling them, and called it injuſtice to expect that he ſhould defend his candor, and plead before them. If they gave him no opportunity to except againſt any of the committee; it was becauſe he refuſed to be heard: and they had a ſufficient authority to appoint a committee without him, and he had no right to except againſt what they did for their own ſatisfaction. If they have not yet given judgment againſt him; it is becauſe the committee did not act as a jury, nor the Royal Society as a formal court of juſtice. The committee examined old letters and papers, and gave their

their opinion upon them alone, and left room for Mr. Leibnitz to produce further evidence for himſelf. And it is ſufficient that the Society ordered their report, with the papers upon which it was grounded, to be publiſhed; and that Mr. Leibnitz, in all the three years and four months which are ſince elapſed, has not been able to produce any further proof againſt Dr. Keill, than what was then before them.

Mr. Leibnitz ſaith, That the letter which I call defamatory, being no ſharper than that which has been publiſhed againſt him, I have no reaſon to complain. But the ſharpneſs of the letter lies in accuſations and reflections, without any proof; which way of writing is unlawful and infamous, and never uſed but in a bad cauſe. The ſharpneſs of the *Commercium* lies in facts, which are lawful and fit to be produced. The letter was publiſhed in a clandeſtine, back-biting manner (as defamatory papers uſe to be) without the name of the author, or mathematician, or printer, or city where it was printed; and was diſperſed above two years, before we were told that the mathematician was John Bernouilli; the *Commercium* was printed openly at London by order of the Royal Society.

The mathematician to whom Mr. Leibnitz appealed from the Royal Society, I called a mathematician, or pretended mathematician, not to diſparage the ſkill of Mr. Bernouilli; but becauſe the mathematician, in his letter of the 7th of June 1673, cited Mr. Bernouilli as a perſon diſtinct from himſelf; and Mr. Leibnitz lately cauſed that letter to be reprinted without the citation; and tells us, that the mathematician was Mr. Bernouilli himſelf: and whether the mathematician or Mr. Leibnitz is to be believed, I do not know. Mr. Bernouilli had the Differential Method from Mr. Leibnitz, and was the chief of his diſciples, and gave his opinion for his maſter in the *Acta Leipſica* before he ſaw the *Commercium Epiſtolicum*; at which time he was *homo novus, & rerum anteactarum parum peritus*, as Mr. Leibnitz objected againſt Dr. Keill; and what he wrote after he ſaw the *Commercium* was in his own defence, and his ſkill in mathematicks will not mend the matter.

He complains, that the committee had gone out of the way, in falling upon the Method of Series. But he ſhould conſider, that both methods are but two branches of one general method of Series: I joined them together in my Analyſis; I interwove them in the tract which I wrote in the year 1671, as I ſaid in my letters of the 10th of December 1672, and the 24th of October 1676. In my letter of the 13th of June 1676, I ſaid, that my method of Series extended to almoſt all problems, but became not general without ſome other methods, meaning (as I ſaid in my next letter) the method of Fluxions, and the method of arbitrary Series: and now to take thoſe other methods from me, is to reſtrain and ſtint the method of Series, and make it ceaſe to be general. In my letter of the 24th of October 1676, I called all theſe methods together, my General Method. See pag. 547, lin. 20. And if Mr. Leibnitz has been tearing this general method in pieces, and taking from me firſt one part, and then another part, whereby the reſt is maimed, he has

has given a juſt occaſion to the committee to conſider the whole. It is alſo to be obſerved, that he is perpetually giving teſtimony for himſelf; and it is allowed, in all courts of juſtice, to ſpeak to the credit of the witneſs.

He repreſents, that the committee of the Royal Society have omitted things which made againſt me, and printed every thing which could be turned againſt him by ſtrained gloſſes; and to make this appear, he produces in his laſt letter but one, an inſtance of my ignorance omitted by them; but confeſſes now, that he was miſtaken in ſaying that it was omitted, and ſaith that he will cite another inſtance. He ſaith, that in one of my letters to Mr. Collins, I owned that I could not find the ſecond Segments of Sphæroids, and that the committee have omitted this. If they had omitted ſuch a paſſage, I think they would have done right, it being nothing to the purpoſe. But on the contrary Mr. Collins, in a letter to Mr. James Gregory the 24th of Decem. 1670, and in another to Mr. Bertet the 21ſt of Feb. 1671, both printed in the *Commercium Epiſtolicum*, pag. 505, 507, wrote, that my method extended to ſecond Segments of round Solids. And Mr. Oldenbourg wrote the ſame thing to Mr. Leibnitz himſelf the 8th of Decemb. 1674. See the *Commercium Epiſtolicum*, pag. 517. So you ſee that Mr. Leibnitz hath accuſed the committee of the Royal Society, without knowing the truth of his accuſation, and therefore is guilty of a miſdemeanour. The committee were ſo far from acting corruptly againſt Mr. Leibnitz, that they took no notice of his ignorance of geometry in thoſe days, and omitted ſeveral other things which made ſtrongly againſt him; ſuch as were the two letters in my cuſtody, and the paragraph in the preface to the two firſt volumes of Dr. Wallis's works relating to this matter; and that a copy of Gregory's letter of the 5th of Septemb. 1670, was ſent to Mr. Leibnitz in June 1676, amongſt the extracts of Gregory's letters.

The committee in their report affirmed, that they had extracted from the ancient letters, letter-books and papers, what related to the matter referred to them: all which extracts, delivered by them to the Society, they believed to be genuine and authentick. Mr. Leibnitz accuſes them for not printing the letters entire, including as well what did not relate to the matter referred to them, as what did relate to it: as if it were not lawful to cite a paragraph out of a book, without citing the whole book. Thus he complains, that the *Commercium Epiſtolicum* ſhould have been much bigger. But when he is to anſwer it, he complains that it is too big, and would require an anſwer as big as itſelf. And ſo the ancient letters and papers muſt be laid aſide, and the queſtion muſt be run off into a ſquabble about philoſophy and other matters: and the great mathematician who, in his letter to Mr. Leibnitz, dated the 7th of June 1713, concealed his name, that he might paſs for an impartial judge; muſt now pull off his maſk, and become a party-man in this ſquabble, and ſend a challenge by Mr. Leibnitz to the mathematicians in England: as if a duel, or perhaps a battle with his army of diſciples, were a fitter way to decide the truth, than an appeal to ancient and authentick writings; and mathematicks

muſt henceforward be filled with atchievements in knight-errantry, inſtead of reaſons and demonſtrations.

Mr. Leibnitz acknowledges, that when he was in London the ſecond time, he ſaw ſome of my letters in the hands of Mr. Collins, eſpecially thoſe relating to Series; and he has named two of them which he then ſaw, viz. that dated the 24th of October 1676, and that in which he pretends that I confeſſed my ignorance of ſecond Segments. And no doubt he would principally deſire to ſee the letter which contained the chief of my Series, and particularly that which contained thoſe two for finding the arc by the ſine, and the ſine by the arc, with the demonſtration thereof, which a few months before he had deſired Mr. Oldenbourg to procure from Mr. Collins; that is, the *Analyſis per æquationes numero terminorum infinitas*. But yet he tells us, that he never ſaw where I explained my method of Fluxions, and that he finds nothing of it in the *Commercium Epiſtolicum*, where that Analyſis and my letters of the 10th of Decemb. 1672, 13th of June 1676, and the 24th of Octob. 1676, are publiſhed. I ſuppoſe he means, becauſe he finds no pricked letters there. And by the ſame way of arguing, he and Mr. Bernouilli may pretend, that they find nothing of the method of Fluxions in the introduction to the Book of Quadratures.

He ſaith alſo, that he never ſaw where I explain the method claimed by me, in which he aſſumes an arbitrary Series. If he pleaſes to look into the *Commercium Epiſtolicum*, pag. 530 and 557, he will there ſee that I had that method in the year 1676, and five years before. And Dr. Wallis, in the ſecond volume of his works, pag. 393, lin. 32, has told him, that this method needs no further explication, than what I there gave of it. Mr. Leibnitz might find it himſelf, but not ſo early; and ſecond inventors have no right.

He pretends, that in my book of Principles, pag. 253, 254, I allowed him the invention of the *Calculus Differentialis*, independently of my own; and that to attribute this invention to myſelf, is contrary to my knowledge there avowed. But in the paragraph there referred unto, I do not find one word to this purpoſe. On the contrary, I there repreſent, that I ſent notice of my method to Mr. Leibnitz, before he ſent notice of his method to me; and left him to make it appear, that he had found his method before the date of my letter; that is, eight months at the leaſt before the date of his own. And by referring to the letters, which paſſed between Mr. Leibnitz and me ten years before, I left the reader to conſult thoſe letters, and interpret the paragraph thereby. For by thoſe letters he would ſee, that I wrote a tract on that method, and the method of Series together, five years before the writing of thoſe letters; that is, in the year 1671. And theſe hints were as much as was proper in that ſhort paragraph, it being beſides the deſign of that book to enter into diſputes about theſe matters.

He ſaith, that when he was in London the firſt time, which was in January and February 1673, he knew nothing of Infinite Series, nor of the advanced geometry, nor was then acquainted with Mr. Collins, as ſome have

have maliciouſly feigned. But who hath feigned this, or what need there was of feigning it, I do not know. At that time Dr. Pell gave him notice of Mercator's Series for the Hyperbola, and he carried Mercator's book with him to Paris, though he did not yet underſtand the higher geometry. And any of thoſe to whom Mr. Collins had communicated mine and Gregory's Series, might give him notice of them, without his being acquainted with Mr. Collins.

He ſaith, that after his coming from London to Paris, his firſt letters were of other matters than geometrical, till Mr. Huygens had inſtructed him in theſe things; and that he found the arithmetical quadrature of the circle towards the end of the year 1683, and began to write of it to Mr. Oldenbourg the next year, and found the general method by arbitrary Series a little after, and the *Differential Calculus* in the year 1676, deducing it from the Series of numbers; and that in his letter of the 27th of Auguſt 1676, by the words *certâ Analyſi*, he meant the Differential Analyſis. And am not I as good a witneſs, that I invented the methods of Series and Fluxions in the year 1665, and proved them in the year 1666: and that I ſtill have in my cuſtody ſeveral mathematical papers written in the years 1664, 1665, and 1666, ſome of which happen to be dated: and that in one of them, dated the 13th of Novemb. 1665, the direct method of Fluxions is ſet down in theſe words:

PROB. *An equation being given, expreſſing the relation of two or more lines, x, y, z, &c. deſcribed in the ſame time by two or more moving bodies,* A, B, C, *&c. to find the relation of their velocities, p, q, r, &c.*

RESOLUTION. *Set all the terms on one ſide of the equation, that they become equal to nothing. Multiply each term by ſo many times* $\frac{p}{x}$ *as x hath dimenſions in that term. Secondly, Multiply each term by ſo many times* $\frac{q}{y}$ *as y hath dimenſions in it. Thirdly, Multiply each term by ſo many times* $\frac{r}{z}$ *as z hath dimenſions in it, &c. The ſum of all theſe products ſhall be equal to nothing. Which equation gives the relation of p, q, r, &c.:* and that this reſolution is there illuſtrated with examples, and demonſtrated, and applied to problems about tangents, and the curvature of curves: and that in another paper dated the 16th of May 1666, a general method of reſolving problems by motion is ſet down in ſeven propoſitions, the laſt of which is the ſame with the problem contained in the aforeſaid paper of the 13th of Novemb. 1665: and that in a ſmall tract written in Novemb. 1666, the ſame ſeven propoſitions are ſet down again, and the ſeventh is improved by ſhewing how to proceed without ſticking at fractions or ſurds, or ſuch quantities as are now called Tranſcendent: and that an eighth proproſition is here added, containing the Inverſe Method of Fluxions ſo far as I had then attained it; namely, by quadratures of curvilinear figures, and particularly by the three rules upon which the *Analyſis per Æquationes numero terminorum infinitas* is founded, and by moſt of the theo-

rems ſet down in the Scholium to the tenth propoſition of the book of Quadratures: and that in this tract, when the area ariſing from any of the terms in the valor of the ordinate cannot be expreſſed by vulgar analyſis, I repreſent it by prefixing the ſymbol □ to the term; as if the abſciſſa be x, and the ordinate $ax - b + \frac{bb}{a+x}$, the area will be $\frac{1}{2}axx - bx + \square\frac{bb}{a+x}$: and that in the ſame tract I ſometimes uſed a letter with one prick, for quantities involving firſt fluxions; and the ſame letter with two pricks, for quantities involving ſecond fluxions: and that a larger tract, which I wrote in the year 1671, and mentioned in my letter of the 24th of Oct. 1676, was founded upon this ſmaller tract, and began with the reduction of finite quantities to converging Series; and with the ſolution of theſe two problems: 1. *Relatione quantitatum fluentium inter ſe datâ, fluxionum relationem determinare.* 2. *Expoſitâ æquatione fluxiones quantitatum involvente, invenire relationem quantitatum inter ſe:* and that when I wrote this tract, I had made my Analyſis, compoſed of the methods of Series and Fluxions together, ſo univerſal, as to reach to almoſt all ſorts of problems; as I mentioned in my letter of the 13th of June 1676: and that this is the method deſcribed in my letter of the 10th of Decemb. 1672.

In the year 1684 Mr. Leibnitz publiſhed only the elements of the *Calculus Differentialis*, and applied them to queſtions about tangents, and *Maxima & Minima*, as Fermat and Gregory had done before; and ſhewed how to proceed in theſe queſtions without taking away ſurds, but proceeded not to the higher problems. The *Principia Mathematica* gave the firſt inſtances made publick of applying this *Calculus* to the higher problems; and I underſtood Mr. Leibnitz in this ſenſe in what I ſaid concerning the *Acta Eruditorum* for May 1700, pag. 206. But Mr. Leibnitz obſerves, that what was there ſaid by him, relates only to a particular artifice *de Maximis & Minimis*, with which he there allowed that I was acquainted, when I gave the figure of my veſſel in my Principles. But this artifice depending upon the Differential Method as an improvement thereof, and being the artifice by which they ſolved the problems, which they value themſelves moſt upon, thoſe of the *Linea celerrimi Deſcenſûs*, and the *Linea Catenaria* and *Velaria*, and which Mr. Leibnitz there calls a Method of the Higheſt Moment, and greateſt Extent; I content myſelf with his acknowledgment, that I was the firſt who proved, by a ſpecimen made publick, that I had this artifice.

In the year 1689 Mr. Leibnitz publiſhed the principal propoſitions of this book as his own, in three papers, called, *Epiſtola de Lineis Opticis, Schediaſma de reſiſtentiâ Medii, & Motu projectilium gravium in Medio reſiſtente, & Tentamen de Motuum Cæleſtium Cauſis*; pretending that he had found them all, before that book came abroad. And to make the principal propoſition his own, he adapted to it an erroneous demonſtration; and thereby diſcovered, that he did not yet underſtand, how to work in ſecond differences. And this was the ſecond ſpecimen made publick, of applying the

the method to the higher problems. Hitherto this method made no noiſe, but within a year or two began to be celebrated.

Dr. Barrow printed his Differential Method of Tangents in the year 1670. Mr. Gregory from this method, compared with his own, deduced a general method of tangents without calculation; and by his letter of the 5th of September 1670, gave notice thereof to Mr. Collins. Sluſius, in November 1672, gave notice of the like method to Mr. Oldenbourg. In my letter of the 10th of December 1672, I ſent the like method to Mr. Collins; and added, that I mentioned it to Dr. Barrow, when he was printing his lectures; and that I took the methods of Gregory and Sluſius to be the ſame with mine; and that it was but a branch or corollary of a general method, which without any troubleſome calculation extended not only to tangents, but alſo to other abſtruſer ſorts of problems concerning the crookedneſs, areas, lengths, centers of gravity of curves, &c. and did all this, even without freeing equations from ſurds; and I added, that I had interwoven this method with that of infinite Series; meaning, in the tract which I wrote in the year 1671. Copies of theſe two letters were ſent to Mr. Leibnitz by Mr. Oldenbourg, in the extracts of Gregory's letters, in June 1676; and Mr. Leibnitz, in his letter of the 21ſt of June 1677, ſent nothing more back, than what he had notice of by theſe two letters; namely, Dr. Barrow's Differential Method of Tangents, diſguiſed by a new notation, and extended to the Method of Tangents of Gregory and Sluſius, and to equations involving ſurds, and to quadratures. But this is not the caſe between me and Dr. Barrow. He ſaw my tract of Analyſis in the year 1699, and was pleaſed with it. And before his lectures came abroad, I had deduced the method of Tangents of Gregory and Sluſius from my general method. But Mr. Leibnitz in thoſe days knew nothing of the higher Geometry, nor was he acquainted with the vulgar Algebra.

In his letter of the 27th of Auguſt 1676, he wrote thus: *Quod dicere videmini pleraſque difficultates (exceptis problematibus Diophantæis) ad ſeries infinitas reduci; id mihi non videtur. Sunt enim multa uſque adeo mira & implexa, ut neque ab æquationibus pendeant, neque ex quadraturis. Qualia ſunt, ex multis aliis, problemata methodi tangentium inverſæ.* And when I anſwered, that ſuch problems were in my power, he replied, in his letter of the 21ſt of June 1676, that he conceived that I meant by Infinite Series, but he meant by Vulgar Equations. See the anſwer to this in the *Commercium Epiſtolicum*, pag. 562.

He ſaith, that one may judge, that when he wrote his letter of the 27th of Auguſt 1676, he had ſome entrance into the Differential Calculus; becauſe he ſaid there, that he had ſolved the problem of Beaune *certâ Analyſi*, by a certain analyſis. But what if that problem may be ſolved *certâ Analyſi* without the differential method? For no further analyſis is requiſite than this; that the ordinate of the curve deſired increaſes or decreaſes in geometrical progreſſion, when the abſciſſa increaſes in arithmetical; and therefore the abſciſſa and ordinate have the ſame relation to one another,

another, as the logarithm and its number. And to infer from this, that Mr. Leibnitz had entrance into the Differential Method; is as if one ſhould ſay, that Archimedes had entrance into it, becauſe he drew tangents to the ſpiral, ſquared the parabola, and found the proportion between the ſphere and the cylinder; or that Cavallerius, Fermat and Wallis had entrance into it, becauſe they did many more things of this kind.

P. S. When the committee of the Royal Society publiſhed the *Commercium Epiſtolicum*, the letters and papers in my cuſtody were not produced. Among them were the following letter of Mr. Leibnitz, dated $\frac{7}{17}$ of March 1693, and a letter of Dr. Wallis's, dated the 10th of April 1695; both which, upon a freſh occaſion two years ago, were produced, examined, and left in the archives of the Royal Society. The firſt ſhews what opinion Mr. Leibnitz had of this matter, before he knew my ſymbols, or any thing more of the method of Fluxions, than what he learnt from my letters and papers writ in or before the year 1676, or from the *Principia Philoſophiæ Mathematica*, and by conſequence before I could deceive him; and that he then gave me the precedence. The ſecond, compared with the preface to the Doctor's works, ſhews what opinion the Engliſh mathematicians, and ſome others abroad, had of this matter, when they heard that the Differential Method began to be celebrated in Holland, as invented by Mr. Leibnitz. The firſt of theſe two letters, and part of the ſecond, are hereunto ſubjoined.

'Illuſtri Viro ISAACO NEWTONO

'GODEFRIDUS GULIELMUS LEIBNITIUS, S. P. D.

'QUANTUM tibi ſcientiam rerum mathematicarum totiuſque naturæ debere arbitrer, occaſione datâ, etiam publicè ſum profeſſus. Mirificè ampliaveras geometriam tuis ſeriebus: ſed, edito Principiorum opere, oſtendiſti patere tibi, etiam quæ analyſi receptæ non ſubſunt. Conatus ſum ego quoque, notis commodis adhibitis, quæ differentias & ſummas exhibent, geometriam illam, quam tranſcendentem appello, analyſi quodammodo ſubjicere; nec res malè proceſſit. Sed à te adhuc magni aliquod expecto ad ſummam manum imponendam; tum ut problemata, quæ ex datâ tangentium proprietate quærunt lineas, reducantur optimè ad quadraturas; tum ut quadraturæ ipſæ, quod valde vellem, reducantur ad Curvarum rectificationes, ubique ſuperficierum aut corporum dimenſionibus ſimpliciores.

'Sed ſuper omnia optem, ut geometricis abſolutis, naturam, uti cœpiſti, mathematicè tractare pergas; in quo genere certè tu unus cum pauciſſimis ingens operæ pretium feciſti. Mirificum eſt, quod inveniſti, ellipſes Keplerianas prodire, ſi tantummodo attractio, ſive gravitatio, & trajectio in planetâ concipiantur. Tametſi enim eò inclinem, ut credam hæc omnia Fluidi ambientis motu ſive effici, ſive regi, analogiâ gravitatis

'&

‘ & magnetiſmi apud nos, nihil tamen ea res dignitati & veritati inventi
‘ tui detraxerit.

‘ Quæ ſummus & ipſe Mathematicus Chriſtianus Huygenius in tua no-
‘ tavit, appendice libelli de cauſâ luminis & gravitatis, expenſa tibi non
‘ dubito; & ſententiam viciſſim tuam velim. Veſtrâ enim amicâ collatione
‘ potiſſimum, qui in hoc genere eminetis, erui veritas poteſt.

‘ Cùm verò maximum tu quoque lumen ipſi Dioptricæ intuleris, expli-
‘ catis colorum phænomenis inexpectatis, velim quid ſentias de Huygeniaâ
‘ explicatione radiationis, utique ingenioſiſſimâ, cùm feliciter adeo prodeat
‘ lex ſinuum. Significavit mihi Huygenius, neſcio quæ nova phænomena
‘ colorum ſibi à te communicata. Ego valde optem ut ratio colorum, quos
‘ fixos vocant, ex apparentibus deduci poſſit; ſeu ut oſtendatur ratio effi-
‘ ciendi per refractiones, ut tota aliqua ſuperficies certum colorem oſtendat.

‘ In librorum apud Anglos editorum indicibus occurrêre mihi aliquoties li-
‘ bri mathematici autore Newtono; ſed dubitavi à te eſſent, quod vellem,
‘ an ab alio homonymo.

‘ Heinſius noſter redux teſtis fuit benevolentiæ erga me tuæ. De cultu
‘ verò meo erga te, non ille tantùm teſtari poteſt, ſed & Stepneius, te-
‘ cum ejuſdem olim Collegii habitator, nunc magnæ Britanniæ Regis ne-
‘ gotia apud Cæſarem, nuper apud Sereniſſimum Electorem Brandeburgi-
‘ cum, curans.

‘ Hæc ſcribo, magis ut ſtudia erga te mea intelligas, quæ nihil tot anno-
‘ rum ſilentio amiſere, quàm ut tua ego ſtudia, quibus auges humani ge-
‘ neris opes, interrumpere velim vacuis literis & ſupervacuis. Vale. Da-
‘ bam Hannoveræ $\frac{7}{17}$ Martii 1693.’

Part of a Letter of Dr. WALLIS'S to Mr. NEWTON, dated from Oxford, April 10, 1695.

‘ I WISH you would print the two large letters of June and October
‘ 1676. I had intimation from Holland, as deſired there by your friends,
‘ that ſomewhat of that kind were done; becauſe your notions of Fluxions
‘ paſs there with great applauſe by the name of Leibnitz's *Calculus Diffe-*
‘ *rentialis*. I had this intimation, when all but part of the preface to this
‘ volume was printed off; ſo that I could only inſert, while the preſs ſtay-
‘ ed, that ſhort intimation thereof, which you find there. You are not
‘ ſo kind to your reputation, and that of the nation, as you might be,
‘ when you let things of worth lie by you ſo long, till others carry away
‘ the reputation which is due to you. I have endeavoured to do you juſ-
‘ tice in that; and am now ſorry, that I did not print thoſe two letters
‘ *verbatim*.’

The intimation above-mentioned publiſhed in April 1695 by Dr. WALLIS in his ſaid Preface.

QUÆ in ſecundo volumine habentur, in præfatione eidem præfixâ dicitur. Ubi inter alia habetur Newtoni methodus de Fluxionibus, ut ille loquitur, conſimilis naturæ cum Leibnitii, ut hic loquitur, *Calculo Diffe-*

rentiali; quod qui utramque methodum contulerit, ſatis animadvertat, utut ſub loquendi formulis diverſis: quam ego deſcripſi, Algebra, Cap. XCI. &c. præſertim Cap. XCV. ex binis Newtoni literis, aut earum alteris, Junii 13, & Octob. 24, 1676, ad Oldenburgium datis, cum Leibnitio tum communicandis, iiſdem ferè verbis, ſaltem leviter mutatis, quæ in illis literis habentur; ubi methodum hanc Leibnitio exponit, tum ante decem annos, nedum plures, ab ipſo excogitatam. Quod moneo, nequis cauſetur, de hoc Calculo Differentiali nihil à nobis dictum eſſe.

Out of the account given of the works of Dr. WALLIS in the *Acta Eruditorum* for June 1696. Pag. 257, 258.

Cæterum ipſe Newtonus——ſummæ reciprocum. *Vide ſupra*, p. 568, lin. 16—29.

N. B. IN my letters of the 13th of June and the 24th of October 1676, I affirmed, that I had the method of Fluxions ſome years before; but I never allowed that Mr. Leibnitz had the Differential Method before the year 1677; nor in thoſe days did I know more of his pretences to it, than what he repreſented that year in his letter of the 21ſt of June; nor did I allow the Method of Tranſmutations to be a general method of Series; nor then knew, that the Series, which he ſent to me, was ſent to him by Mr. Oldenbourg the year before, and invented by Mr. James Gregory in the year 1671. The Method of Tranſmutations is not a Method of Series, but a particular theorem for tranſmutations of figures into one another, like thoſe of Gregory and Barrow. And as for the ſcholium upon the ſecond lemma of the ſecond book of the *Principia Philoſophiæ Mathematica*, which is ſo much wreſted againſt me; it was written, not to give away that lemma to Mr. Leibnitz, but on the contrary to aſſert it to myſelf. Whether Mr. Leibnitz invented it after me, or had it from me, is a queſtion of no conſequence; for ſecond inventors have no right.

Dr. Wallis, by his letter of Decemb. 1, 1696, printed in the third volume of his works, gave notice to Mr. Leibnitz of the paragraph in his preface to the firſt volume above cited. And Mr. Leibnitz denied not that in the year 1676, I explained to him the method found by me ten years before or above, nor complained of the Doctor for ſaying this: *De te autem queri*, ſaith he in his letter of the 29th of March 1697, *nunquam mihi in mentem venit; quem facilè apparet noſtra, in Actis Lipſienſibus prodita, non ſatis vidiſſe.* He allowed, that the methods were of like nature as the Doctor had affirmed; and ſaid, in his letter of the 28th of May 1697, that he therefore called them both by the common name of the *Infiniteſimal Method*; but added, that as the Analyſis of Vieta and Cartes were both called by the common name of *Analyſis Specioſa*, and yet differed in ſome things, ſo my *Infiniteſimal Analyſis* and his might differ in ſome things; meaning ſome improvements made by him to this method; and excuſed the Doctor for not mentioning theſe improvements, becauſe he had not ſufficiently ſeen them in the *Acta Lipſienſia*; and declared that it never came into his mind to complain of him for any thing elſe.

And

And yet although Cartes never laid claim to the *Analyſis Specioſa* of Vieta, Mr. Leibnitz continued amongſt his friends to call the Infiniteſimal Method his own, and thereby gave occaſion to Mr. Fatio to write what follows:

Out of the tract of Mr. NICHOLAS FATIO DE DUILLIER, intituled, Inveſtigatio Geometrica Solidi rotundi, in quod minima fiat reſiſtentia, publiſhed in the year 1699.

'QUÆRET forſan Cl. Leibnitius, unde mihi cognitus ſit iſte calculus, quo utor. Ejus equidem fundamenta, ac pleraſque regulas, proprio Marte, anno 1687, circa menſem Aprilem & ſequentes, aliiſque deinceps annis, inveni; quo tempore neminem eo calculi genere præter meipſum, uti putabam. Nec mihi minùs cognitus foret, ſi nondum natus eſſet Leibnitius. Aliis igitur glorietur diſcipulis, me certè non poteſt. Quod ſatis patebit, ſi olim literæ, quæ inter clariſſimum Hugenium meque interceſſerunt, publici juris fiant. Newtonum tamen primum ac pluribus annis vetuſtiſſimum hujus calculi inventorem, ipſâ rerum evidentiâ coactus agnoſco: à quo utrum quicquam mutuatus ſit Leibnitius, ſecundus ejus inventor, malo eorum quàm meum ſit judicium, quibus viſæ fuerint Newtoni literæ, aliique ejuſdem manuſcripti codices. Neque modeſtioris Newtoni ſilentium, aut prona Leibnitii ſedulitas inventionem hujus calculi ſibi paſſim tribuentis, ullis imponet, qui ea pertractârint, quæ ipſe evolvi, inſtrumenta.'

N. B. Mr. Fatio wrote this as a witneſs. He related what he had ſeen; and his teſtimony is the ſtronger, becauſe it was againſt himſelf, and he was no Engliſhman. He underſtood the methods of us all, and by what he had ſeen and underſtood, he was able to make a true judgment.

FINIS TOMI QUARTI.

G.R. III.

CORRIGENDA IN CONTEXTU.

Pag. 270, lin. penult. pro *adhæreus*, lege *adhærens*.
P. 271, lin. 5, pro *interior*, lege *anterior*.
P. 279, lin. 7, for *CBbc* and *CBbc*, read *Cbac*.
P. — lin. 16, for *AC*, read *Ac*.
P. 280, lin. 23, for *F*, *G*, read *FG*.
P. 507, lin. 23, pro $170\frac{0}{1}$, lege $167\frac{0}{1}$.
P. 530, Ad initium lineæ 23 in orâ libri ſcribatur *N° L*.
P. 544, Ad initium lineæ primæ in orâ libri ſcribatur *N° LVII*.

IN NOTIS.

P. 264, lin. 4, pro *Nonchidarum*, lege *Noachidarum*.

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